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OF THE

Royal Institution of Great Britain

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 27, 1911.

Sir James Crichton Browne, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Professor William H. Bragg, M.A. F.R.S.

Radioactivity as a Kinetic Theory of a Fourth State of Matter.

There are many points of resemblance between the movements of the molecules of a gas and the movements of those corpuscular radiations with which we have become acquainted in following up the discovery of radioactivity. In both cases we find that things of extremely minute dimensions are darting to and fro with great velocity; and in both cases the path of any one individual is made up of straight portions of various lengths, during which it is moving uniformly and free from external influence, and of encounters of short duration with other individuals, during which energy is exchanged and directions of motion are altered. There is even a resemblance in the universality of each movement. The motion of molecules is a fundamental fact throughout the whole of our atmosphere, and indeed in all material bodies; the motion of the radiant particles emitted by radioactive substances is also widely distributed, and of great importance. Taking Eve's estimate of the usual ionisation of the air we can calculate that in this room, in every second, some thousands of α and β particles enter into existence, complete their paths through all the atoms they meet, and sink into obscurity; some of them, viz. the α particles, as atoms of helium. Of these last some move through a range in air of just over 4 cm., an equal number have a range of just under 5 cm., and again an equal number move through 7 cm.; and the speed is so great that the life of each α particle as such is completed in about a thousand-millionth of a second. They leave their mark behind them in the ionisation of the air through which they have passed, and in the heat into which their energy has been commuted. The former effect is easily detected by the sensitive measuring instruments which we now possess; the latter is too small to measure, and must be greatly increased by the aid of radium itself before it can be investigated. But on a large scale which takes into account the distribution of radioactive material through the earth, the sea and the air, the effects are of first rate importance to the physical conditions of our earth.

If we compare the movements a little more closely, we find differences as interesting as the resemblances. The motions which the kinetic theory of gases considers are those of the molecules of which

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gases consist: in the case of radioactivity, the things which move are quite different. They are sometimes electrons, which have come to be called β rays when their speed is great, and cathode rays when it is somewhat less; or they are γ, or x rays, which are new things to us; or if as α particles they are helium atoms, such as we have known before, they move with excessive speeds which give them quite new properties. In general the radiant particles move hundreds of thousands of times as fast as the gas molecules do, and it is, no doubt, on account of this fact, as well as through their usually extreme minuteness, that their power of penetrating matter is so great. When two molecules of a gas collide, they approach within a fairly definite distance, which we call the sum of the radii of the molecules: and the approach is followed by a recession and new conditions of motion. Each molecule has, as it were, a domain into which no other molecule can penetrate. But the defences which guard the domain are of no account to the vigorous movements which we are considering now. The radiant particles pass freely through the atoms, and their encounters are rather with one or other of a number of circumscribed and powerful centres of force which exist within the atomic domain, and act with great power when and only when approached within distances which are small in comparison with the atomic radius. It is on this account that the new theory opens out to us such possibilities of discovering the arrangement of the interior of the atom. Never before have we been able to pass anything *through* an atom: our spies have always been turned back from the frontier. Now we can at pleasure cause to pass through any atom an α particle, which is an atom of helium, or a β particle, which is an electron. or a γ, or x ray, and see what has happened to the particle when it emerges again: and from the treatment which it seems to have received we must try to find out what it met with inside.

The newer movement exists superimposed upon the other. Its velocities are so great that the gas (or liquid or solid) molecules are, in comparison, perfectly still. There is, as it were, a kinetic theory within a kinetic theory: there is a grosser movement of gas molecules which has long been studied, and in the same place and at the same time there is a far subtler and far more lively movement which is practically independent of the other. Your Vice-President, Sir William Crookes, was the first to find any trace of it. The behaviour of the cathode rays in the vacuum tubes which he had made showed him that he was dealing with things in no ordinary condition. Whatever was in motion was neither gas, nor solid, nor liquid, as ordinarily known, and he supposed it must be possible for matter to exist in a fourth state. We have gone far since Sir William's first experiments. The x ray tube and radium have widely increased our knowledge of phenomena parallel to those of the Crookes tube. But I think we may still be glad to use Sir William's definition.

There is another very striking characteristic of the newer kinetic theory which differentiates it sharply from the older. The experiences of any one of the radiant particles in an atom which it crosses are quite unaffected by any chemical combination of that atom with others; that is to say, by any molecular associations it may have. Naturally, this simplifies investigation. We may no doubt ascribe this state of things to the fact that a radiant particle is concerned rather with the interior of the atom than with the exterior, and that it is the latter which is of importance in chemical action.

Let us take notice of one more important difference. The molecules of a gas move with velocities which vary at every collision, yet vary about a certain mean. But the peculiar motion of the radiant particle is only temporary. For only a very short time can any ray be described as matter in a fourth state; at the end of it the extraordinary condition has terminated, the particle has lost its tremendous speed or suffered some other change, and the ray ceases to exist. Speaking technically, we are dealing with initial not permanent conditions.

Let us now come back to resemblances between the two kinds of motion; for there is one point of similarity which is not quite so obvious as others I have mentioned, and is, I think, of the greatest importance; in fact, it is largely on account of this similarity that I have ventured to put the two theories together for comparison.

When the first experimenters in radioactivity allowed their streams of rays to fall upon materials of various kinds, they found that the irradiated surfaces were the sources of fresh streams of radiation. The secondary rays were sometimes of the same nature and quality as the primary, sometimes not. Further, they found that the secondaries, on striking material substances, could produce tertiaries, and so on. The examination of all the variations of this problem—the investigation of the consequences of changing the primary, of changing the substance, and last, but not least, of changing the form of the experimental arrangements—has been the cause of an enormous amount of work. There is a large literature dealing with secondary radiations of all kinds, which, I imagine, but few have read with any completeness, and the subject has become, on the surface at least, complicated and difficult. Now I believe that it is possible to clear away the greater portion of this complexity at a stroke, by the adoption of an idea which makes it possible to describe and discuss the whole of these phenomena in a very simple way. When an encounter takes place between two gas molecules, we suppose that the sum of the energies of the two is the same after the collision as before, and, further, that there are just two things to consider—two molecules—after as well as before. I think that we may carry this idea over almost bodily to the newer theory. A radiant particle encounters an atom. The particle is a definite thing, it contains a definite amount of energy, and whether it is an α or β or γ or x ray, its energy is to be found almost entirely inside a very minute volume. The encounter takes place. When it is over there are still two things, an atom and a radiant particle going away from it. The sum of the energies of the two is still the same, which means that we deny a possibility much considered at one time—viz. that in the encounter the atom could be made radioactive, and could unlock a store of energy usually unavailable. We suppose there is no energy to be considered except the original energy of the radiant particle, and we suppose that there are not now two or more radiant particles in place of the original one, which also is a limitation on previous ideas. It is a theory which ascribes a corpuscular form to all the radiations. Each particle, α, β, γ or x, is to be followed from its origin to its disappearance, and we have nothing to think of but the one particle threading its way through the atoms. It loses energy as it goes, though little at any one collision, and it passes out of our reckoning when it has lost it all. There are no secondary radiations other than radiant particles moving in directions which are different from those in which they moved at first. Even when a cathode ray excites an x ray in the ordinary Röntgen tube, or the x ray excites a cathode ray in a manner almost as well known, it is hardly an exception to this rule. The cathode ray has an encounter with an atom and disappears; simultaneously the x ray comes out of the atom, a circumscribed corpuscle carrying on the energy of the cathode ray. There is a change, but it extends only to the external characteristics of the carrier of energy. The x ray passes through the glass wall of the x ray bulb, or at least it does so sometimes; it may pass through other matter as well, but sooner or later it has a fatal encounter with an atom, and the reverse change takes place. In all cases, in that of the undeviating α ray, or the β ray which suffers so many deflections, or the γ or x rays, it is a matter of tracing the movements of individual minute quantities of energy until they finally melt away.

Let us consider one or two simple experimental results from this point of view in order that we may illustrate this corpuscular theory, and at the same time may learn something of the properties of the corpuscles and of the arrangements of the atoms through which they pass.

We take first one of the simpler cases, the movement of an α particle through a gas. The relatively large mass of the particle gives it an effectiveness which the other radiations do not possess. It moves straight through every atom it meets, and ionises most of them. Very rarely does it suffer any deflection from its course until its velocity is nearly run down. Then indeed it does appear to depart considerably from the straight path, and it may be that it is much knocked about by collisions before it finally comes to comparative rest. In this way we may explain the distribution of the ionisation along its path, which increases slowly at first and rapidly afterwards, until the α particle has nearly finished its journey: it then falls off rapidly. Considering that the ionisation increases as the particle slows down and spends more time in each atom, and considering the more broken nature of the path near its end, the reason of these peculiarities is clear enough. Apart from its comparative simplicity, there are some other very interesting features of the particle's motion. It is found, for example, that the loss of energy which the particle incurs in crossing an atom is proportional to the square root of the atomic weight very nearly, and there is no certain explanation as yet of this curious law. And again, Geiger has examined the small scattering that does occur, and found that α particles when moving quickly may be swung round completely even by the thinnest films of gold leaf, though the number is so small that the effect would have remained undetected had it not been for the scintillation method which he and Rutherford have perfected. He has found that about one particle in 8000 is returned in this way from a gold plate, which need consist only of a few thicknesses of gold leaf in order to give the maximum effect.

Now let us take an example from the behaviour of the β rays. The β particle is so light that it is easily deflected, even though it moves several times as fast as the heavier α particle. Because it therefore possesses little energy its effects are much smaller, and no one has yet succeeded in handling a single β particle in the same way as Rutherford and Geiger have handled the other. We are obliged to content ourselves with observations of the effects of a crowd of β particles, since the combined action of many is necessary to give us an observable result. And at the same time that the β particle gives much less effect than the α, it has a much more irregular course, so that the problem is doubly difficult. We are, in fact, only just beginning to understand it. There is a compensation in the fact that its very liability to deflection makes it all the more interesting an object. It is possible—and this is the particular β-ray problem I wish to consider now—to examine the deflection of a single β particle by a single atom: the parallel result in the kinetic theory of gases has never, of course, been achieved.

Suppose that we project a stream of β rays against a thin plate and measure the relative number sent back, which we do by measuring the ionisations caused by the incident and returned rays respectively. We do this for varying thicknesses of the plate, and plot the results, as, for example, Madsen has done. His plate was made of gold leaves, which could be had of extreme fineness. From the relation thus obtained, it is possible to obtain with confidence the amount of β radiation that would be returned by the thinnest plate that could be imagined, only one molecule thick. In such case the particles turned back could have had but one collision, and we have achieved our purpose. Madsen's figures show that a plate weighing 4 mmg. to the square centimetre turned back a tenth of the molecules that fell upon it, and as far as can be judged the ratio of the proportion turned back to the weight of the plate would be almost doubled for very thin plates. We might go more into detail, and find the distribution of those that are returned ; we should then have data from which we might determine in some measure the distribution of the centres of force inside the atom. We cannot follow this up now, but I would like to draw your attention to a curious indication which we obtain when we compare the results for gold with those which Madsen found for aluminium. They show that the lighter metal turns back fewer *β* particles, and that its power of absorbing a stream of rays is rather an absolute abstraction of energy. There is clearly an actual absorption effect, which is to be distinguished from the scattering effect. Indeed, the two effects are obviously of different importance in the two cases. When a *β* ray strikes a gold atom it must be much more liable to deflection than when it strikes the lighter atom of aluminium. On the other hand, I think it can be shown clearly that in ploughing through aluminium atoms there is a relatively quicker absorption of energy. We may illustrate this by a rough model. Let us stand an electromagnet upright on the table, and let us suspend another magnet so that it can swing over the fixed one and just clear it. If we draw back the swinging magnet and let it go towards the fixed one, the currents running so that the two repel, then as the moving magnet tries to go by there will be a deflection depending on the relative speed, the closeness of approach, and the strength of the poles. This may represent the turning aside of an electron by a centre of force inside an atom. Now let the magnet at the table be supported by a spiral spring so as to be still upright, but have some freedom of motion ; then, when the experiment is repeated, the swinging magnet pushes the other more or less to one side ; it is less deflected, but it has to give up some of its energy. This is exactly what happens in the case of the *β* particle. The centre of force in the gold atom behaves like the stiffer electromagnet on the table ; it deflects the electron more, but robs it of less energy in doing so. It will not do to suppose the gold atom to differ from the aluminium atom simply in the number of centres of force, such as electrons, which it contains, if it is supposed that they all act independently. There is some other fundamental difference, equivalent to a difference in the stiffness with which the electrons are set in their places. There are two things to be expressed in the behaviour of the atom towards the *β* particle, as has been pointed out several times. H. W. Schmidt has actually calculated them from experiments which gave them indirectly and somewhat approximately. The method I have just outlined gives one of them directly, viz. that which is called the scattering coefficient; and I think the other can also be found directly by a method which will serve as an illustration of the behaviour of *γ* rays.

We must first, however, consider the part which *γ* and *x* rays play generally in this theory. Workers are by no means agreed as to the proper way in which to regard them, but there is no need to enter at once on a discussion as to their nature. It is well known that they have the most extraordinary powers of penetration, and are unaffected by electric or magnetic fields. They have one property which alone, as I think, brings them within our experience; that is to say, the power of exciting β rays from the atoms over which they pass. Were it not for this, they would still be unknown. When we examine this production of β rays, we find that in the first place their speed depends on the quality of the γ rays which cause them, and not on the nature of the atoms in which they arise; in the second, that the β rays to a large degree continue the line of motion of the γ rays as if the latter pushed them out of the atoms; and, lastly, that the number of the β rays depends on the intensity of the γ rays. It is these facts which suggest the simple theory I have already described. The γ ray is some minute thing which moves along in a straight line without change of form or nature, which penetrates atoms with far greater ease than the α or β particle, which is not electrified, and which sooner or later disappears inside an atom, handing on a large share of its energy to a β particle which takes its place. The absorption of γ rays is simply the measure of their disappearance in giving rise to β rays, one γ ray producing one β ray, and no more.

We find the same sort of scattering in the case of γ rays as in that of β rays. Of a stream of rays directed against a plate which it can penetrate easily, we find that a few are turned completely back, a very much larger number are only slightly turned out of their path, and the rest go on. The scattered rays are very similar to the original rays: there is no need to suppose that the original ray disappears to be replaced by a secondary, any more than there is to suppose that α and β rays disappear and are replaced by others in similar cases. When therefore a γ ray enters an atom three possibilities await it. The first is a negative one, it may go through the atom untouched, and this must happen in the majority of cases; the second chance is that of deflection, and the third that of conversion into a β ray, using the word conversion in a general sense without going into details as to the nature of the process.

Now we may consider our γ ray problem. Suppose a stream of these rays passing over a block of any substance such as aluminium or zinc or lead. When they are really penetrating rays they are equally absorbed by equal weights of these materials, which means that in equal weights equal numbers of β rays spring into existence. If these β rays were able to move through equal weights of the metals, we should find in each metal the same "density" of β rays; and the important point is that this is independent of whether the rays are straight or crooked in their paths. If ten lines of given length were begun in every square centimetre of a sheet of paper, the ink used in drawing them would be independent of the straightness of the lines but proportional to their length. Now if we make a cavity in each metal the β rays will cross it in their movements to and fro; and if a little air is introduced into the cavity the ionisation produced in it will be a measure of the density of the β rays, and therefore the average distance each moves in the metal. Experiment shows that we get twice as much ionisation in a cavity in the lead as in a similar cavity in the aluminium, and we conclude that the β particle really has a longer track in the heavier metal. This experiment gives us the second constant of β ray absorption—that is to say, the rate at which its energy is taken away from it: the other experiment gave the chance of deflection only. We see that the path of a β ray in aluminium is more direct, but of less length, than in lead; in the latter metal it has really a longer path, but it does not get so far away from its starting point because it suffers so many more deflections.

Finally, let us take a problem from the x rays. Let us see how we may test the idea that x and γ rays do not ionise themselves, but leave all the work to be done by the β rays which they produce. Suppose a pencil of x rays to pass across a vessel and to produce ionisation therein. It is convenient to use, not the original x rays, which are heterogeneous, but the rays which are scattered by a tin-plate on which the primary rays fall. Such "tin-rays," as we often call them briefly, are fairly homogeneous and give cathode-rays of convenient penetration. In some experiments of mine the rays crossed a layer of oxygen 3·45 cm. wide, having a density ·00137, and the ionisation produced was 227 on an arbitrary scale. The result may be put in the following way. Suppose, provisionally, that all this ionisation is done indirectly; the oxygen has converted so much x-ray energy into cathode-ray energy, and these cathode-rays penetrating their one or two millimetres of oxygen, which is all they can do, have ionised the gas. Then we may say that in crossing a layer of oxygen weighing 3·45 × ·00137, or ·00473 gr. per sq. cm., enough cathode-rays have been produced to cause an ionisation of 227 units: and therefore that a layer weighing one milligram per sq. cm. would produce 48 units in the same way. We now proceed to compare this production in oxygen with the similar effect in a metal such as silver. Stretching a silver foil across the chamber in the path of the rays we find that under the same intensity of rays the ionisation is largely increased, and the change is due to cathode-rays which the x-rays have generated in the silver. Not all these rays get out of the silver, but we can overcome this difficulty by taking silver foils of different thickness, drawing a curve connecting the effect of the foils with their thicknesses, taking the curve back to the origin, and so finding what would be the effect of a foil so thin that all the cathode-rays did get out. In my case I found that a milligram of silver produced enough cathode-rays to give an ionisation 1580. This is 33 times as much as the oxygen could do. Now according to our theory this should be because silver absorbs tin-rays 33 times more than oxygen does;

and experiment showed this to be very nearly the case. In finding the absorbing power of oxygen I measured first those of carbon and oxalic acid, and then proceeded by calculation; for the absorption in a gas is difficult to determine.

Two interesting points appeared in this experiment. In the first place the ratio between the two quantities of cathode rays, which appear on the two sides of a silver leaf through which the "tin rays" pass, is nearly constant for different thicknesses of leaf, and with the thinnest leaf obtainable each quantity was about half its full value. It would have been desirable to have had still thinner leaves; but it is fairly clear that the ratio would be nearly the same for extreme thinness. The cathode radiation, which appears on the side of the leaf whence the x rays emerge, is 1·30 times that which appears on the other, and we may take it that this would be the case even if the leaf were but one atom thick. Thus when an x ray plunges into an atom in which its energy is converted into that of a cathode ray, the cathode ray may emerge at any point, but there is a 30 per cent. greater chance that it will more or less continue the line of motion of the x ray than that it will not. In previous work on the conversion of γ ray into β ray energy, I have found that the β ray may practically be supposed to continue the line of motion of the γ ray, so that there is a great difference in behaviour of the two classes of ray in this respect. It is remarkable that the scattering of the γ rays shows also a much greater dissymmetry than is found in the case of the x rays. It looks as if the β rays that appear when γ or x rays impinge on atoms are related rather to the scattered than to the unscattered primary rays. Putting it somewhat crudely, no doubt, it might be said that when a γ or x ray is deflected in passing through an atom, it runs a risk of being converted into a β ray in the process, so that β rays are found distributed about the atom in rough proportions to the secondary γ or x rays. In the case of γ rays this practically amounts to their all going straight on at first: in the case of x rays the distribution is more uniform.

Another interesting point arises in this way. When the x rays from tin are allowed to pass into the ionisation chamber through increasing thicknesses of silver foil, the cathode rays grow at a rate which is not represented by the exponential curve usually assumed. The amount is for some time more nearly proportioned to the thickness of the foil. A second foil adds its own effect without destroying much of the one on which it is laid. This may easily be ascribed to the relation of the ionisation due to the β particle to the energy it has to spend. The ionisation is nearly all at the end of the path, and the second layer does not absorb the rays made in the first because they are still at the beginning of their career.

These few experiments which I have described may serve to illustrate both the justice and the convenience of placing all these rays, α, β, γ, and x, in one class. We are tempted to consider them all as corpuscular radiations of some sort, and we then look upon our researches into their behaviour as attempts to understand the collisions of the various new corpuscles with the constituent centres of force in the atoms. But if we ascribe corpuscular properties to the γ and x rays, we are led far away from the original speculations as to their nature. Stokes supposed them to be spreading æther pulses, but in his theory the energy of the pulse spreads on ever-widening surfaces as the time passes, and is utterly insufficient to provide the energy of the β rays which the γ or x rays excite. Some sort of mechanism has to be devised by which the energy of the γ ray moves on without spreading, so that at the fateful moment it may be all handed over to the β ray, which carries it on. I had the hardihood myself to propose a theory of this kind. My idea was that the γ or x ray might be considered as an electron which had assumed a cloak of darkness in the form of sufficient positive electricity to neutralize its charge. Nor do I see any reason for abandoning this idea, for it is at least a good working hypothesis. It means, of course, that not only does the energy of the β ray come from the γ ray, but the β ray itself.

Many insist that my neutral corpuscle is too material, and that something more ethereal is wanted. For it appears that ultra-violet light possesses many of the properties of x and γ rays. It can excite electrons to motion, and sometimes the speed of the electron depends on the quality of the light and not on the nature of the material from which it springs. They propose, therefore, a quasi-corpuscular theory of light, γ and x rays being included. The immediate objection to this proposal is that it seems to throw away at once all the marvellous explanations of interference and diffraction which Young and Fresnel founded on a theory of spreading waves; and I do not think anyone has yet made good this defect. The light corpuscle which is proposed is a perfectly new postulate. It is to move with the velocity of light, keeping a circumscribed and invariable form, to have energy and momentum, and to be capable of replacing and being replaced by an electron which possesses the same energy but moves at a slower rate; and, of course, it has to do all that the old light-waves did. The whole situation is most remarkable and puzzling. We are working and waiting for some solution which, perhaps, will come in a moment unexpectedly. Meanwhile, we must just try to verify and extend our facts, and be content to piece together parts of the puzzle, since we cannot, as yet, manage the whole. My object to-night has been to show you how we may conveniently bind together a large number of the phenomena of radioactivity into an easily-grasped bundle, using a kinetic theory which has many points of resemblance to the older kinetic theory of gases.

[W. H. B.]

WEEKLY EVENING MEETING,

Friday, February 3, 1911.

SIR WILLIAM CROOKES, O.M. LL.D. D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

ARTHUR E. SHIPLEY, ESQ., M.A. Sc.D. F.R.S., Master of Christ's College, Cambridge, Reader of Zoology in the University.

Grouse Disease.

"THE longer I live, the more I am convinced that the apothecary is of more importance than Seneca; and that half the unhappiness in the world proceeds from little stoppages, from a duct choked up, from food pressing in the wrong place, from a vext duodenum, or an agitated pylorus."

Thus that incorrigible amateur-physician, Sydney Smith wrote of our poor suffering humanity, and thus we can as truly write of the grouse. Little stoppages, food pressing in the wrong place, a vext duodenum, and an agitated blind-gut, and there you have "Grouse disease"!

At the onset I must, however, protest against that fallacious and all-embracing expression. It will be difficult to get rid of, for the average keeper and sportsman is seldom clinically inclined, and if he see his birds diseased or dead or dying, and they are grouse, he is content to put it all down to "grouse disease" and to leave it at that. But grouse suffer and die from many diseases. In a few dozen birds examined chiefly in Cambridge, the following disorders were seen:—Pleuro-pneumonia in a bird which had lived long in captivity; pericarditis; necrotic changes in the liver; chronic diseases of the peritoneum; and a septic infection due to gangrene supervening upon a broken wing.

Sick and dying animals are apt to creep away into corners and hide themselves; thus it comes about that when these animals die patently and in large numbers, the public is apt to regard this mortality as due to some disorder peculiar to the animal in question, and the disease receives the name of the species which is afflicted. Hence we hear of such illnesses as "horse-sickness," "silk-worm disease" and "grouse disease."

The disorder which is usually associated with the too comprehensive expression "grouse disease" was investigated by Klein some eighteen years ago, and in this lecture it will be called Klein's grouse disease. Klein found in the tissues of the bodies of birds that had been dead for some time a certain bacterium, whose nature and life-history he investigated. This bacterium is now recognised as one of the *Bacillus coli* group, a widely spread group of bacteria which are found universally in the alimentary tracts and which rapidly invade the tissues of the body after death. At the time Klein was working bacteriology was comparatively a new subject, and this invasion by bacteria of the tissues at the time of and after death was not appreciated.

Klein's grouse disease was associated also with some congestion of the lungs; the windpipe was described as dark in colour, the air-sacs contained blood, in the cavity of the lungs blood or some granular exudation occurred, the liver and kidneys were congested and soft, and there was an exudation on the peritoneum. We now, however, know that many, if not all, of these appearances in the chief organs of the body are but normal post-mortem changes and occur sooner or later after death in birds which were perfectly healthy when killed. Another feature attributed to the Klein's grouse disease was that its onset was comparatively sudden, its course rapid, and according to all observers it attacked healthy and plump birds. The present Inquiry has not yet succeeded in coming across any sick or dead birds which are plump or in good condition. All the grouse, and they amount in number to nearly two thousand, which have been investigated have been weighed, and in every case where there has been any disease there has invariably been wasting; the sick birds are always thin, have lost flesh, and are in a poor condition. One final feature of Klein's grouse disease is its seasonal incidence; usually it is said to occur with greatest virulence in the spring, to die down during the summer, and to recur in a less virulent form in the autumn. To this seasonal variation I shall return.

Klein's grouse disease is still a matter of inquiry. During the last five years, whilst the Commission has been prosecuting its inquiries, this "disease" has frequently been reported, but on investigation the characteristics enumerated above have not been present; still, the sportsman and the gamekeeper, who do not weigh their grouse and seldom their evidence, and who are but imperfectly acquainted with post-mortem changes, firmly believe in the existence of this "disease," and it may be that it really exists, and that it is the misfortune of the Inquiry that in their researches the investigators have not come across it.

If we now turn from what must seem rather negative criticism to the more positive results attained by the investigation of the last four years, we may begin by pointing out some of the difficulties which confronted the Inquiry.

In considering exceptions it is so immensely important to know the rule. In studying disease our starting-point should be the normal, the healthy; yet until lately no one has studied the healthy grouse, and indeed it is almost impossible to find a normal grouse, *i.e.* one free from parasites. A grouse cannot express to us its feelings; the state of its tongue, the rate of its pulse, even its temperature tell us nothing, because we have no norm and no means of estimating the extent to which a diseased bird has departed from the standards of a healthy grouse. The nature of the numerous kinds of blood corpuscles, which alter in proportion so markedly in animals when they become parasitized, was but a few months ago quite unknown, the "blood count" uninvestigated; in fact, the Inquiry started, as regards the cause and symptoms of the diseases which affect grouse, practically at "scratch." It was, of course, known that the suffering birds lose their activity and are more easily caught than healthy grouse; their flight is slow and limited in length; they are said to seek water; the "call" becomes feeble and hoarse: the feathers of the back and throat lose their lustre and become ruffled; the eye is dimmed. But these external symptoms may be associated with several diseases and diagnostic of none. Nearly all of them occur in the two diseases Coccidiosis and Strongylosis, which, according to the Inquiry, are responsible for a very large percentage of deaths among grouse.

Each of these diseases is caused by an animal parasite, and the investigation of the parasites attracted the attention of the scientific advisers of the Inquiry from an early date. I am afraid that in describing these organisms I shall have to use some rather long words; in extenuation I can only say, slightly altering Captain Kedgick's retort to Martin Chuzzlewit, "Well! I didn't fix the zoological language, and I can't unfix it, else I'd make it pleasant."

Five years ago we knew two internal parasites of the grouse (endoparasites) and two or three parasites which live outside the skin (ectoparasites). At the present time we know that grouse, like other animals, have a considerable fauna living both in and on them. They are in fact not only birds, but in a small way aviating Zoological Gardens. The scientific members of the Inquiry have recorded eight different species of insect or mite living either amongst the feathers or on the skin of the bird or in other ways associated closely with the grouse, and no fewer than fifteen animal parasites living in the blood, the alimentary canal, the lungs, or other organs. Some of these are negligible. They either exist in too small numbers or infest but a very small percentage of the birds; others, however, are found in about 95 per cent. of the cases investigated, and two at least are associated with grave disorders which often terminate in death.

The interest of the insects and mites which live on the skin of the bird is that these very likely form the second host of the tapeworms, which undoubtedly do a certain amount of harm to the lining of the alimentary canal. There are, for instance, a couple of species of bird-lice, lively little creatures, which take cover amongst the small feathers—which, by the way, form their arid diet—like startled deer in the undergrowth of a forest. Few grouse are free from these bird-lice, perhaps hardly 10 per cent., and the number on each bird is to some extent a measure of its ill-health. On a healthy grouse perhaps but two or three are found. They are animals with stout and powerful jaws, which they use to bite off the barbules of the feather or the finer plumules which form their sole nutriment. What fluid they obtain to moisten this somewhat dry nutriment is not apparent, but the animals are active and by no means so easy to catch as one at first thinks. They lay very beautiful eggs attached in small groups to the base of the after-plume of the feather or between it and the main shaft. The young hatch out as miniatures of the parents and there is no metamorphosis. The same species occurs on the Black Grouse and upon the Willow or Hazel Grouse. On a piner these bird-lice increase enormously in number, and their numbers to some extent serve as a measure of the gravity of the disease. Both of these bird-lice cast their skin several times; the exact number of times is, however, not known, but cast skins are frequently met with. The young birds are probably infected with these ectoparasites whilst in the nests, the bird-lice falling from one bird to another when they are contiguous. They have also been known to cling to the grouse-fly and in this manner may be transported to a new host. In no case was any specimen of either of these two species found in the crop of the grouse.

Two fleas are found on grouse, one rare, but the other is a well-known bird-flea which has also been found in the nest of the hawfinch, the dipper, the blackbird. the moorhen and others. Since it is known that a certain dog-flea is the second host of one of the cestodes of the the dog, and a rat-flea of a tape-worm of the rat, it seems possible that one of these fleas may be the intermediate host of one of the chief worms which infest the alimentary canal of the grouse. We have, however, not succeeded in finding the cysts, neither have we found specimens of the flea in the crop of the bird.

There is a tick, the common rice- or dog-tick, usually attached below the jaw of the bird or to the eyelid or to some other position where the beak cannot reach it. Ticks are responsible for the transference of a very fatal epizootic termed *Spirillosis* in fowls in the Sudan, and for numerous other diseases which afflict man and cattle throughout the world, but ticks are not common on the grouse, and the Inquiry has as yet traced no disease to them. In parts of Ross-shire, however, especially in certain woods, these ticks are said to be extremely numerous, and the keepers aver that they frequently kill off large numbers of black-game. They are commoner during the spring and early summer, but usually disappear at the beginning of July. Curiously enough a common cheese- or flour-mite was from time to time found in considerable numbers on the skin of the grouse, and apparently these mites sucked the blood of their host, for their alimentary canal contained red food.

Finally, there are a couple of true flies, the well-known grouse-fly, which is apt to crawl up the sleeves of those who handle grouse in the early autumn. The grouse-fly belongs to the same group as the horse-fly and the sheep-tick. The latter, however, has lost its wings and burrows in the wool of the fleece. Most members of the family to which this grouse-fly belongs live upon birds: it particularly frequents swallows and other allied species. Recently Dr. Sharp has pointed out that the grouse-fly (*Onithomyia lagopodis*) is distinct from the ordinary bird-fly (*O. avicularia*). The habits of the grouse-fly are difficult to investigate. It is believed to suck the blood of the grouse, and very probably inoculates the bird with some of the protozoa which infests its blood. The adult or imago burrows amongst the feathers of the bird, and any one handling grouse during the late summer is apt to disturb a fly or two. Their feet, although large, are very beautiful. Each is provided with a pair of most powerful hooks. Altogether, these insects have a sinister aspect and they are very repellent to people who do not like flies. The grouse-fly occurs very frequently in larders where freshly killed grouse have been placed, and after a short time they readily leave their dead hosts and accumulate on the windows. Like the fatal tseste-fly of Africa, which conveys sleeping sickness, they lay no eggs, but produce one larva or maggot at a time, and this immediately turns into a pupa. The pupæ of the grouse-fly, usually found in the nest of the grouse during August and September, are black, shiny, seed-like-looking objects. Probably each pupa takes some three-quarters of a year to develop into the adult fly, and the latter disappears from October until June. There is thus a certain tragedy in the life of these insects. No parent ever sees its offspring, no offspring has ever known parental care. We have never found one of these flies in the crop of the grouse, nor have we succeeded in finding cysts in the bodies of flies which were broken up, or teased up, or cut into sections.

Finally there is another fly whose larva lives in grouse droppings. All these creatures have been carefully searched for the larva of the grouse tape-worms, but so far with no definite success.

Of the fifteen endoparasites but two or three demand attention; the others are comparatively rare or innocuous, and some, such as the gape—or forked—worm so fatal to pheasants, are not normally parasites of the grouse. Occasionally by some accident they get into the wrong Paradise.

Some of these endoparasites, which live inside the body of the grouse, are responsible for the illnesses from which grouse suffer. Any attempt to control their number and their activity must depend on our knowledge of their life-history, hence the stress which has been laid on the external parasites which may function as the second or larval host of some of them.

At the time the present Inquiry commenced to inquire there were but two worms described as being in the alimentary canal of the grouse—the large tape-worm which lives in the small intestine all the year round, known to every sportsman, and a slender thread-worm which inhabits the paired cæca or blind-guts, which are unusually large in the grouse and play a very important part in its digestion. The latter worm under certain conditions, and when present in considerable numbers, is associated with one of the two diseases which have especially attracted the attention of the Inquiry.

Davainea urogalli (Modeer).—Of the three tape-worms that are found in the grouse, this species is by far the largest and by far the most common. It exhibits little seasonal variation and is found in considerable numbers all the year round. The birds become infected at an early age.

D. urogalli is normally found in the small intestine, though sometimes parts of it are found in other portions of the alimentary canal. As a rule, three or four individuals are met with. At other times, especially in weakly birds, there are dozens, and these fill up the lumen of the intestine to such an extent that it is difficult to see how food can pass along.

D. urogalli, like most cestodes, produces a very large number of eggs at any one time. It may be, at a rough estimate, at least 100,000, but this figure is no measure of the reproductivity of the cestode, because as fast as new segments break off at one end new ones are formed just behind the head, and the animal goes on producing new segments very much in the same way as a recurring decimal reproduces ciphers. Hence the eggs of this cestode must be scattered in countless millions all over the grouse moors. They are probably eaten by some insect or land mollusc and in the body of these invertebrates change into the cysticercus or larval stage.

The popular notion that grouse do not eat animal food is entirely wrong. For the first three weeks of the bird's life the greater part of its diet consists of insects or arachnids, and from the crop of the first grouse I ever dissected I took six saw-fly larvæ, eight caterpillars of a Geometrid moth, one caterpillar of a smaller moth, two small Tineid moths and a number of Hemipterous insects resembling the frog- or cuckoo-spit, a fly, two specimens of plant-lice, one small spider, and the remains of four slugs. The gizzard of the same animal contained, in a more broken-up condition, two or three dozen larva of saw-flies and moths, some young Hemipterous insects and the pupa of two true flies.

In searching for the larval or cysticercus stage of these and the other cestodes, we have examined a considerable number of insects which occur commonly on grouse moors. We have also carefully searched the bodies of many fresh-water crustacea which abound in the pools and tarns from which grouse drink, but hitherto our searches have met with no success. One specimen of cestode which infests the common fowl is said to have its second host in several species of the slug *Limax*, but we have not succeeded in finding cysts of either form of tape-worm in this slug.

Besides the large tape-worm (*Davainea urogalli*), which was described by Baird fifty-seven years ago, and the thread- or round-worm (*Trichostrongylus pergracilis*), described by Cobbold thirty-seven years ago, we have two other species of tape-worm and four other species of round-worm. One of the former is negligible, the other, the transparent (*Hymenolepis microps*), is however to some extent associated with disease. These worms, like the larger species, may exist in incredible numbers in the duodenum or that part of the alimentary canal which comes just after the gizzard: yet they are quite invisible whilst alive. The contents of the alimentary canal in this region resemble a thick *purée*, which, on the addition of some fixing reagent, resolves itself into an inextricable tangle of fine threads, each representing a tape-worm. The head of these worms is hidden away in the folds of the lining mucous membrane of the alimentary canal, and undoubtedly they do something to interfere with its continuity. A certain amount of inflammation is set up. We have no sure information as to the second host of this cestode, but as a general rule the cysts of the genus *Hymenolepis* live in some insect or centipede, as is shown by the fact that the adults exist in bats, insectivores and insectivorous birds. Tape-worm cysts have recently been found in a flea by Professor Minchin, and these cysts have been shown by Mr. Nicoll to grow into *Hymenolepis diminuta* in the intestine of the rat. Hence the suggestion, first made by Dr. Leiper, that the fleas of the grouse may be the second host of *H. microps* is well worth following up.

It is curious to note that this tape-worm disappears during the winter months, a fact which may afford some hint as to its second host. The large tape-worms, on the other hand, remain all the year round, and must be of quick growth, for they are found 35 cm. in length in a young grouse but three weeks old.

Three other round worms have also been shown to exist in the grouse. One of these in the duodenum may prove of importance. This species (*Trichosoma longicolle*) is allied to a form which lives in the human appendix, and at times is the cause of appendicitis.

COCCIDIOSIS.

Besides the worms, we have in the grouse seven distinct unicellular or protozoan parasites which live in the intestines or in the blood of the grouse. Most of these are uncommon and comparatively harmless, but one, a *Coccidium* (there is no more popular word for it), is the cause of disease in the grouse chicks.

Since this disease was first found in the young grouse much has been written about it in the newspapers, and in nearly every case the writer has taken the *Coccidium* to be a *Coccus*. Now a *Coccus* is no more like a *Coccidium* than a crocus is like a crocodile. The *Coccus* is a bacterium, a vegetable, and it has a simple life-history; the

Coccidium is a protozoan, an animal with, as we shall see, a very complicated life-history.

Dealing first with the Coccidiosis :

One of the aims of grouse-preservers is to have numbers of healthy young grouse chicks in order to produce stocks of strong birds. Bad seasons for grouse are partly due to epizootics among the young broods in the spring, and the chief cause of mortality among the grouse chicks is a small, one-celled microscopic animal parasite, *Eimeria* (*Coccidium*) *avium*. This parasite penetrates the lining membrane of the gut of the bird and gradually destroys it, thereby setting up digestive troubles in the form of intestinal inflammation (enteritis), accompanied by acute diarrhœa, which usually terminates fatally. Grouse chicks are most susceptible to Coccidiosis during the first six weeks of their life, and if they can survive this period unattacked they usually reach adult life.

The disease—we may call it Coccidiosis for short—caused by this species of Coccidium (*Eimeria avium*) is brought about in this way. The grouse moor is simply peppered over with millions of oval cysts, or capsules, which represent the free-living stage in the life-history of the Coccidium. Each cyst is very resistant to changes of temperature and moisture, and can live for a long time. The cysts pass with the food or the water or the grit into the alimentary canal of the bird, and in the duodenum the thick cyst wall is dissolved and four spores emerge. Now, when one reflects on the thousands of cysts which are at times taken up by the grouse, one can readily understand that the presence of these numerous spores boring into the epithelial cells ultimately destroys the lining of the duodenum, where in fact the most active digestive processes are carried on in the normal bird. Furthermore, the Coccidia multiply in the intestine, and the resulting progeny attack new regions of the alimentary tract, especially the cæca, which become swollen and inflamed. After a time some of them produce small forms (males) and others change into large forms (females); these two forms fuse and the resulting stage is the cysts mentioned above, which, passing from the body, infects the whole moor. There are many details omitted in this short abstract of the complicated life-history of this parasite, which has been worked out in the grouse by Dr. Fantham at Cambridge; but enough has been said to show the nature of the disease, which is well known to occur in several other animals. There is a Coccidiosis of the rabbit, and a very fatal form attacks pigeons, fowls and pheasants, and the grouse Coccidium, if administered to chickens, will set up the disease in fowls.

To repeat a little, for the life-history of this parasite is very complicated.

The chief source of contamination on the moors is the droppings of other diseased grouse. The droppings contain thousands of cysts (oöcysts) or spores of the parasite, and these spores, with their hard

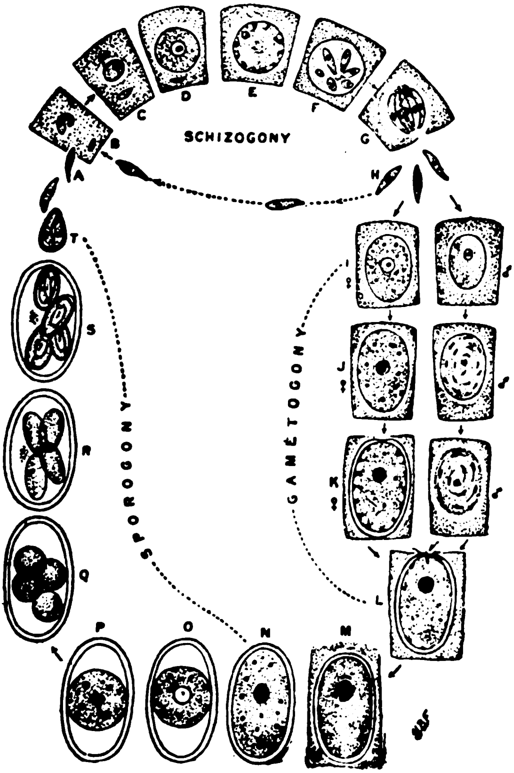

FIG. 1.—Diagram of the life-cycle of *Eimeria* (*Coccidium*) *avium*.

A–B illustrate the asexual reproduction (schizogony) of *Eimeria* (*Coccidium*) *avium*; I–L illustrate the formation of sexual forms (gametogony) of *E. avium*; N–T show the formation of the oöcysts (cysts) and sporocysts (spores) of *E. avium* (sporogony); A, sporozoite or primary infecting germ, which is penetrating an epithelial cell of the duodenum of the host; B, sporozoite curving on itself before becoming rounded within the host-cell; C, young growing parasite; D, fully grown parasite; E, parasite (schizont) with its nucleus divided (seen in transverse section); F, parasite forming merozoites, *i.e.* daughter forms of the dividing parasite; G, group of merozoites, arranged like the segments of an orange; H, free merozoites. I ♀, young female mother cell with coarse granules; I ♂, young male mother cell with fine granules; J ♀, growing female mother cell; J ♂, young males beginning to form in mother cell; K ♀, female gamete that has formed a cyst-wall for itself, but left a thin spot for the entry of the male; K ♂, fully formed males attached to their mother cell; L, union of ♂ and ♀ : one male only unites with the female, the others are excluded; M, further stage in fertilization; N, fertilized oöcyst (cyst) with its contents filling it; O, oöcyst (cyst) with contents forming a central mass: many such cysts are seen in the droppings of infected grouse: P, oöcyst (cyst) showing first stage in division into four: Q, oöcyst with four daughter spores forming; R, oöcyst with four fully formed sporocysts (spores); S, cyst with four sporocysts (spores), in each of which two sporozoites are formed; T, free sporocyst in which the sporozoites have taken up the most suitable position for emergence.

coats, are extremely resistant and can endure for very long periods without the death of their contents, which gradually divide to form four smaller spores inside. The spores are scattered over the moors by the action of the wind and rain, and, alighting on the heather or in the tarns of the moors, are taken up by the grouse in their food or drink. When the cysts are swallowed they enter the gizzard of the bird and pass unchanged into the first part of the intestine, called the duodenum. Here the pancreatic juice is poured into the intestine to aid in digestion, and under its influence the cyst-wall is softened and dissolved, and the four small spores (contained within the ripened spore or oöcyst) are set at liberty. Each small spore contains two active motile germs or sporozoites, which emerge from the softened spore-case and proceed to penetrate the epithelium of the duodenum. The young parasites ultimately cause the destruction of the lining of the first part of the small intestine—the region where, normally, the most active digestive processes occur. The *Coccidium* parasites multiply in the duodenal epithelium and then invade the cæca, or "blind guts," with disastrous results.

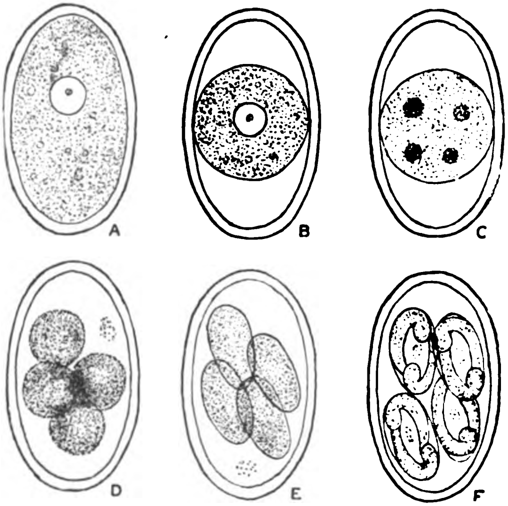

FIG. 2.—Oöcysts (cysts) of *Eimeria* (*Coccidium*) *avium*.

A, oöcyst (cyst) with contents completely filling it; B, oöcyst with contents concentrated to form a central mass; C, commencement of division of the oöcyst contents; D, oöcyst containing four rounded masses that will become spores; E, oöcyst containing four sporocysts (spores); F, oöcyst containing four sporocysts, in each of which two sporozoites have formed.

Sooner or later a limit is reached, on the one hand, to the power of the grouse chick to provide nourishment for the parasites, and, on the other hand, to the multiplicative capacity of the parasites themselves. The *Coccidium* then begins to reproduce sexually. Many small male parasites are produced, together with larger food-containing female *Coccidia*. The male and female parasites conjugate and then encyst, bursting through into the cavity of the gut and giving rise to the spores found in the cæcal droppings on the moors.

So far as the grouse chick is concerned, the formation of Coccidian cysts means either recovery or death. If the infection of the parasites has been a heavy one, and multiplication of the parasites has proceeded apace, then the destruction of the intestinal epithelium has been so great that death of the grouse chick results. If, on the other hand, the epithelium of the intestinal wall has not been too much destroyed

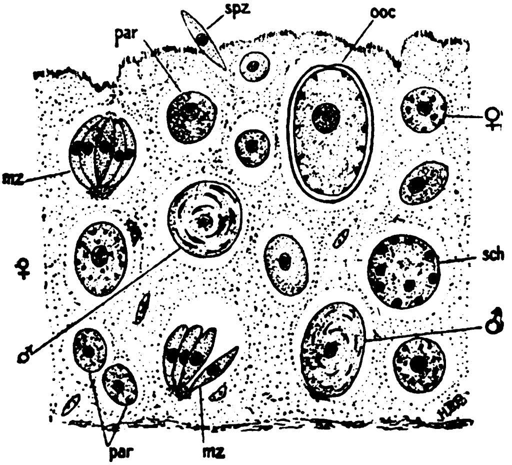

Fig. 3.—**Small piece of the epithelial lining of the gut of an infected grouse chick, showing various stages in the life-history of *Eimeria (Coccidium) avium*, parasites of different ages being present.**

***par*, parasite; *spz*, sporozoite or primary infecting germ; *sch*, schizont or dividing form; *mz*, merozoite or daughter germ; ♀, female Coccidium; ♂, male parasite; *oöc*, mature female, ready for fertilization, with oöcyst wall formed round it.**

—fewer Coccidian parasites having been present—then the gut-epithelium may slowly regenerate, and the young bird gradually recover and improve in condition, after the Coccidian spores have been passed out with the cæcal droppings.

As a rule grouse are most susceptible to Coccidiosis during the first six weeks of their life, and if they can survive the dangers of this period they usually grow up. The chief external symptom of the disease is diarrhœa; the legs show weakness, and the feathers, especially round the legs, are in poor condition; flight is feeble, and the bird loses weight. Internally the alimentary canal is inflamed and digestion greatly impaired; perityphlitis is set up around the cæca, which become greatly enlarged. The blood corpuscles also undergo marked alteration and an anæmic condition prevails. Further, the destruction of the lining wall of the alimentary tract allows the escape of bacteria which are all very well in their place—*i.e.* the cavity of the intestine—but which are apt to set up trouble when they make their way into other tissues. This is, however, but a subsidiary matter; the real injury caused by the *Coccidium* is the destruction of the lining membrane of the alimentary canal.

Coccidiosis may be spread from moor to moor by the agency of flies. The maggots of certain flies readily eat the cysts, and it has been shown both experimentally and on the moor that the cysts pass through the bodies of both maggot and fly undigested and unharmed.

Several other one-celled microscopic organisms or protozoa besides *Eimeria* (*Coccidium*) *avium*, as is mentioned above, have been found in the intestines of grouse and also in their blood, but in no case, so far, has any of these parasites been shown to have a markedly dangerous effect on the bird harbouring it. Some of these parasites, in fact, occur in perfectly healthy grouse and apparently are almost innocuous.

STRONGYLOSIS.

The second disease which the Inquiry has found responsible for grouse epizootics observed between 1905 and 1910 is one to which Cobbold drew attention in 1873, though he attempted little in the way of pathological investigation. According to him it is caused by the presence of a round-worm, now known as *Trichostrongylus pergracilis*, in the cæca. We may call the disease "Strongylosis of the grouse." The worms are minute, transparent, very slender, a little less than half an inch in length, and they may exist in enormous numbers, 10,000 occurring in the two cæca of one bird They are about equally divided between the two blind-guts. We may recall the fact that in the grouse the cæca are of unusual size, and that in these birds the digested food is absorbed in this region of the alimentary canal alone.

The worms seem to be most numerous at the proximal end of the cæca, and when they exist in large numbers the contents of these most important diverticula of the alimentary canal become hard and very adherent to the mucous membrane, forming whitish patches when seen from the outside. After washing away the flocculent matter, which can only be effected with time, the mucosa frequently appears reddened. There was no reason to believe this inflammatory state is due to post-mortem changes. The ridges which run along the walls of the cæca become thickened, the villi, as a rule, greatly hypertrophied (in other cases atrophied), and both villi and ridges were embedded in the above-mentioned cementing material, and seem to be composed of a mixture of mucous and granular débris. Numerous Strongyles were seen adhering to the mucous membrane, and seem in places frequently to penetrate the villi. In some of the most serious cases the ridges were found to resemble masses of coral, with cave-like depressions between the individual villi which frequently cease to be distinguishable ; from these depressions one or more Strongyli protruded. In section the epithelium was seen to be hypertrophied. The muscular wall suffered from an infiltration of fibrous tissue. The fat which normally lies at the bases of the ridges in a healthy grouse had disappeared and the blood-vessels showed thickenings of their walls. The connective-tissue base of the ridges is greatly increased, and the vessels and the villi were dilated and probably increased in number. In most places the connective tissue contained a large number of cells probably inflammatory in origin, and in some cases fibroid change was taking place. The epithelium was proliferated and thrown into folds. The lymphoid follicles were indistinguishable

The general condition of a grouse suffering from Strongylosis shows evidences of a chronic inflammation of the alimentary canal leading to fibrosis. The *Trichostrongylus* in some cases penetrates to the deepest portions of the crypt. The epithelium lining these depressions when the round-worm is adjacent to the walls of the intestine has greatly altered, and there is a marked increase in the fibrous tissue at its base.

In fact we have, as Dr. Cobbett and Dr. Graham Smith, who have so ably worked out the pathology of Strongylosis, and whose results have appeared in the *Journal of Hygiene*, say, " a chronic inflammation leading to fibrosis." This portion of the alimentary canal is, in Sydney Smith's words, both " vext " and " agitated."

It thus appears that the presence of those Strongyli leads to a chronic inflammation of the cæca and to fibrosis. The normal functions of these very important organs, the cæca, are interfered with and the tissue lining them is destroyed. The bird wastes away and ultimately dies. But there is another factor in the case. When the degree of infection surpasses 1,000 Strongyli in each cæcum, there is no doubt a larger increase in the *Bacillus coli* in the liver and in the lungs and other organs. These bacilli do no harm whilst free in the alimentary canal, but when they pass the mucosa and reach the other tissues of the body they undoubtedly serve to set up various disorders.

But the presence of these worms, and also, I believe, to a minor extent the presence of the transparent tape-worm in the duodenum, has a further and indirect ill-effect on the grouse. When hand-reared birds which were free from *T. pergracilis* were investigated, practically no bacilli were found in the liver or other tissues of the grouse. When the birds harboured from 100–1000 round-worms, bacilli occurred in the tissues of about 50 per cent. of the birds—bacilli let out from the cavity of the intestine "by a worm's pin-prick," to quote Browning's "Lovers' Quarrel." When over 1000 or more worms were present, the bacilli, with one exception, were found in the liver and in other organs of the body in 100 per cent. of the birds investigated. The exact relation of these bacilli to the sickness of the bird is still a matter of inquiry. It seems as if they are soon absorbed and that no specific disease is traceable to them, but if they exist in numbers their products must exert a harmful influence.

The existence of disease caused by the passage of these bacilli through the walls of the alimentary canal through lesions caused by tape-worms is less well established than through the disorganisation of the lining membrane of the cæca caused by *T. pergracilis.* On the other hand, one must not overlook the fact that *Hymenolepis* is especially numerous in the spring and autumn months, during which the greatest mortality takes place, and is absent during the winter, when the birds are comparatively healthy.

It is seen that the small number of colonies of *Bacillus coli* which can be cultivated from the tissues of a diseased grouse points to the fact that these bacteria do not multiply to any extent in the tissues. Hence it would seem that no specific disease is caused by the infection of these bacteria. The toxic products of the bacilli, however, may be harmful, and should the bacilli exist in great numbers there is little doubt that toxins would be produced which would have a very deleterious effect on the health of the bird.

But let us leave the bacteria and get back to the round-worms. How do they get into the grouse? Between 95 and 100 per cent. of birds on different moors contain these worms. There may be as many as 10,000 in round numbers in one grouse, about equally divided between the two cæca. Each female worm lays hundreds of eggs, which are constantly passing out of the bird's body and lie scattered all over Scotland. These eggs give rise to larvæ in about two days; the larvæ surround themselves about the eighth day with a capsule or cyst and undergo a "rest cure." After a period of quiescence they quickly change into a second and active larval form, which in wet weather—a not unusual atmospheric condition in Northern Britain—writhe and wriggle and creep and crawl until they attain the stem and the leaves and the flowers of the heather. Here these larvæ wait patiently until a grouse consumes them with the heather tips, and then, once inside the alimentary canal, they become adult, make their way to the cæca, and in four days ripe eggs are again infesting the moors.

It is recorded that Prince Bismrack once said to Lady Randolph Churchill, "Have you ever sat on the grass and examined it closely? There is enough life in one square yard to appal you."

It has always seemed to me a strange thing for the Prince to have said. To begin with, throughout his long life he had shown but an imperfect sympathy with the lower Invertebrata, and then, again, he was a man not easily appalled: but the saying is perfectly true. It is difficult for the layman to grasp what is going on in and on the soil and on the plants which it supports. Suppose we could by means of a gigantic lens magnify a square yard of a grouse moor one hundred times. The heather plants would be as tall as lofty elms, their flowers as big as cabbages, the grouse would be about six or seven times the size of "Chantecler" at the Porte St. Martin.

Creeping and wriggling up the stem and over the leaves, and gradually, yet surely, making their way towards the flowers, would be seen hundreds and thousands of silvery-white worms about the size of young earth-worms. Lying on the leaves and on the plant generally would be seen thousands of spherical bodies the size of grains of wheat—the cysts of the *Coccidium*; and on the ground and on the plants as large as split-peas would be seen the tape-worm eggs, patiently awaiting the advent of their second host. It is perhaps a picture which will not appeal to all, but yet it represents what, unseen and unsuspected, is always going on on a grouse moor.

Two other points remain, the seasonal character of the disease, and whether any means can be suggested to check either Coccidiosis or Strongylosis, or both.

"Grouse disease" is always said to be at its worst in the spring months, to decline during the summer, and to recrudesce in a milder form in the autumn. Coccidiosis undoubtedly is a spring disease; it attacks the chicks, and if they survive the first six or seven weeks of their life they usually live to grow up. This disease certainly abates during the summer, but it does not recrudesce during the autumn. Strongylosis also occurs most virulently in the spring, when the birds are exhausted by a winter of semi-starvation, and the female especially by the demands made on her by egg-laying; it is also prevalent in autumn, but the worst cases have by this time presumably been killed off, and those not so heavily afflicted are still struggling to survive It is not as a rule reported during June and July, but very few know what happens on the moor during these months. The grouse are almost unseen, their state of health is unknown. This again is a matter for further inquiry, but at present the view that "disease" dies down during the summer has little but negative evidence to support it. It probably lingers on, gradually lessening in intensity until the near approach of August 12 again attracts the attention of the moor owner and sportsman to his birds.

With regard to the prevention of the disease, a hopeful view can be taken. Intelligent management has already diminished, and in certain cases almost, if not quite, eliminated the danger of disease, and this without resort to outside aid or scientific advice. There is reason to hope, with a clearly defined objective and a more general realization on the part of moor owners, not only of the immediate cause of grouse disease, but also of the contributory conditions leading thereto, that the best methods which obtain at present in moor management will be more widely adopted, and that the suggestions of new lines of experiment which will be put forward in the final Report will be followed up.

As to the stay of the disease, when fully established in the bird no practical remedial measures can at present be suggested. The apothecary is to the sick bird of no more importance than Seneca. Nothing can be done " pour soulager les entrailles " of the patient.

One word of conclusion to recall the fact that the Grouse Inquiry has not issued its final Report, and this article cannot represent its final findings ; research is still going on. All I have attempted to do is to summarize the existing knowledge of the diseases of the grouse, which have never before been so specifically defined. When the final Report is published remedial measures will be suggested. This Report will also contain chapters on moor management, on the economics of grouse-shooting, on the life-histories of the many parasites associated with the grouse, and on many other matters connected with the well-being of the bird.

[A. E. S.]

GENERAL MONTHLY MEETING,

Monday, February 6, 1911.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Alfred Carpmael, Esq., B.A.
Mrs. Alfred Carpmael,
Walter Stacy Colman, Esq., M.D. F.R.C.P.
Guy Ellis, Esq.
Mrs. Edward Brocklehurst Fielden,
Aaron Harold Levy, Esq., B.A. M.D. C.M. F.R.C.S.
Basil Mott, Esq., M.Inst.C.E.
Alan Faraday Campbell Pollard, Esq.
Nathan Raw, Esq., M.D. B.Sc. F.R.C.S.E.

were elected Members of the Royal Institution.

The Chairman reported that the Treasurer had received £1200, part of the legacy to the Royal Institution of the late Miss Wolfe, and £62 10*s*., part of the legacy of the late Mr. C. E. Layton.

The Special Thanks of the Members were returned to Dr. J. Y. Buchanan for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Special Thanks of the Members were returned to Dr. F. Blumbach and Mr. H. N. Mladentseff for an etched portrait of the late Professor D. Mendeleef, *Hon.Mem.R.I.*, by the Russian artist Roondaltseff.

The Honorary Secretary read the following Letters received from the Honorary Members who were elected at the General Meeting on December 5th, 1910:—

Charlottenburg 2,
Marchstrasse 25b,
10*th December*, 1911.

Translation.

Dear and Honoured Sir,—I beg to acknowledge herewith the receipt of your esteemed communication of the 5th December telling me that I had been chosen as an Honorary Member of the Royal Institution.

Will you kindly convey to the Royal Institution my most grateful thanks for the distinction shown to me by their choice?

I remain, dear Sir,
E. Warburg.

Moscow 11, XII., 10.

SIR,—I have the honour to inform you that I have received your communication that the Royal Institution of Great Britain has conferred on me the highest dignity in electing me an Honorary Member of the Institution.

Would you be so kind to transmit to the Royal Institution my sincerest acknowledgment of this high honour?

I am, Sir,
Your very obedient servant,
PETER LEBEDEW.

ROME,
Translation. *December 15th,* 1910.

ILLUSTRIOUS SIR,—The information conveyed to me by your kind letter of December 5th last has caused me extreme gratification, since I appreciate and understand the high honour which has been conferred on me by my election as a member of the Royal Institution of Great Britain.

Reserving my thanks to his Grace the Duke of Northumberland until I shall have received the diploma, I beg you meanwhile to accept my heartiest thanks on your own behalf and on behalf of the eminent members of the august body.

With perfect esteem, I have the honour to be,
Your most devoted servant,
E. PATERNO.

Translation.

SIR,—On my return from my travels I found the letter telling me that I had been admitted as an Honorary Member to the Royal Institution of Great Britain. I cannot tell you how flattered I feel at this honour shown to me, and I ask you to express my gratitude to your colleagues.

Yours most faithfully,
POINCARÉ.

NATIONAL GEOGRAPHIC SOCIETY,
WASHINGTON, D.C.
December 17, 1910.

DEAR SIR,—I have the honour to acknowledge receipt of your letter of December 5th addressed to Dr. Alexander Graham Bell, informing him of his election as an Honorary Member of the Royal Institution of Great Britain.

Dr. Graham Bell is at present in China and will not return to the United States until about May 1st, when I am sure he will be very much gratified to learn of the honor the Royal Institution has conferred upon him.

Yours very truly,
GILBERT H. GROSVENOR.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday, February 10, 1911.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

SIR SIDNEY COLVIN, M.A. D.Litt.,
Keeper of Prints and Drawings, British Museum.

Robert Louis Stevenson.

AT the beginning of August 1873—that is between thirty-seven and thirty-eight years ago—I remember it as if it were yesterday—I landed from a Great Eastern train at a little country station in Suffolk, and was met on the platform by a slender youth in a velvet coat and straw hat, who walked up with me to the house where he was staying and where I had come to stay. I was then a somewhat juvenile professor at Cambridge; the house was Cockfield Rectory, near Bury St. Edmunds; the host was my much older colleague, Professor Churchill Babington, of amiable and learned memory; the hostess was his wife, a grand-daughter of the Rev. Lewis Balfour, of Colinton, Midlothian; the youth was her young cousin from Edinburgh, Louis Stevenson, whom I then saw for the first time. Another guest in the house had written advising me to come if I could before the youth went away, as he seemed a fine young spirit and she was sure I should find him interesting. Interesting he was with a vengeance. The next few days and evenings were spent revelling in a bath—a flood—how shall I say? under an illuminated and rainbow-tinted cataract of talk such as I never experienced before, nor since, except in the same company. Perhaps I may mention parenthetically that I was not unused to the presence and ways of genius. I had had the luck, as little more than a child, to receive drawing lessons from Ruskin, Ruskin the golden-mouthed, and cake and sherry—which I was much better able to assimilate—from his mother; and at the time I speak of was a close intimate in the circles of Rossetti and Burne-Jones, impressive personalities and inspiring companions both of them. As to Burne-Jones especially, many of you here present must remember the abounding gaiety and imagination and knowledge, the charm and classic purity of his beautiful and caressing talk. W. K. Clifford again, the great mathematician—some of you, I am sure, can conjure up in memory his muscular Socratic face and candid eyes, and how he would hold his friends hour after hour, while in his small quiet voice, and in words almost of one syllable for simplicity, he made the abstractions of speculative physics and of metaphysics as thrilling as a fairy tale and as easy as

A B C, or spun real fairy tales which never ended and which you never wished to end. But this new Scottish youth, his first shyness past, beat everything I had ever known. Genius shone from him; he held and drew you by the radiance of his eye and smile no less than by the enthralling quality of his conversation. The new company in which he found himself drew him out, and jest and earnest, wisdom and folly—folly of the illuminating kind which is sometimes wiser than wisdom itself—streamed and flashed from him.

He was only twenty-two, but seemed already to have lived and felt and suffered and laughed and thought more than others do in a long life-time. He seemed moreover to have read everything, at least in English and French—for he was very little of a classical scholar—and his mind and speech ran exultingly over the whole range of life and literature, as the fingers of an inspired musician run over the keyboard of an instrument. Grave argument and criticism and pure poetic eloquence would alternate with bursts of riotous slang and insanely apposite misquotation; stale and vulgarised forms of speech would gain brilliance and illuminating power from hitherto undreamt-of applications; and all the while an atmosphere of goodwill, a glow of eager benignity and affectionate laughter, would diffuse itself from the speaker, till everyone about him seemed to catch something of his own gift and inspiration. This sympathetic power of inspiring others has been noted by many of those who knew him as an especial and distinguishing note of Stevenson's conversation. As long as he was there you kept discovering with delight unexpected powers in yourself: you felt as if you had taken service with some wonderful conjuror, whom you supplied with balls of clay and who took them and transmuted them into gold and sent them spinning and whirling and glowing about his head, and kindly made you believe all the while that they were still truly your own.

Underneath all this gaiety and charm it soon became clear that there lay a troubled spirit, in risk from the perils of youth, from acute distress over temporary misunderstandings at home, from a constitution naturally frail and already overstrained, from self-distrust and uncertainty as to his own powers and purposes. It was the good fortune of his new friends to be able to strengthen, encourage, and steady him. My own part in particular at this juncture was to make him known to editors and get him a start on the path of literature, to which his own instincts and private self-training were already diffidently pointing. This, I have said, was in the summer of 1873. A very close friendship ensued between us, which was maintained by correspondence quite unbroken even after he had made his home in a remote island of the Pacific.

Twenty-one years had passed, when on a gusty, sodden December day in 1894 I came down from lunching in a friend's rooms in

Westminster, and saw newspaper posters flapping at the street corners with the words "Death of R. L. Stevenson" printed large upon them. Judge what a light went out of my world with the reading. Nor was it a light extinguished only for his friends and those who had been familiar with the charm of his character and presence. So well had he spent his time, in the teeth of cruel hindrances from ill-health and mortal weakness, that it was a light extinguished for all those who cared for the English tongue and for English letters throughout the world.

For what had this frail Scottish youth done with his life and talents in the interval since that summer week when he was my companion in the Suffolk lanes and garden? At school and college the most notorious of truants and reputed idlers, once his way of life determined he had stuck to his chosen art with an intense tenacity of industry under circumstances often cruelly trying. After four or five years of prentice work, always aiming at the highest, never satisfied with what he had done, toiling, as he says of himself after Balzac, like a miner buried under a fall of earth, he began about his twenty-seventh and twenty-eighth year to impress a small circle of judicious readers by the charm and vividness of the short tales, parables, essays, and notes of travel, which appeared in magazines under the signature "R. L. S." His first three books, "The Inland Voyage," "Travels with a Donkey," and "Picturesque Notes on Edinburgh," the two former now classics in their dainty cameo kind, had been published when suddenly, in August 1879, he left us all, parents and friends, to risk his whole life on what seemed to us a desperate gambler's throw for happiness. Taking no money and expecting none from his father, travelling to California and living there under a double strain of mental anxiety and self-imposed hardship beneath which a strong man might have broken down, he fought as hard a six month's fight as is recorded in the life of almost any man of letters. Then he fell sick almost to death, recovered partially though never fully, and came back after a year's absence with the wife whom he went out determined to make his own at all costs. Reconciled to his family, dearer than ever to his friends, he remained a stricken man in health, and for seven years lived the cramped life of an invalid, spending at first the winters at Davos and the summers in the Highlands, then trying Marseilles and Hyères, where he had a few months' respite from illness, then lastly settling for two years and a half at Bournemouth. Throughout this period he was debarred by weakness from almost all exercise, and kept never far from the risk of death by hæmorrhage of the lungs.

To his adventurous, active, outdoor spirit the restrictions and deprivations of the sickroom were galling in the extreme. But he never let them quench his spirit, or even his spirits. He remained inextinguishably gay and charming to those about him; as inspired and inspiring a talker as ever, when he was allowed to talk at all—

as he was often forbidden to do except by signs and written notes for weeks together. And he continued indefatigably to work, work, work; delighting more than ever in his art, more than ever unwearied in his efforts to come within sight of his unattainable ideal. His mind was always humming with projects for romances, dramas, treatises on criticism and on ethics, more than could be accomplished in the lifetime of ten men unhampered by illness. Five-sixths of such projects came to nought, as they needs must; but the amount actually carried through seemed a miracle, given the conditions under which he wrought. These seven years were the years of "Treasure Island," the masterpiece among boys' books since "Robinson Crusoe": these were the years of "Prince Otto," that highly wrought piece of *marivaudage*, with its artificial Court comedy intrigue suddenly taken out from the palace into the open night, suddenly bathed and transformed in the sweet influences of starlight and the forest dews: they were the years of the "Child's Garden," those verses in which, at moments of physical slackness and fatigue, the undying child in Stevenson awoke and piped: of "Kidnapped," that truncated but none the less masterly romance of Highland and Lowland character, of the fatigues and straits of hunted men, of friendship strained for an hour and in the end strengthened by the strain. Lastly, these were the days of "Jekyll and Hyde," in which the vulgar art of the bloodcurdler is tuned up and strengthened to the production of a parable speaking thrillingly to the general heart and conscience.

"Treasure Island" had appealed to a wide circle of readers. "Kidnapped" appealed to a much wider; "Jekyll and Hyde" to a much wider still. By the success of these last two books Stevenson seemed, in his thirty-sixth year, to be assured of fame and competence. But health was farther off than ever. The death of his father broke the strongest tie which bound him to this country: the doctors urged a complete change. His wife's connections pointing to the west, he sailed with his remaining family in August 1887, his friends waving to him what they were spared the pain of knowing were their last farewells as the ship unmoored from Tilbury dock. Then came a cheerless but tonic winter in the Adirondacks, with the "Master of Ballantrae" and the essays of the "Lantern Bearers" and "Pulvis et Umbra" series for its chief fruits: then the determination to try a cruise in the Pacific Islands, the longing for which had lurked in his heart for a dozen years, ever since in a troubled hour at Edinburgh a guest in his father's house from New Zealand had told him of the healing power of those scenes and climates. Then followed the enchantments of the cruise itself, the sense of returning vigour, the rush of new interests; the renewed failure of health at Sydney; the purchase of a plot of forest land on the mountain side in Samoa; the determination to make his home there; the settlement, the clearing, the building, the happiness of a recovered capacity for outdoor life, the gathering about him of a devoted household, almost like a Scottish clan, of native servants. Then his entanglement in the maze of island politics, his hospitalities, his responsibilities, and through and above all the unceasing, the ever-growing passion and assiduity of work. Some of that work was no doubt partly wasted on material he had not yet fully grasped. Such, at least to my mind, was the series of chapters on the South Seas, composed with incredible labour; though I am bound to tell you that a more authorised judge, a many-voyaged master-mariner as well as a gifted master-writer—no less a person than Mr. Joseph Conrad—disagrees with me and likes the South Sea book better than "Treasure Island." Such a waste, again, I should say, was the "Footnote to History," dealing in scrupulous detail with the broils, blunders, and intrigues of local politicians on that petty and remote scene. On the other hand his new experiences yielded him much material for his only long novel of modern life, "The Wrecker," as well as a story, the "Beach of Falesá," which breathes, as those say who know the islands best, the very quintessence of the island life and character: they yielded, in the "Ebb-tide," some of the most hateful characters and some of the grimmest, but at the same time most vivid and intense, chapters that he ever produced. But it was still Scotland and the thoughts and memories of his home, growing in his exile ever deeper, richer, more charged with emotion and shot through with imagination—it was these that still best inspired him. First came "Catriona," the exquisite if not perfectly well-fitting sequel to "Kidnapped"; then one project after another conceived and begun but not finished—"Heathercat," the young chevalier, "St. Ives," and lastly "Weir of Hermiston." In this last romance, so far as it was carried, all Stevenson's gifts seemed raised to a new and higher power. It seems as though all his writings up to this date, his forty-fourth year—the age, remember, at which Scott wrote "Waverley"—it seems as if they had all been only so much experiment, only so much preluding on his instrument, and that the day of his full power had only now dawned. But that dawn was never to pass into full noon. His physical powers were not equal to the tension of his mind and spirit. The soothing climate of the South Pacific had relieved and gone far to cure the old paralysing weakness, the old menace from the lungs. But one day, without warning, while he was rallying his wife on the gloomy forebodings that beset her, a blood-vessel broke in his brain, and suddenly, in the midst of his noblest work and happiest projects, he was dead.

The news of that death reached London, as I have said, in mid-December. You, the Managers of this Institution, showed your sense of its importance by inviting, in the course of the following May, a brilliant young critic as he was then—Sir Walter Raleigh, Professor of English Literature at Oxford, as he is now—to speak of Stevenson to your members in this hall. Among many wise and just things said by the speaker was this: "When a popular writer dies, the question it has become the fashion with a nervous generation to ask, is the question, 'Will he live?' There is no idler question, none more hopelessly impossible and unprofitable to answer." But, at least, added Sir Walter Raleigh, "he has made a brave beginning in that race against Time which all must lose." Sixteen years have passed since Sir Walter Raleigh spoke, and of the progress of that race we can now see more. That Stevenson holds his own gamely is proved among other things by the fact that you have done me the honour to ask me to speak of him here to-day. The score or so of years following a famous writer's death are those most dangerous for his reputation. They are those in which its decline begins if it is going to suffer early decline at all. At present Stevenson's reputation has undergone no such shake as has befallen those of writers far greater and far more celebrated in their day. I hear, for instance, many clever young people of to-day scoff at Tennyson: and I could beat them for it. Our great Victorian Virgil was truly a Virgil, never doubt it, although he was Victorian. George Eliot, whom the thoughtful readers of my generation thought almost the greatest of novelists—George Eliot's fame threatens to founder—I hope it never may—from the weight of philosophical reflection and formula with which her imagination and her wit and her profound sympathetic knowledge of English provincial character were ballasted. Meantime, Stevenson's lighter bark sails on more buoyantly than it ever did. The editions of his works keep multiplying, a new generation of readers thinks even more of him and loves him even better than did his own; you can scarcely take up any current newspaper or magazine without finding some reference to his name, his dicta on life and letters, or the characters of his creation. If I may speak for a moment from personal experience, the number of letters which reach me, simply as one of his known friends, testifying not only to the delight, but to the strength and courage and comfort in distress received from his work, so far from diminishing, increases. In a word, it looks as if he, if any recent writer, bade fair to become one of the classics, and not one of the neglected but one of the loved and read classics, of our literature. In the time that remains to us let us try and define to ourselves the special qualities which give to Stevenson's work this power to charm and help us, this distinction, this apparent promise of permanence.

It is often asked, was he a truly great and original mind, or only a skilful refurbisher of old forms and fashions in literature? And again, did he write any special or single great masterpiece, like the masterpieces of the great novelists, which the world cannot be expected to let die? Was he a great creator of characters, as Scott was above all, or Dickens or Thackeray or Balzac, or any or the acknowledged masters of fiction you please to name? These questions are not quite easy to answer: let us try.

As to originality, that depends upon what you mean by the word. If to be original is to startle, to stimulate and bewilder by paradoxes, to emit flashes that dazzle like the flash of a magnesium light and leave a deeper darkness afterwards, then he was not. He could play those tricks in conversation, no man better, but did not choose to do so in writing. Perhaps in the last twenty years we have had too much of that kind of originality. It has become a literary fashion. Mr. Oscar Wilde began, and Mr. Bernard Shaw and Mr. Chesterton —witty persons both of them, and Mr. Chesterton sometimes, I think, a wise one—have each in his own way continued to play this trick of turning things upside down for us. But it is a trick of which one ends by getting tired. And everything will not bear being turned upside down. You may stand an empty pot on its head, but it is an empty pot none the less, or may be a little the more: a fact which our paradox-mongers sometimes forget. Stevenson's originality was of a different kind. If to be original is to be born with an extreme natural intensity of perception, feeling, and preference, to be in all things acutely yourself, to take nothing at second-hand, to have lived, felt, tested, intensely in experience or in imagination all that you utter before you utter it, to have an instructive and inbred unwillingness to accept the accepted and conform to the conventional, then he was one of the most original of men.

As to the question about masterpieces, what is most remarkable about him, you will agree, is not the pre-eminent quality of any one or two or three works, but the vitality and charm which he was able to give to work in so many different kinds—romances and short tales, both historical and modern, parables and tales of mystery, boys' stories of adventure, the moral, critical and personal essay, travels sentimental and other, memoirs, lyrical and meditative verse both English and Scottish, and even nursery verse, a new vein for genius to work in. All these forms he in his day re-animated by the personality of his own mode of conceiving and handling the vividness of his own vision and temperament. From the first he strongly held imaginative narration to be the true goal and test of his powers: so we are bound to consider his narrative work first. Personally, I should say that if we are to look for what can strictly and technically be called masterpieces in this vein, we shall find them among the shorter rather than among the longer tales. Does there exist a more gothically grim, more intense and vivid, short historical tale, every phrase thrilling and tingling with compressed life, than a "Lodging for the Night," with the rascal poet François Villon for its hero? Or a more true and touching and deftly handled comedy of conjugal relations than "Providence and the Guitar"? Better still, because searching deeper into the mysteries of life and conscience, are tales with a touch of symbol and allegory like "Markheim," which develops nearly the same idea as Stevenson in another place packs into the proverb, "Shame had a fine bed, but where was slumber? Once he was in gaol he slept." Or again "Will o' the Mill," the parable of the hanger-back from life and its adventures and responsibilities. In another of his proverbial sayings Stevenson writes, "Acts may be forgiven: not even God can forgive the hanger-back." Or more emphatically: "Choose the best, if you can; or choose the worst; that which hangs in the wind dangles from a gibbet." In "Will o' the Mill," you will remember, Stevenson deals more gently with the character of the hanger-back, setting forth the case that is to be made for and against him in a manner rich at once with human shrewdness and with imaginative suggestion; evoking first the allurement of the world's activities, then the self-approving wisdom of the man who decides to shirk them, then the circumstances which make him doubt that wisdom, then the mysterious, the fragrant, the reconciling approach of the great Unshirkable—Death. To combine genial human narrative and subtle symbolic evocation in this manner is surely beyond the range of any but a true and great master: indeed I know of no name but that of Nathaniel Hawthorne that in this respect is fit to be coupled with Stevenson's.

Coming to the longer tales and romances, it must be admitted that we often find points of invention and construction lying very open to criticism. "Kidnapped" seems nearly a faultless narrative; but in its sequel, "Catriona," we are disconcerted to find how little the failure of the hero's great purpose of bearing witness in favour of the innocent affects either his mind or the course of the story: and that James More, even he, should leave his daughter resourceless and alone on her landing in Holland is too great a strain on our credulity. But there is none, I think, of Stevenson's tales in which the power and charm of individual scenes and characters fail to carry us lightly over such flaws. To take one instance from many—in that fantasia on the sea and shipwrecks, as Stevenson calls it, the tale of "The Merry Men," criticism may well doubt whether the invention which ends the tale, of the black man chasing the conscience-tortured and storm-demented Gordon Darnaway till both are drowned, is a happy or satisfying one. But the flaw—if it is a flaw—scarcely counts, with such high imaginative mastery is the main theme rendered, that is the power and treachery and wickedness of the sea. Read that story first: then turn to a tale far more famous and five times as long—Hugo's "Travailleurs de la Mer"—and tell me which rings the truer; in which the words are the more charged and potent with the double-distilled essence of the sea magic and the sea fury. Again, it is a commonplace to find fault with the medicinal powders which effect the transformation of the two personalities in Jekyll and Hyde: but that was how the thing came to Stevenson in a dream, and when the essential machinery of a tale or parable is given you in a dream you must take it or leave it: change it you cannot. (Many of you will remember how in an essay called "A Chapter on Dreams" Stevenson narrates a first-rate story which came to him in sleep, but, for certain reasons belonging to its essence, could not be used: and in passing, let me suggest that passage as a model of the plain condensed narrative style scarcely to be beaten in English prose.) Criticise the invention of the powders as you will in cold blood, who can read the tale for the first time—or the twentieth—and not feel the awe and horrid thrill of it in his nerves and pulses, and through them in his mind and conscience?

Once more, in "Treasure Island," people familiar with the sea say that the adventure of Jim Hawkins in the coracle and the capture of the schooner are impossible. The best answer is Stevenson's own: "Of course my seamanship is jimmy; did I not beseech you, I know not how often, to find me an ancient mariner—and you did nothing for me? As for my seamen, did Runciman ever know eighteenth century buccaneers? No? Well, no more did I. But I have known and sailed with seamen too, and lived and eaten with them; and I made my put-up shot in no great ignorance, but as a put-up thing has to be made, i.e. to be coherent and picturesque, and damn the expense. Are they fairly lively on the wires? Then favour me with your tongues. Are they wooden, and dim, and no sport? Then it is I that am silent, otherwise not."

In this passage Stevenson throws up the defence of his nautical invention, and shifts the ground to a defence of his pirates. Let us pass with him to that other question of his gift as a creator of characters. In the sense of giving certain of his characters a high degree of individual vitality by calculated strokes of admirably adroit and telling art, it is clear that he was a remarkable creator. In the sense of peopling his canvases with a crowd of living figures, as some of the great spontaneous creators in literature have been able to do, or rather seem to have been compelled to do by an unconscious instinct, his gift, it would seem, was less. The only crowded canvas is "The Wrecker," a collaboration story, and in collaboration he was to my mind never at his best: but here I should say that Jim Pinkerton and Captain Nares at least are full-length figures as well characterised and unforgetable as any in recent literature. Certainly what most distinguishes "Treasure Island" from other pirate stories is the saliency and sharp discrimination of the individual figures; Long John, the fruitiest and most formidable leader of mutiny in all sea literature, Ben Gunn, the most delectable of marooned seamen, the Doctor, Captain Smollett, and the rest. In maintaining this quality Stevenson was no doubt immensely helped by his unequalled ear for dialects and modes of speech. Run your memory over the various strains and styles of language in which his puppets severally deliver themselves. What a range—from the rich traditional Lowland Scots, of which he was one of the greatest masters and perhaps the last, and which comes with such noble and moving eloquence from the lips of the elder Kirstie in the midnight chamber at Hermiston, to the hideous decayed dialect ranted from the ship's boat in the tropic lagoon by the deadly little cockney hell-cat Huish in "The Ebb-tide." Remember that range, and in this particular trick of his art you will acknowledge him a master almost beyond competition.

But what about the characters themselves, and the motives and springs of actions underneath their words? Granted, they are not very numerous—but remember how young he died, and how many of his projects had been nipped by illness. In romances of the past a singularly keen historic sense—as is attested by those who know their history best—came to the aid of his inborn creative powers, and Alan Breck and James More are certainly the two best Highlanders in literature. I am not forgetting Scott: but Scott himself would be the first to say so; his own Highlanders in "Waverley" and "Rob Roy," even in "The Legend of Montrose," are apt to smack a little of romantic convention and the footlights. Then who that has any sense of Scotland or of womanhood can forget the charming contrasted pair, Catriona and Barbara Grant? Or coming to the last great fragment, "Weir of Hermiston," where do we feel the breath of creation moving much more strongly than in the figures of the masterful Weir himself, of his feckless wife, of the four brothers of the Cauldstaneslap, of both the younger and the elder Kirstie? This last personage, the elder Kirstie, with Alan Breck and David Balfour, are the three in Stevenson's work who, as he wrote, took the bit between their teeth (we know so much as to Alan and David from his own words), getting beyond his control and going their own way and not the way he had first planned. Ordinarily, what dominates in his art is not the creative impulse, but the artistic care to strike a given key of narrative in each case and keep it, to subordinate and control every incident, speech, action, or description to the general effect of his design. Take an instance from "The Master of Ballantrae." The tale of the two fraternal enemies, the sterling, charmless younger brother and the accomplished, satanically gifted elder, has reached the point where the elder is trying to seduce to himself the affections of the younger's wife and child:—

"The next sign was a friendship this insidious devil struck up with innocent Miss Katherine; so that they were always together, hand in hand, or she climbing on his knee, like a pair of children. Like all his diabolical acts, this cut in several ways. It was the last stroke to Mr. Henry, to see his own babe debauched against him; it made him harsh with the poor innocent; which brought him still a peg lower in his wife's esteem; and (to conclude) it was a bond of union between the lady and the Master. Under this influence, their old reserve melted by daily stages. Presently there came walks in the long shrubbery, talks in the Belvedere, and I know not what tender familiarity. I am sure Mrs. Henry was like many a good woman; she had a whole conscience, but perhaps by the means of a little winking. For even to so dull an observer as myself, it was plain her kindness was of a more moving nature than the sisterly. The tones of her voice appeared more numerous; she had a light and softness in her eye; she was more gentle with all of us, even with Mr. Henry, even with myself; methought she breathed of some quiet melancholy happiness. To look on at this, what a torment it was for Mr. Henry!"

To any writer possessed by an overruling creative impulse, what an opportunity—what an irresistible call, one would have said—must the situation thus subtly conceived and defined give to work out in full the scenes thus suggested, to detail the words, the tones, the phases and fluctuations of the man's sinister pursuit and the woman's wavering heart and conscience. But no, Stevenson has put the tale into the mouth of the homely house-steward Ephraim MacKellar, and his artistic scheme compels him to tell it no otherwise or more fully or dramatically than MacKellar himself would have done. Remember his own account, in the dedication to Sir Percy and Lady Shelley, of the special problems which the tale presented to him. "The character and fortune," he writes, "of the fraternal enemies, the hall and shrubbery of Durrisdeer, the problem of MacKellar's homespun and how to shape it for superior flights; these were his company on deck in many star-reflecting harbours, ran often in his mind at sea to the tune of slattering canvas, and were dismissed (something of the suddenest) on the approach of squalls."

Let us remember, in any estimate of Stevenson's work in fiction, that what he most admired in the work of others, and aimed at in his own, was the expresssion of the romance and the morality which lie deep together at the soul of nature and experience. "In the highest achievements of the art of words," he writes, "the moral and the romantic interest rise and fall together by a common and organic law. Situation is animated by passion, passion clothed upon with situation." And again, "To embody character, thought, or emotion in some act or attribute that shall be remarkably striking in the mind's eye, this is the highest and hardest thing to do in words." What he meant by the romantic interest, then, is the striking and the memorable and figuratively appropriate scene, situation, or incident: that which the mind and imagination under the given circumstances craves for, or again, in his own words, "the crystallisation of a day-dream." What he meant by the moral interest is the crisis of conflicting motive or passionate tension in a story: and he would have these two interests coincide or come together at one or more culminating moments. He has attempted himself to bring about such coincidence in scenes like that, in Prince Otto, of the change of Seraphina's heart, re-baptised to humility and love under the mystic thrill of starlight and the forest dews: like that where Henry Durie, watching in maddened suspense, again under the thrill of a forest night, the exhumation of his abhorred brother, falls dead at the sight of the dead man's eyes for one ghastly moment opening on him: or, again, that of Archie Weir deciding to sacrifice his love under the appeal of the elder Kirstie as she stands in the midnight chamber transfigured by the passion of her pleading.

This is not the method of the realists. But how often does a writer who attacks life by the realistic method get so near to the heart of it as Stevenson in these passages, or many lighter passages similarly conceived? The realist undertakes a formidable task. His book has to be in some way or another a work of art, after all; he must weave a pattern of some kind out of the materials offered him by life, and to make his pattern he must pick and choose. Sometimes he satisfies his artistic conscience by choosing the grosser and more animal, or let us say bestial, parts of life in quite undue proportion to the rest, and, to vary the pattern, thrusts in masses of undigested notes of crude fact, relevant or irrelevant. These two processes, carried out indeed with remarkable vigour, constitute a large part of the art of that early high priest of realism, M. Zola. There are other and duller kinds of realism than that. There are also other and better kinds; I should like to instance the later works of Mr. Arnold Bennett, as instances of a realism that can faithfully and patiently and inexorably track every daily detail of dull lives in dull surroundings, but yet manages to give them the thrill, the sting, the zest, which in fact all living has, and which art must know how to express or cease to be art at all. And realism of the better kind is no doubt in part a necessary and wholesome reaction against the excesses and weaknesses of false and sentimental romance. Sentimentalism—sugaring, refusing to acknowledge harsh facts—has been the bane and weakness of a vast deal of English art. But neither in life nor in literature was Stevenson the least atom of a sugarer or a sentimentalist. He never shrank from facing the bitter worst of things. His remedy for their bitterness and badness was, in life, courage, and a kind of stoic faith in a hidden underlying and controlling drift—call it drift or guidance—towards good. The inmost of that faith is expressed in the rugged and intensely felt set of verses, of one of which let me remind you:—

> "If to feel, in the ink of the slough
> And the sink of the mire,
> Veins of glory and fire
> Run through and transpierce and transpire,
> And a secret purpose of glory in every part,
> And the answering glory of battle fill my heart;
> To thrill with the joy of girded men,
> To go on for ever and fail and go on again,
> And be mauled to the earth and arise,
> And contend for the shade of a word and a thing not seen with the eyes:
> With the half of a broken hope for a pillow at night
> That somehow the right is the right
> And the smooth shall bloom from the rough:
> Lord, if that were enough?"

If such was in life Stevenson's remedy for the bitterness and badness of things, his remedy in art, and the only true remedy, was that such things should be kindled in the fire of imagination till they burned in a light as vivid and thrilling as the light of beauty, although different—even the light, if need be, of horror. "Your remarks about the ugly," he once wrote to Mr. Henley, "are my eye, ugliness is only the prose of horror. It is when you are not able to write 'Macbeth' that you write 'Thérèse Raquin.' Fashions are external: in any case, and under any fashion, the great man produces beauty, terror, and mirth, and the little man produces cleverness instead of beauty, ugliness instead of terror, and jokes instead of mirth. As it was in the beginning, is now, and shall be ever, world without end. Amen."

So much for his ideas on the romantic element in art. As for his ideas on the moral element, the word of course includes all the critical phases of passion and emotion. But with Stevenson let it be remembered—a Scotchman, a son of the Covenanters—there entered into the conception of a moral crisis more of what in the ordinary sense is called morality than with many writers of fiction. A great deal of modern fiction, whether following the romantic or the realistic method, has dealt with life as though it were made up of nothing but the play and strife of passion and circumstance: as though it were emptied of those other complications that spring from the conflict of duty and desire, the strivings of the conscience, the longing for right, the claims of one right against another. A singular impoverishment of the elements of life and of the themes and opportunities of art! The human conscience is an ancient organ indeed: ancient, but not yet disused. Stevenson at least was the last man to think it so. Perhaps the passage in all his writings which, next to the verses I have already quoted, is most characteristic of his deeper mind is the well-known one from "Pulvis et Umbra":—

"If the first view of this creature, stalking in his rotatory isle, be a thing to shake the courage of the stoutest, on this nearer sight he startles us with an admiring wonder. It matters not where we look, under what climate we observe him, in what stage of society, in what depth of ignorance, burthened with what erroneous morality; by camp-fires in Assiniboia, the snow powdering his shoulders, the wind plucking his blanket, as he sits, passing the ceremonial calumet and uttering his grave opinions like a Roman senator; in ships at sea, a man inured to hardship and vile pleasures, his brightest hope a fiddle in a tavern and a bedizened trull who sells herself to rob him, and he, for all that, simple, innocent, cheerful, kindly like a child, constant to toil, brave to drown, for others; in the slums of cities, moving among indifferent millions to mechanical employments, without hope of change in the future, with scarce a pleasure in the present, and yet true to his virtues, honest up to his lights, kind to his neighbours, tempted perhaps in vain by the bright gin-palace, perhaps long-suffering with the drunken wife that ruins him; in India (a woman this time), kneeling with broken cries and streaming tears, as she drowns her child in the sacred river; in the brothel, the discard of society, living mainly on strong drink, fed with affronts, a fool, a thief, the comrade of thieves, and even here keeping the point of honour and the touch of pity, often paying the world's scorn with service, often standing firm upon a scruple, and at a certain cost, rejecting riches—everywhere some virtue cherished or affected, everywhere some decency of thought and carriage, everywhere the ensign of man's ineffectual goodness:—Ah! if I could show you this! if I could show you these men and women, all the world over, in every stage of history, under every abuse of error, under every circumstance of failure, without hope, without help, without thanks, still obscurely fighting the lost fight of virtue, still clinging, in the brothel or on the scaffold, to some rag of honour, the poor jewel of their souls! They may seek to escape, and yet they cannot; it is not alone their privilege and glory, but their doom; they are condemned to some nobility; all their lives long, the desire of good is at their heels, the implacable hunter."

Viewing human nature in this light, Stevenson was the last man to relieve the children of his creation from the scruples or the problems of conscience. Perfectly unpriggish, often radically unconventional, always tempered by the sense of humour, his treatment of such problems is one of the things by which his work is most humanized and enriched, made most fruitful and life-enhancing to the reader. They find their way even into his lightest and most capricious inventions—remember the case of the young clergyman in the "Rajah's Diamond"—and they nourish and fortify his strongest—it is the commingling of the terrors of conscience with the terrors of the sea that gives the story of the "Merry Men" half its power. Their discussion crops up in unexpected places, but always appropriately to the scene and person, and imparts touches of a serious or a humorous life-wisdom to almost every character in almost every crisis. In one case, that of the stubborn young Lowlander, David Balfour, his irrepressible scruples of honour and conscience become in the end too much even for a brother Scot, the old and worldly-wise Lord-Advocate Grant. Alan Breck has a queer special morality of his own concerning the blind eye to be turned on the identity of a murderer. Some of you may remember the brilliant little fable in which two of the characters of "Treasure Island," Long John and Captain Smollett, are made to step out of the story between two chapters and discuss the nature of good and evil, and the intentions of the author—under whose guise a greater Author is figured—over their pipes. The very climax of the story "A Lodging for the Night" consists in the brilliant, unanswerable argument of François Villon for his own, a thief's, private view of honour and conduct as against those of the self-righteous old nobleman whose life he spared. The whole purport of the latter half of "The Master of Ballantrae" is to exhibit the gradual moral decay of the excellent Henry Durie under the strain of unnatural hatred which he cannot choose but cherish against his brother. (This, let me remark in passing, is a good instance of that refusal to sentimentalise which we have noted in Stevenson. A weaker writer would have shirked this painful issue, and given us in the end a common-place triumph of the just brother over the unjust, the injured over the injurer).

One of the points on which Professor Raleigh dwelt in his lecture here sixteen years ago, was the richness of Stevenson's fiction in this kind of apt and bracing life-wisdom. We find it uttered by his characters now gravely and now lightly; now by the warning Cassandra-lips of the passionate Scottish nurse, now by the houseless poet-thief in the Paris winter night, or again by the broken sailorman, Dick Arblaster, in the Yorkshire harbour, or by the lovable vain and valiant Highlander in the flight across the moors, or the brilliant, ineffectual, self-distrusting Prince in the miller's cottage. There are some, perhaps there are many, of Stevenson's readers who prefer the same quality of life-wisdom gay, or stern, where they find it unmixed with the elements of fiction, and uttered not by the puppets in his tales, however much in character and with whatever grace or quaintness of dialect, but straight from his own lips and with a more direct savour of his own personality. Such readers turn by preference to his essays and letters.

As to the quality of the essays, there has been no doubt amongst sane judges since they began to appear in the "Cornhill Magazine," under the editorship of Sir Leslie Stephen, five and thirty years ago. Whether they revivify and enkindle to our imagination world-old truths like that of the courage of the human race in face of the imminence of death, as in "Aes Triplex"; or whether they plead with grown people for a better understanding of the thoughts of children, to whom their imaginations are so much more true and important than facts, as in "Child's Play"; whether they strike light into the essential relations of art to life, like "The Lantern Bearers" or "A Gossip on Romance," or call up figures out of his own past like those of the Scotch gardener or the shepherd John Tod, or face some of the great problems of good and evil and human destiny like "Pulvis et Umbra" or "A Christmas Sermon," they always hold one by the same charm, the charm of vivid, sincere, and human thinking—thinking done with heart and brain together—the charm of illuminating imagery, of turns of phrase that are a perpetual delight and stimulus alike to our sense of words and our sense of things, and by a harmonious movement and sequence of clauses and sentences which dance one down the argument like the ripple of a full, clear brook—clear, but in its clearness holding more things and deeper than it quite reveals.

They have the further charm that in them Stevenson lets us see much, though never obtrusively or too familiarly, of himself: of a personality, that is, certainly one of the most interesting and inspiring on record. "Divers et ondoyant," in the phrase of Montaigne, beyond other men—more human, in his own phrase, than he found quite convenient—Stevenson seemed to contain within himself a whole troop of singularly assorted characters. He was a man of infinite jest and yet infinite earnest, the one very often a mask for the other. He was a poet, with the poet's senses as well as the poet's mind; a man, for all his lack of health, of strong appetites and unchecked curiosities; a Bohemian, impatient of restraint and shams: and yet a profoundly sincere moralist and preacher and son of the Covenanters after his fashion, deeply conscious of the war within his members, and deeply bent on acting up to the best he knew. Not the negative and cautious, but the active and glowing and enterprising virtues were those on which his heart were set. He was prone to plunge into difficult social passes and ethical dilemmas, which might sometimes more wisely have been avoided, for the sake of trying to behave in them to the utmost according to his own personal sense of the obligations of honour, duty, and kindness. Such a man could not but often expose himself, and make slips and errors in life; but he was always cheerfully ready to pay, and singularly free from sentimentality towards himself and others. At the same time he had a limitless charity of the heart; and, through all his many moods and phases of being, the law of kindness—which is quite a different thing from a weak indulgence—was the one ruling law of his life. From certain faults, from the fretfulness of vanity, from self-seeking, jealousy, or the shadow of the possibility of meanness, he was absolutely free. He had every kind of courage, the active, delighting in danger, and the passive, unshaken in the cheerfulness of endurance. Sympathetic like a woman, engaging like a child, with every faculty of his soul as well as every farthing in his purse always at the service of those in trouble, he was not only the most endearing of companions but the most devotedly warm-hearted and serviceable of friends.

No wonder if readers like to get as closely into touch with such a spirit as they can. Since his death the veils of reticence have been to a great extent lifted, and the character made more fully known, by the publication of selections from his letters, which I daresay many of you know. I am busy preparing a new edition, which will contain about one hundred and fifty new letters and furnish a continuous and pretty complete autobiography in that form. These letters illustrate as nothing else does his surprising play of being and variety of mood. In them he tosses and tumbles about, irresponsibly and rhapsodically, the whole resources of his vocabulary—or his many vocabularies—to communicate the affection or impression, the thought or silence, mood or freak, of the moment. They are the only things left of him that convey any impression of his talk. Some readers may even like them better than the deliberate results of his exquisitely studied literary art. When I open a letter at random and find it beginning, "My bosom's lord" (bosom's lord, you remember, is from the great speech of Romeo on the fatal morning)—when I read, "My bosom's lord is literally swipey with elevation," I catch a far off but a genuine note of that flood of mingled poetry and slang which used to pour from him in speech. Whereas in serious writing he was careful above all things to strike a key of expression and keep it, to use no discords but such as might artfully enhance the harmony.

Of that famous prose style of Stevenson's I do not propose to say much more than has been said by implication already. He began with the resources of a large vocabulary diligently acquired and an ear both naturally sensitive and highly trained for the endlessly variable cadences and harmonies of prose. At first he was prone to aim at verbal sweetness above all things—what he afterwards came to call "fluting." But fortunately his sense of things—of life, nature, and human nature—was not less keen and urgent than his sense of words. Hence after the prentice years, the phrases came to him more and more full-charged and vibrating with life and the images and emotions of life, yet without loss of clarity or beauty or music. To possess in such equal balance a command of things—the material of life—and of words—the material of the writer's art—is one of the rarest gifts of the man of letters. We can all think of writers whose genius is above all for words, who have over words a quite magical command, but whose verbal achievements, when you come to be familiar with them, turn cloying and lose their power over us because the artist has no equal grasp of the material of life—because the words are but faintly impregnated by the power and truth of things. In poetry the late Mr. Swinburne was to my mind the very type of such a writer. In the work of other men, and greater men than Stevenson, the words will sometimes twist and creak and jam themselves under the stress and pressure of vital meanings and overcharged significance, as happens not a little in the case of the writing of both Browning and Meredith. If I were asked what was the first of Stevenson's qualities as a prose writer, I should say this happy balance of power in his dealing with those two worlds so unlike and so mysteriously related, the world of things and the world of words. As contributing to this singular power of expressing what he calls "the incommunicable thrill of things," we can all perceive his infinitely scrupulous choice as well as his fine parsimony of word and phrase. In his mature writing at least he would not only leave standing no redundant word, but none that was trite or effaced or inanimate or a word of all work, or anything but livingly and absolutely expressive of his thought, even if, as sometimes happens, such a word might seem to another mind far-fetched, unusual, or strained.

As a comment on what I have been trying to express, let me read a rough, unmethodical but very suggestive phrase or two taken down by his step-daughter from his table-talk. He has been speaking of the need, for every artist in every work of art, to strike from the outset a particular key and keep it. "Yet I am wrong to liken literature to painting. It is more like music—which is time; painting is space. In music you wind in and out, but always keep in the key; that is, you carry the hearer to the end without letting him drop by the way. It winds around and keeps on. So must words wind around. Organised and packed in a mass, as it were, tight with words. Not too short—phrases rather—no word to spare. There are two kinds of style: the plastic, such as I have just described; the other, the simple placing of words together for harmony. The words should come off the tongue like honey. I began so as a young man; I had a pretty talent that way, I must confess."

Another, indeed an obvious, quality of Stevenson's prose is the amount of poetry it can carry without falling into verse rhythms or any kind of straining of its easy, composed, and ever lightly varying cadences. "A poetical character with a prose talent," such is one of Stevenson's definitions of himself. Now, from the days of the romantic revival in 1800 onward, it has been one of the aims and problems of English prose writers to combine the expression of a full measure of poetic imagination and emotion with the maintenance of the ease, the colloquial composure and simplicity, which Dryden and the other writers of the age of Anne had brought into eighteenth century prose. It is a difficult problem, and in its solution even great writers have often strained the fabric of their style, often raised their prose to too lofty a pitch, and given us the over-eloquent rhythmic period, in a word the purple patch. I think hardly any writer was more successful than Stevenson in avoiding this pitfall. It would be an exercise in which I should delight, and which I think might not be tedious to you, my hearers, to take some of the passages of prose in which Stevenson writes with the vision, the imagination, the emotion of a poet—let us say some of the passages of night-magic from "Prince Otto," or of sea-terror from the "Merry Men," or of moorland solitude tingling with the sense of coming tragedy from "Weir of Hermiston"—and compare them with famous passages from other great nineteenth century writers of poetical prose, De Quincey or Landor or Ruskin. But time fails: so that I can only suggest your doing this for yourselves, and in the meantime quote a phrase or two from his letters showing how easily and naturally the poet in him awakes and speaks.

From Swanston Cottage in the Pentlands, 1875:—

"I have been staying in town and could not write a word. It is a fine strong night, full of wind; the trees are all crying out in the darkness; funny to think of the birds asleep outside on the tossing branches, the little bright eyes closed, the brave wings folded, the little hearts that beat so hard and thick (so much harder and thicker than ever human heart) all stilled and quieted in deep slumber in the midst of this noise and turmoil. Why it will be as much as I can do to sleep in here in my walled room; so loud and jolly the wind sounds through the open window. The unknown places of the night invite the travelling fancy; I like to think of the sleeping towns and sleeping farmhouses and cottages, all the world over, here by the white road poplar-lined, there by the clamorous surf. Isn't that a good dormitive?"

From his chalet at Hyères, 1883:—

"I am very quiet; a person passing by my door half startles me; but I enjoy the most wonderful view into a moonlit garden. By day this garden fades into nothing, overpowered by its surroundings and the luminous distance; but at night and when the moon is out, that garden, the arbour, the flight of stairs that mount the artificial hillock, the plumed blue gum-trees that hang trembling, become the very skirts of Paradise. Angels I know frequent it; and it thrills all night with the flutes of silence."

From a hotel at Royat, 1884:—

"I read for the first time 'Captain Singleton'; it has points; and then I re-read 'Colonel Jack' with ecstasy; the first part is as much superior to 'Robinson Crusoe' as Robinson is to 'The Inland Voyage.' It is pretty, good, philosophical, dramatic, and as picturesque as a promontory goat in a gale of wind."

So much for Stevenson as a poet in prose. Let us end with a few words on his gift as a poet in verse. The highest pitched and most imaginative things he had to say did not, as we have seen, express themselves naturally in that form. The things which it came natural to him to say in the regular rhythms of verse were things nearer at hand, more everyday and simple, and the rhythms in which they clothed themselves to his mind's ear were plain and not much polished. He scarcely claimed for them the character of poetry at all, and was apt to insist on the fact that they must be taken not as song but as speech. And yet he loved them, and many of his readers love them too, and find much that is most characteristic of the man expressed in them very movingly and quite tunefully enough. Of the "Child's Garden" there is no need to speak: or if we speak of it at all let us do so by reminding ourselves of his own words written to a friend when he first held the book in his hand:—

"They look ghastly in the cold light of print; but there is something nice in the little ragged regiment for all that: the blackguards seem to me to smile, to have a kind of childish treble note that sounds in my ears freshly; not song, if you will, but a child's voice."

It is because we hear in them a child's voice truly, speaking by instinct and without effort, that they are and will remain rootedly endeared both to children and to their elders. And that they are so belongs to a special grace in Stevenson's nature, a grace which no man can acquire by prayer or fasting, still less by trying or pretending, be he a post-impressionist painter or another; the grace that the childish soul and the childish vision never perished in him, but were always latent in some corner of his being, quite freshly alive and ready to awake; that there never was a time in his life when he had to say with St. Augustine, "Behold my childhood is dead, but I am alive."

Hear him again about one of his other modes of verse. "You may be surprised," he writes from Hyères to Mr. Henley, "to hear that I am now a great writer of verse, that is, however so. Really, I have begun to learn some of the rudiments of that trade, and have written three or four pretty enough pieces of octosyllabic nonsense, semi-serious, semi-smiling. A kind of prose Herrick, divested of the gift of verse, and you behold the Bard. But I like it." The fact is that some of these sets of meditative octosyllabice are excellent both in substance and form, and would take a high place in any lesser man's work; whether you take one almost wholly smiling, like "The House Beautiful," from Underwoods, or one deeply serious like "The Woodman," written during his labour of clearing the jungle at Vailima. Excellent again, and by no means to be left out of any estimate of his work—next, indeed, I should say, in affectionate grace of feeling and urbane distinction of style to those of Landor—are his complimentary addresses and epistles in blank verse to his friends. The lighter of these again dates from Bournemouth days; the graver and more deeply felt from the days of his exile. In like manner it is the emotions of exile that give a deeper note to his lyrics. The richer and more complicated lyrical measures he never attempted; the impassioned rush of exalted lyrical feeling and music finds no place in his work. But in simpler modes he is sometimes a lyrist that both "plucks at our heart and draws us by the ear" (to borrow a phrase of Meredith). Remember the verses which he wrote to the air of "Wandering Willie" in the Island of Tahiti,

"Home no more home to me, whither must I wander?"

Is the pathos of exile anywhere expressed more poignantly or more hauntingly? Not, in this case, of his own exile, which was voluntary and full of consolations, but that of those exiles who depart hunger-driven and leave their hearthstones cold behind them. Touching his own personal exile, there is a note even of metrical genius in the heart-searching lilt of those lines suggested by a phrase in the letter of an acquaintance, Mr. S. R. Crockett. Writing to him about his old haunts in the Pentland hills, the country of the Cameronian persecutions, Mr. Crockett had said, "The whaups (Scotch for curlews) are still crying above the tombs of the martyrs, your heart remembers how." Stevenson took this up and wrote:—

"Blows the wind to-day, and the sun and the rain are flying,
Blows the wind on the moors to-day and now,
Where about the graves of the martyrs the whaups are crying,
My heart remembers how!
Grey recumbent tombs of the dead in desert places,
Standing stones on the vacant wine-red moor,
Hills of sheep, and the homes of the silent vanished races,
And winds, austere and pure:
Be it granted me to behold you again in dying,
Hills of home! and to hear again the call;
Hear about the graves of the martyrs the peewits crying,
And hear no more at all."

Granted his prayer was not: but before the end came this desire had lost its poignancy, and he was content that his bones should rest on the mountain side above his new island home. But we may well let those be the last of his words to sound in our ears to-night. There are none more fitted to bring near to us the sense of the strong and loving and loyal, the unforgetful and unforgotten, spirit who uttered them.

[S. C.]

WEEKLY EVENING MEETING,

Friday, February 17, 1911.

SIR THOMAS BARLOW, BART., K.C.V.O. M.D. D.C.L. LL.D. F.R.S.,
Vice-President, in the Chair.

PROFESSOR HENRY E. ARMSTRONG, Ph.D. LL.D. F.R.S. *M.R.I.*,
Professor of Chemistry, City and Guilds of London
Central Technical College Institute.

The Stimulation of Digestive Activity.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, February 24, 1911.

Sir William Crookes, O.M. LL.D. D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Professor Jean Perrin, D.Sc., Faculté des Sciences à la Sorbonne, Paris.

Mouvement Brownien et Grandeurs Moléculaires.

Nous observons chaque jour des liquides en équilibre, dont toutes les parties nous semblent immobiles. Si nous y mettons un objet plus dense, cet objet tombe, tout à fait verticalement s'il est sphérique. Enfin, quand il est arrivé au fond du vase, nous savons bien qu'il ne lui arrive pas de remonter spontanément.

Ce sont là des notions bien familières, et pourtant elles ne sont bonnes que pour les dimensions auxquelles nos organes sont accoutumés. Il suffit en effet d'examiner au microscope de petites particules placées dans de l'eau pour voir que chacune d'elles, au lieu de tomber régulièrement, est animée d'un mouvement très vif et parfaitement irrégulier. Elle va et vient, tournoie, monte, déscend, remonte encore, sans tendre aucunement vers le repos. C'est le *mouvement brownien*, ainsi nommé en souvenir d'un de vos compatriotes, le botaniste *Brown*, qui le découvrit en 1827.

Cette découverte si remarquable attira peu l'attention. Les physiciens qui entendaient parler de cette agitation la comparaient, je pense, au mouvement des poussières qu'on voit à l'œil nu se déplacer dans un rayon de soleil, sous l'action des courants d'air qui résultent de petites inégalités dans la pression ou la température. Mais, en ce cas, des particules voisines se meuvent à peu près dans le même sens et dessinent grossièrement la forme de ces courants d'air. Vous allez voir, au contraire, qu'il y a indépendance complète des mouvements browniens de deux particules, même quand elles s'approchent à une distance inférieure à leur diamètre.

Je vais, en effet, essayer de vous montrer ce phénomène, un des plus beaux que nous ait livrés le microscope, et dont nous allons démontrer qu'il est l'image fidèle des mouvements moléculaires, ou mieux, *qu'il est déjà un mouvement moléculaire* (aussi bien que les radiations infra-rouges sont déjà de la lumière).

Vous apercevez en ce moment sur l'écran la cinématographie de l'agitation que possèdent dans l'eau des grains ronds de gomme-gutte (de diamètre un peu inférieur à 1 millième de millimètre). Cette cinématographie a été faite par MM. Victor Henri et Comandon (film Pathé) et m'a été obligéamment prêtée par eux. J'aimerais mieux vous faire directement regarder l'émulsion que voici, en projetant sur un écran la vision animée qu'en donne le microscope, mais le grossissement nécessaire est si fort que cette projection serait trop peu lumineuse pour ce grand amphithéâtre.

Vous voyez qu'on ne peut saisir, en cette agitation profondément irrégulière, aucun courant d'ensemble entraînant en même temps plusieurs particules. Elle ne peut donc être due à des trépidations de la plaque qui porte la gouttelette observée, car ces trépidations, quand on en produit exprès, produisent précisément des courants d'ensemble, que l'on voit simplement se superposer à l'agitation irrégulière des grains. D'ailleurs, le mouvement brownien persiste sur un bâti bien fixe, la nuit, à la campagne, aussi nettement que le jour à la ville, sur une table sans cesse ébranlée par le passage de lourds véhicules (Gouy). De même il ne sert à rien de se donner beaucoup de peine pour assurer l'uniformité de température de la gouttelette. Tout ce qu'on gagne est encore seulement de supprimer des courants d'ensemble parfaitement reconnaissables et sans aucun rapport avec l'agitation irrégulière observée. On ne gagne rien non plus en diminuant extrêmement l'intensité de la lumière éclairante ou en changeant sa couleur (Gouy).

Bien entendu, le phénomène n'est pas particulier à l'eau, mais se retrouve dans tous les fluides, d'autant plus actif que ces fluides sont moins visqueux, et par suite il est surtout vif dans les gaz. Je vous montrais, il y a un instant, la cinématographie de grains de gomme gutte dans l'eau ; en voici une autre que je dois à l'obligéance de MM. Victor Henri et de Broglie, et qui vous montre le mouvement brownien dans l'air des gouttelettes ultra-microscopiques qui forment la fumée de tabac.

Dans un fluide donné, la grosseur des grains importe beaucoup, et l'agitation est d'autant plus vive que les grains sont plus petits. Cette propriété fut signalée par Brown, dès le premier instant de sa découverte. Quant à la nature des grains, elle paraît avoir peu d'influence, si elle en a. Dans un même fluide, deux grains s'agitent de même quand ils ont la même taille, quelle que soit leur substance et quelle que soit leur densité (Jevons, 1869). Et, incidemment, cette absence d'influence de la nature des grains élimine toute analogie avec les déplacements que subit un morceau de camphre jeté sur l'eau.

Enfin—et ceci est peut-être le caractère le plus étrange et le plus véritablement nouveau—le mouvement brownien ne s'arrête jamais. A l'intérieur d'une cellule close (de manière à éviter l'évaporation), on peut l'observer pendant des jours, des mois, des années. Il se manifeste dans des inclusions liquides enfermées dans le quartz depuis des milliers d'années. *Il est éternel et spontané.*

L'ensemble de tous ces caractères impose la conclusion que les grains observés servent seulement à révéler une agitation propre au fluide lui-même, ce qu'ils font d'autant mieux qu'ils sont plus petits ; (c'est ainsi qu'un petit bateau suit plus facilement qu'un gros le mouvement des vagues de la mer). Nous atteignons par là une propriété essentielle de ce qu'on appelle un fluide en équilibre : ce repos apparent n'est qu'une illusion, due à l'imperfection de nos sens, et correspond en réalité à un certain régime permanent d'agitation violente et spontanée.

J'aurais voulu vous dire comment cette agitation perpétuelle, enlève au second principe de la thermodynamique le rang d'une véritete absolulue (Gouy, 1888), et le réduit à une loi de statistique, c'est à dire à l'affirmation de probabilités d'autant plus faibles qu'on les applique à des systèmes plus petits, mais le temps nous est trop mesuré, et je veux tout de suite chercher quelle cause intérieure au fluide peut expliquer le phénomène.

Nous devrons, pour deviner cette cause, faire appel aux hypothèses moléculaires. Vous savez combien elles sont anciennes, puisque déjà *Démocrite* et *Leucippe* pressentirent, il y a plus de vingt siècles, que la Matière est faite de grains indestructibles en mouvement incessant, atomes que le Hasard, ou le Destin, aurait groupés au cours des âges, selon les formes et les corps qui nous sont familiers. Mais la tradition seule nous renseigne sur ces premières théories, et, dans le beau poëme déjà très postérieur où Lucrèce expose l'atomisme d'Epicure, je n'ai rien vu qui indique la façon dont les philosophes grecs ont été conduits à attribuer á la Matière une structure granulaire.

Il me semble que cette hypothèse a pu leur être suggérée par les propriétés si familières des mélanges. En tout cas, il y a dans ces propriétés un argument déjà sérieux, que je veux donner d'abord. Nous disons tous, par exemple, après avoir dissous du sucre dans de l'eau, que le sucre et l'eau subsistent dans le mélange, bien qu'on ne puisse pas distinguer dans l'eau sucrée des parties différentes. De même nous disons qu'il y a de l'oxygène dans l'air et plus généralement nous *reconnaissons* souvent sans hésitation, au travers des propriétés d'un mélange, celles de ses constituants.

Cela se comprendrait facilement, si les corps qui ont formé le mélange y subsistaient comme subsistent à côté les uns des autres la fleur de soufre et la limaille de fer que je mélange sous vos yeux. Vous ne pouvez plus à présent distinguer les grains jaunes des grains noirs, et le tout a pris à vos yeux une coloration grise bien homogène. Vous y reconnaissez le fer, pourtant, même de loin, si j'approche un aimant du ballon qui contient le tout, et cela ne vous surprend pas, car vous savez bien qu'il vous suffirait de vous approcher pour distinguer les grains de fer et de soufre juxtaposés, mais non modifiés. De même, la persistance des propriétés du sucre dans l'eau sucrée s'expliquera très bien si dans cette solution se trouvent simplement mêlées, juxtaposées, de petites particules qui à elles toutes seules, formeraient de l'eau, et d'autres particules qui prises de même seules ensemble formeraient du sucre. Ces particules élémentaires, ces *molécules*, se retrouveraient dans tous les mélanges où l'on reconnaît de l'eau ou du sucre, et leur extrême petitesse nous empêcherait seule de les percevoir individuellement.

De plus, les molécules d'un corps pur comme l'eau, si elles existent, doivent être identiques, sans quoi elles ne réagiraient pas de même aux divers essais de fractionnement, à l'ébullition par exemple, et les fractions successivement séparées ne seraient pas identiques, alors qu'en fait elles le sont. (Dalton.)

Toujours si les molécules existent, nous sommes forcés, pour comprendre les dissolutions ou les diffusions, d'admettre qu'elles s'agitent sans cesse. Versons avec précaution de l'alcool sur de l'eau, ou superposons du gaz carbonique et de l'hydrogène (Berthollet) et abandonnons le système à température bien constante. Vous savez qu'alors les deux fluides se pénètrent progressivement, bien que le plus léger soit au-dessus, et cela ne se pourrait pas si leurs molécules restaient immobiles. Quand nous aurons ainsi vu que les molécules de gaz carbonique diffusent dans l'hydrogène, diffusent dans l'oxygène, diffusent dans l'azote, et ainsi de suite, il nous semblera bien probable qu'elles diffusent aussi dans le gaz carbonique lui-même, et cela revient à dire que les molécules du gaz carbonique sont en mouvement incessant.

Cette agitation des molécules explique aussitôt la pression qu'exercent les fluides sur les parois des récipients qui les enferment, pression qui sera due aux chocs des molécules contre ces parois. Cette explication, développée au dix-huitième siècle par Euler et Bernouilli, entraîne la loi de Boyle et permet de calculer la vitesse moyenne des molécules de divers gaz ; (dans l'air de cette salle, par exemple, les molécules auraient une vitesse moyenne d'environ 500 mètres par seconde). Ce résultat marque le premier succès, dans la Physique moderne, des vieilles hypothèses de l'atomisme grec.

Mais nous sommes encore loin d'une vérification directe, et les mouvements supposés des molécules nous échappent comme le mouvement des vagues de la mer échappe à un observateur trop éloigné. Cependant, si quelque bateau se trouve alors en vue, le même observateur pourra percevoir un balancement qui lui révèlera l'agitation qu'il ne soupçonnait pas. Ne peut-on de même espérer, si des particules microscopiques se trouvent dans un fluide, que ces particules, ces poussières encore assez grosses pour être suivies sous le microscope, soient déjà assez petites pour être notablement agitées par les chocs moléculaires ?

Vous devinez, maintenant, quelle explication nous allons donner au mouvement brownien. Tout granule situé dans un fluide est sans cesse heurté par les molécules voisines. Il n'y a aucune chance pour que ces chocs s'équilibrent exactement : notre granule sera donc irrégulièrement ballotté, et peut-être ce sera là le mouvement brownien.

Cette explication séduisante (Wiener, 1863), a été regardée longtemps comme douteuse. Pour qu'elle s'impose, il faut au moins que la théorie prévoie l'ordre de grandeur du phénomène, dont nous ne savons pas encore s'il n'est pas colossalement plus grand (ou plus petit) que ne le permet notre hypothèse.

Nous allons voir que cette vérification quantitative est possible, mais, pour y arriver, je dois vous rappeler d'abord comment, depuis longtemps déjà, la mesure des viscosités des gaz avait permis de trouver des valeurs probables pour la grandeur et le poids des molécules. Je vous montrerai ensuite que le mouvement brownien, supposé dû aux chocs moléculaires, donne également un moyen d'atteindre ces mêmes grandeurs moléculaires. Si les deux résultats obtenus par des chemins si différents concordent, l'origine du mouvement brownien ne sera plus douteuse.

Si l'on admet l'existence des molécules, on est conduit, pour expliquer les lois de discontinuité de la Chimie (loi des proportions définies, loi des proportions multiples, loi des nombres proportionnels), à admettre avec votre illustre Dalton l'existence d'*atomes* caractéristiques des divers corps simples. Dès lors, en effet, la composition d'une molécule varie nécessairement par bonds discontinus, correspondant à l'entrée (ou à la sortie) de au moins 1 atome. Après quoi, on peut tirer avec précision, des phénomènes de *substitution* chimique, non pas les poids absolus des molécules et des atomes, mais les rapports de ces poids.

Quand par exemple on dissout du calcium dans l'eau, on chasse la moitié seulement de l'hydrogène de cette eau. Cela peut se comprendre si l'on admet que l'hydrogène de chaque molécule de cette eau se compose de deux parties égales. Il faut considérer ces parties comme insécables par tout moyen chimique puisque jamais une autre substitution ne conduit à en supposer plus de deux ; on dira que ce sont des "atomes" d'hydrogène. D'autre part, une molécule d'eau, comme toute masse d'eau, pèse 9 fois autant que l'hydrogène qu'elle contient ; elle pèse donc 18 fois autant qu'un seul atome d'hydrogène. On trouverait par une marche semblable que par exemple une molécule de benzine doit peser 78 fois autant que 1 atome d'hydrogène. Les poids des molécules de benzine et d'eau et de l'atome d'hydrogène sont donc entre eux comme 78, 18, et 1. On trouverait de même, en un grand nombre de cas, les rapports des poids de diverses molécules et de divers atomes.* Il est bien remarquable que toutes ces déterminations sont concordantes, en sorte que par exemple il faudrait compter par milliers le nombre des corps dont l'étude conduirait à dire que l'atome de carbone est 12 fois plus lourd que l'atome d'hydrogène. Cela donne confiance dans la théorie atomique, mais ne nous apprend absolument rien sur l'échelle des grandeurs moléculaires, peut-être proches de nous, peut-être infiniment lointaines.

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* A vrai dire tous ces rapports ne seraient pas encore atteints : par exemple, il n'est pas de substitution qui puisse renseigner sur le poids moléculaire d'un corps simple comme l'oxygène. Nous verrons dans un instant comment la loi d'Avogadro permet de combler cette lacune.

On voit du moins que, pour avoir les poids absolus des divers atomes ou molécules, il suffirait de connaître l'un d'eux. Du même coup nous aurions aussi, comme l'a remarqué Helmholtz, la charge élémentaire que transporte dans l'électrolyse 1 atome d'hydrogène (et plus généralement 1 ion monovalent). Si en effet nous savions ce que pèse 1 atome d'hydrogène, nous saurions combien 1 gramme d'hydrogène contient de ces atomes, et par suite combien chacun transporte dans l'électrolyse, puisque nous savons que 1 gramme d'hydrogène y transporte environ 100,000 coulombs.

Tâchons donc d'atteindre l'un de ces poids élémentaires, celui de la molécule d'oxygène par exemple.

Théorie Cinétique.—Supposons qu'on liquéfie la masse d'oxygène qui occupe 1 litre (pression et température ordinaires). Les n molécules qui s'y trouvent vont venir presqu'au contact, car notre liquide est presque incompressible. Or, on sait que le volume vrai des grains d'un tas de sable vaut à peu près les $\frac{2}{3}$ du volume apparent du tas ; il est donc probable que le volume vrai de nos n molécules n'est pas très inférieur aux $\frac{2}{3}$ du volume du liquide, et, en acceptant cette valeur on ne ferait déjà pas une erreur énorme. Une théorie plus precise, due à Van der Waals, conduit à admettre plutôt une valeur deux fois plus petite, et à écrire :

$$n \times \text{volume de 1 molécule} = \tfrac{1}{3}\ \text{volume du liquide}.$$

D'autre part, à l'état gazeux, dans un volume donné, des molécules en nombre fixé n se heurtent d'autant plus souvent qu'elles sont plus grosses. Ou, si vous préférez, le libre parcours moléculaire moyen L est d'autant plus petit que les molécules sont plus grosses, ont plus de surface. Clausius et Maxwell ont pu calculer cette relation (en admettant les molécules sphériques) et ont trouvé :

$$n \times \text{surface de 1 molécule} = 0{,}7\,\frac{\text{volume du gaz}}{\text{libre parcours moyen}}.$$

Si nous connaissions le libre parcours moyen, nous aurions donc, divisant ces deux équations l'une par l'autre, le rapport du volume $\frac{4}{3}\pi R^3$ à la surface $4\pi R^2$, c'est à dire le rayon de la molécule, et par suite son volume et sa surface. L'une des deux équations, la première par exemple, nous donnerait alors n, nombre de molécules qui forment une masse connue d'oxygène. La masse de la molécule d'oxygène serait donc obtenue.

Mais il faut pour cela connaître le libre parcours moléculaire.

Maxwell a su le déduire de la force qui s'exerce par frottement entre deux couches gazeuses qui glissent l'une sur l'autre avec des vitesses différentes.

Il est facile de montrer l'existence de ce "frottement intérieur," de cette "viscosité." Vous voyez le jet de gaz que j'allume au bout de ce long tube capillaire. Si maintenant je raccourcis le tube (en le coupant), le jet de gaz qui en sort donne une flamme beaucoup plus longue. Le gaz sort donc plus vite par un tube pourtant de même section. Il y avait donc un grand frottement dans la partie que j'ai supprimée, à l'intérieur de laquelle glissaient les unes dans les autres des gaînes gazeuses cylindriques.

L' hypothèse moléculaire explique aisément ce frottement. Pour le comprendre, imaginons deux trains de voyageurs se mouvant sur des rails parallèles avec des vitesses presque égales. Les voyageurs pourraient s'amuser à sauter sans cesse de l'un à l'autre, recevant chaque fois un léger choc. Grâce à ces chocs, les voyageurs tombant sur le train le moins rapide en accroîtraient lentement la vitesse, tandis que, de même, celle du train le plus rapide diminuerait lentement. Les deux vitesses s'égaliseraient donc, comme par un frottement, si on ne maintenait leur différence constante. Il en sera ainsi pour deux couches gazeuses qui glissent l'une sur l'autre, puis qu'elles se bombardent sans cesse, en s'envoyant l'une à l'autre des molécules.

L'analyse mathématique de cette idée si simple, faite par votre compatriote Maxwell, a montré que la force de frottement par centimètre carré entre deux couches gazeuses distantes de 1 centimètre et ayant une vitesse relative de 1 centimètre par seconde (force qui vérifie la viscosité) vérifie à peu près l'équation :

$$\text{viscosité} = \tfrac{1}{3}\ \text{densité} \times \text{libre parcours} \times \text{vitesse moléculaire.}$$

La densité du gaz et la viscosité sont mesurables. Nous aurons donc le libre parcours si nous pouvous déterminer la vitesse moléculaire (moyenne).

Ce dernier effort est facile. La pression du gaz, nous l'avons déjà dit, s'explique par les chocs incessants des molécules sur les parois, et ces chocs sont d'autant plus efficaces que chacune d'elles est plus lourde et plus rapide. Le calcul précis, dû à Bernoulli, donne :

$$\text{pression} = \tfrac{1}{3}\ \text{densité} \times \text{carré de la vitesse moléculaire.}$$

Ce qui détermine la vitesse cherchée. Appliquant à l'oxygène, on trouve environ 500 mètres par seconde à la température ordinaire : les molécules que nous respirons sont aussi rapides que des balles de fusil.

Nous n'avons plus alors qu' à remonter de proche en proche : nous aurons le libre parcours moyen (1 dix-millième de millimètre pour l'air, dans les conditions ordinaires), puis le diamètre moléculaire (une molécule d'oxygène est à un homme à peu près comme un homme est au soleil), puis le nombre n de molécules par litre (environ 30 milliards de trillions), puis enfin le poids de la molécule d'oxygène qui nous donnera, nous l'avons vu, tous les autres poids de molécules ou d'atomes.

On trouve ainsi, l'erreur possible étant largement de 30 %, que le poids de l'atome d'hydrogène vaut seulement le trillionième du trillionième de $1^{\text{gr}}6$, soit :

$$\frac{1^{\text{gr}}6}{1.000.000.000.000.000.000.000.000}$$

ou, plus brièvement, $1^{\text{gr}}6.10^{-24}$. Quant à la charge élémentaire, elle serait environ 5.10^{-10}, soit environ le milliardième de la charge que peut déceler un électroscope ordinaire.

Je vais maintenant vous dire comment j'ai pu, grâce au mouvement brownien, donner d'autres moyens pour obtenir ces grandeurs moléculaires.

La loi d'Avogadro.—Je vous indiquais tout à l'heure comment l'étude des substitutions chimiques donne par exemple le rapport des masses des molécules de benzine et d'eau, qui doivent être entre elles comme 78 et 18. Deux masses de benzine et d'eau qui sont l'une à l'autre comme 78 est à 18 contiennent donc autant de molécules l'une que l'autre. Or, si nous mesurons les volumes occupés à l'état gazeux, par ces deux masses, à la même température et à la même pression, nous trouvons que ces volumes sont égaux. Ce n'est pas là un hasard, et de façon tout à fait constante on trouve que deux masses gazeuses qui doivent contenir autant de molécules occupent le même volume quand elles ont la même température et la même pression. C'est la loi d'Avogadro, que l'on peut énoncer comme ceci :

*Dans l'état gazeux, des nombres égaux de molécules de substances différentes, enfermées dans des volumes égaux à la même température, y produisent des pressions égales.**

Vous savez par ailleurs (loi de Boyle), que la pression ainsi développée varie en raison inverse du volume occupé.

Ces lois des gaz ont été étendues par Van 't Hoff aux solutions diluées. Il faut, bien entendu, considérer alors, non la pression totale exercée sur les parois, mais seulement la part de cette pression qui est due aux chocs des molécules dissoutes, portion appelée pression osmotique du corps dissous (et que l'on ne peut mesurer que si l'on sait réaliser une paroi " semi-perméable " qui arrête les molécules du corps dissous, mais non pas celles du dissolvant). Les lois d'Avogadro et de Boyle deviennent alors :

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* La loi d'Avogadro, une fois bien établie, nous donnera par extrapolation des poids moléculaires non atteints par la méthode des substitutions chimiques. Par exemple, quand nous aurons trouvé que 32 parties d'oxgène ou 2 parties d'hydrogène exercent dans le même volume (et à la même température), la même pression que 18 parties de vapeur d'eau, nous saurons que les masses de ces trois sortes de molécules sont entre elles comme 32, 2, et 18.

Dans l'état dilué (gazeux ou dissous), des nombres égaux de molécules quelconques, enfermées dans des volumes égaux à la même température y produisent la même pression. Cette pression varie en raison inverse du volume occupé.

Ces lois sont indifféremment applicables à toutes les molécules, grosses ou petites. Les lourdes molécules de sucre ou de sulfate de quinine ne comptent ni plus ni moins que la molécule d'hydrogène. Pourtant la molécule de sucre contient déjà 45 atomes, celle de sulfate de quinine en contient plus de 100, et l'on en trouverait aisément de plus complexes qui obéissent aux lois de Van 't Hoff (ou aux lois de Raoult, qui en sont la conséquence).

N'est-il pas alors supposable qu'il n'y ait aucune limite de grosseur pour l'assemblage d'atomes qui vérifie ces lois ; n'est-il pas supposable que même des POUSSIÈRES *déja visibles les vérifient encore exactement, en sorte qu'un granule agité par le mouvement brownien ne compte ni plus ni moins qu'une molécule d'hydrogène en ce qui regarde l'action de ses chocs sur une paroi qui l'arrête ? Ou, plus brièvement, est-il déraisonnable de penser que les lois des gaz parfaits s'appliquent encore aux* EMULSIONS *faites de grains visibles ?*

J'ai fait ce postulat, et c'est dans ce sens que j'ai cherché une expérience cruciale qui décidât de l'origine du mouvement brownien, et du même coup donnât ou retirât une base expérimentale aux théories moléculaires.

Voici celle qui m'a paru le plus simple :

La Repartition d'Equlibre, dans une Colonne Verticale de Matière Diluée.—Vous savez que l'air est plus raréfié sur les montagnes qu'au niveau de la mer, et que, de façon générale, une colonne de gaz s'écrase sous son propre poids, l'état d'équilibre résultant d'un antagonisme entre la pesanteur, qui fait tomber les molécules, et leur agitation, qui les éparpille sans cesse.

La loi de raréfaction, indiquée par Laplace (pour montrer comment l'altitude peut être donnée par le baromètre), est une conséquence nécessaire de la loi de Boyle, et peut s'énoncer de la manière suivante :

Chaque fois qu'on s'élève d'une même hauteur, la densité se trouve divisée par un même nombre. Ou, plus brièvement : *des bonds verticaux égaux s'accompagnent de raréfactions égales.*

Par exemple, dans de l'air à la température ordinaire, la densité diminue de moitié chaque fois qu'on s'élève de 6 kilomètres (à partir de n'importe quel niveau).

Mais l'élévation qui entraîne une raréfaction double ne serait pas la même dans de l'hydrogène. Un raisonnement simple montre que la façon dont influe la nature du gaz est une conséquence nécessaire de la loi d'Avogadro et peut s'énoncer comme ceci :

Les élévations qui entraînent une même raréfaction, pour deux gaz *différents (à la même température) sont dans le rapport inverse des poids des molécules de ces deux gaz.*

Par exemple, si dans de l'oxygène à 0° il faut s'élever de 5 kilomètres pour que la densité devienne 2 fois plus faible, il faudrait s'élever de 80 kilomètres dans de l'hydrogène à 0° parce que la molécule d'hydrogène est 16 fois plus légère que la molécule d'oxygène.

Vous voyez ici (Fig. 1), un diagramme représentant trois éprouvettes verticales gigantesques (la plus grande à 300 kilomètres de haut), où l'on aurait mis en même nombre, des molécules d'hydrogène, des molécules d'hélium, et des molécules d'oxygène. A température supposée uniforme, les molécules se répartiraient comme l'indique le diagramme, d'autant plus remassées vers le bas qu'elles sont plus pesantes.

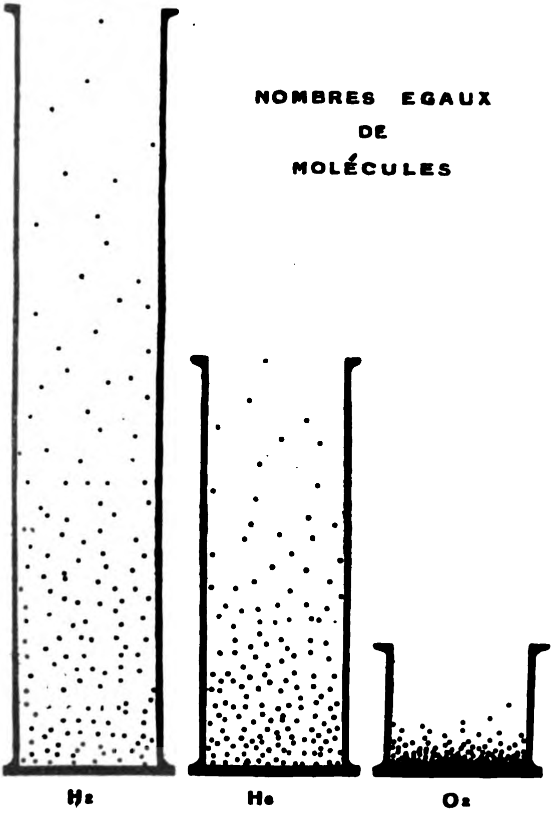

FIG. 1.

Or, nous avons été conduits à penser que les lois des gaz parfaits s'appliquent peut-être aux émulsions. Si cela est vrai, et si nous réalisons une émulsion où les grains soient égaux, la distribution de la matière dans une colonne verticale de cette émulsion devra être la même que pour un gaz. En d'autres termes, une fois atteinte la distribution d'équilibre, des élévations égales s'accompagneront de raréfactions égales. Mais s'il faut s'élever seulement de $\frac{1}{20}$ de millimètre, c'est à dire 100 millions de fois moins que dans de l'oxygène, pour que la concentration devienne 2 fois plus faible, on devra penser que chaque grain de l'émulsion pèse 100 millions de fois plus que 1 molécule d'oxygène. On aura donc ce dernier poids si on arrive à peser le grain, qui jouera le rôle d'un relai entre les dimensions moléculaires et celles qui sont à notre échelle.

Il est bien entendu que le poids efficace du grain sera la différence entre son poids réel et la poussée qu'il subit dans le liquide (conformément au principe d'Archimède). Si les grains étaient plus légers que le liquide intergranulaire, c'est dans les couches supérieures qu'ils s'accumuleraient (des abaissements égaux entraînant des raréfactions égales). Ils se répartiraient uniformément s'ils avaient la même densité que le liquide.

Réalisation Pratique.—J'ai utilisé, pour réaliser ces expériences, les émulsions que l'on obtient en précipitant par l'eau les solutions alcooliques des résines. On obtient ainsi (comme vous le voyez), pour la *gomme-gutte* un beau liquide jaune, pour le *mastic* un liquide blanc comme du lait. Le microscope révèle dans ces liquides la résine précipitée sous forme de grains ronds *solides*, qui ne s'agglutinent pas quand les hasards du mouvement brownien les amènent au contact (ce qui arrive pour d'autre résines, qui donnent des grains mous).

Mais les diamètres de ces grains sont très variés et il me fallait les trier de façon à réaliser des émulsions où tous les grains fussent à peu près de la même taille. Le procédé que j'ai employé peut se comparer au fractionnement d'un mélange liquide par distillation. De même que, pendant la distillation, les parties d'abord vaporisées sont relativement plus riches en constituants volatils, de même pendant la centrifugation d'une émulsion les parties d'abord sédimentées seront relativement plus riches en gros grains. Vous concevez ainsi qu'il soit facile de trouver une technique permettant de trier les grains selon leur taille par centrifugation fractionée. L'opération est d'ailleurs longue (j'y ai passé plusieurs mois), mais enfin ne demande que de la patience.

Une fois réalisée une émulsion à grains suffisamment uniformes, il faut déterminer le poids moyen de ces grains. Leur densité se mesure comme pour une poudre quelconque (le poids de résine en suspension dans le flacon employé se dose par simple dessiccation). Toute la difficulté est donc d'obtenir le diamètre. Il semble indiqué de le mesurer au microscope par le procédé de la chambre claire, au

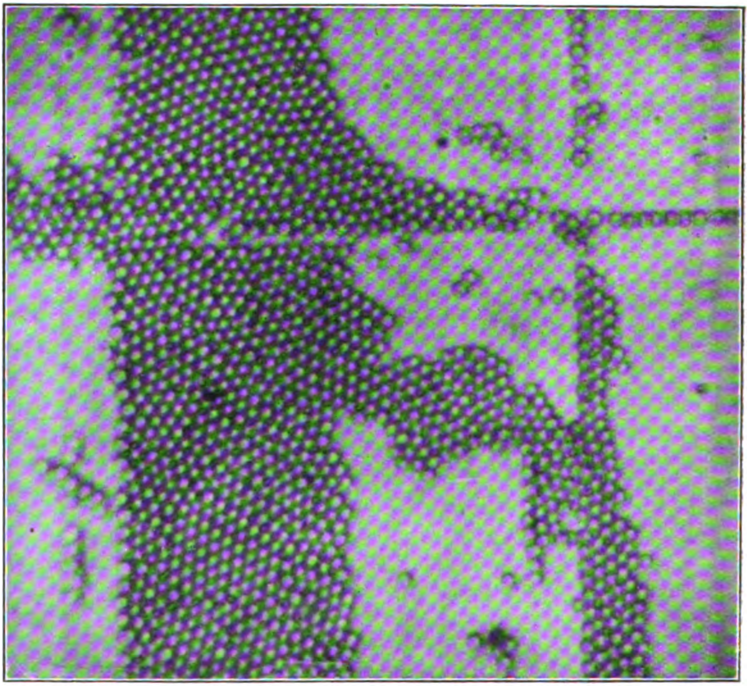

FIG. 2.

moyen d'un micromètre objectif. Mais les grains utilisables sont si petits qu'on ferait facilement ainsi des erreurs de 20 pour 100 et plus. Un procédé presque aussi direct consiste à laisser évaporer une gouttelette d'émulsion sur le micromètre objectif : il se trouve, comme vous pouvez voir ici sur une projection (Fig. 2), que les grains se disposent alors en alignements réguliers, dont cette fois on peut mesurer la longueur avec assez de précision. Divisant cette longueur par le nombre de grains, on a le diamètre.

Un autre procédé, plus long, mais plus précis, consiste à compter combien il y a de grains dans un volume connu d'émulsion titrée, ce qui donne la masse d'un grain, et, par suite, son rayon, puisque l'on connaît sa densité. J'ai utilisé pour cela le fait, accidentellement observé, qu'en milieu très faiblement acide les grains de gomme-gutte se collent sur le verre. A distance notable des parois, le mouvement brownien n'est pas modifié ; mais, sitôt que les hasards de ce mouvement amènent un grain au contact d'une paroi, ce grain s'immobilise. L'émulsion s'appauvrit ainsi progressivement, et, après quelques heures, tous les grains qu'elle contenait sont fixés. On peut alors compter à loisir tous ceux qui proviennent d'un cylindre de base arbitraire (mesurée à la chambre claire).

Enfin un troisième procédé, que je ne vous explique pas en détail, se fonde sur l'observation du temps nécessaire pour que la partie supérieure d'une colonne verticale d'émulsion (haute de plusieurs centimètres) se clarifie sur une hauteur donnée. Ce temps, nécessaire aux grains pour descendre, en moyenne, de cette hauteur, donne le diamètre par application de la loi de chute d'une sphère dans un fluide visqueux (Stokes).

Ces trois procédés concordent, et cela est nécessaire pour que nous puissions avoir confiance dans la précision de mesures qui portent sur un rayon inférieur au millième de millimètre.

Dispositif d'Observation.—Il nous faut maintenant réaliser un dispositif qui permette de connaître la répartition d'équilibre en fonction de la hauteur. Pour cela, dans une cuve plate, dont la profondeur est $\frac{1}{10}$ de millimètre, on place une goutte d'émulsion qu'on aplatit aussitôt par un couvre-objet qui ferme la cuve et dont on paraffine les bords pour éviter l'évaporation.

On peut, comme l'indique la figure, disposer cette cuve verticalement, le corps du microscope étant horizontal, et donner à la colonne verticale où peut se distribuer l'émulsion une hauteur de quelques millimètres. On voit alors les grains s'accumuler dans les couches inférieures et tendre vers une distribution d'équilibre (pratiquement atteinte après un ou deux jours) où la raréfaction progressive en fonction de la hauteur est manifeste, comme vous pouvez le voir sur une projection, qui rappelle de façon évidente la loi de raréfaction des gaz pesants.

Mais, si petits que soient nos grains, ils sont encore tellement lourds que la raréfaction en est extrêmement rapide et que la hauteur sur laquelle on peut utilement faire des mesures est inférieure au dixième de millimètre.

On peut donc, comme on voit également sur la figure, laisser la cuve horizontale, le corps du microscope étant vertical, auquel cas il suffit d'un quart d'heure environ pour que la répartition d'équilibre s'établisse. L'objectif du microscope, très grossissant, a une faible profondeur de champ, et on ne peut voir nettement, à un même instant, que les grains situés dans une tranche horizontale très mince dont l'épaisseur est peu supérieure au millième de millimètre. Si l'on élève ou abaisse le microscope, on voit les grains d'une autre tranche. La distance de ces deux tranches se déduit du déplacement vertical lu sur la vis du microscope ; le rapport des nombres de grains aperçus donne donc la raréfaction qui correspond à une élévation connue. C'est sous cette forme que j'ai d'abord fait ces expériences.

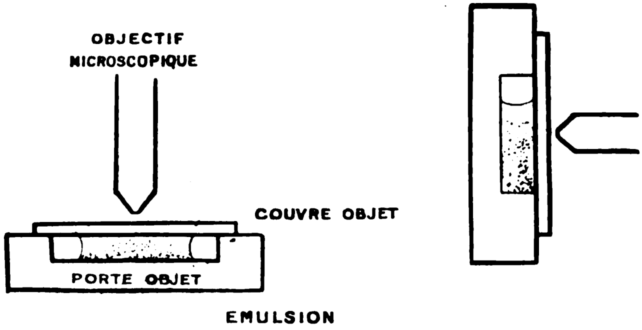

Fig. 3.

Je n'étais pas sûr qu'il y aurait la moindre raréfaction, et pas davantage sûr que, au contraire, tous les grains ne se ramasseraient pas tout contre le fond. Or, j'ai vu s'établir un régime permanent de raréfaction bien régulière. Cette raréfaction est particulièrement frappante quand, tenant l'œil fixé sur la préparation, on soulève rapidement le microscope au moyen de sa vis micrométrique. On voit alors les grains se raréfier rapidement, comme fait l'atmosphère autour d'un aérostat qui s'élève.

Reste à faire des mesures précises. Quand on aperçoit dans le champ quelques centaines de grains qui s'agitent en tous sens ou disparaissent en même temps qu'apparaissent de nouveaux grains, on renonce vite à les compter. Heureusement, on peut faire des photographies instantanées des diverse tranches, et compter ensuite à loisir sur les clichés les nombres de grains présents dans ces tranches

Il est facile de vérifier ainsi que des élévations égales s'accompagnent de raréfactions égales. Par exemple, pour des grains de rayon égal à 0,212 μ, trois bonds successifs de 30 μ abaissaient la concentration sensiblement à la moitié, au quart et au huitième de sa valeur. Pour d'autres grains, de rayon égal à 0,367 μ, une élévation de 6 μ suffisait pour rendre la densité deux fois plus faible environ. Vous voyez ici précisément un dessin obtenu en plaçant juste au-dessous l'une de l'autre 5 coupes faites à 6 μ de distance dans cette émulsion (Fig. 4).

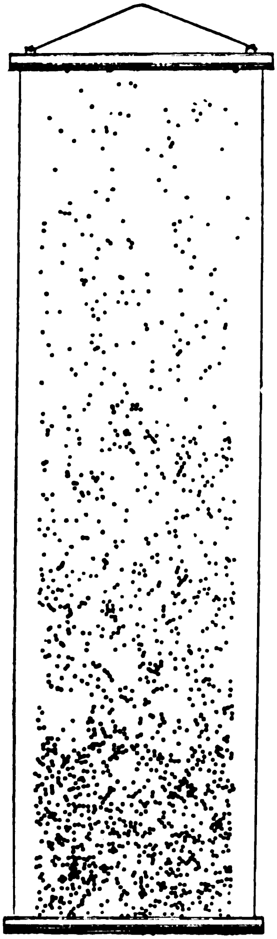

Fig. 4.

Pour obtenir la même raréfaction dans de l'air, nous avons vu qu'il faudrait faire un bond de 6 kilomètres, un milliard de fois plus grand. Si notre théorie est bonne, le poids d'une molécule d'air serait donc le milliardième du poids que pèse, dans l'eau, un de nos grains. Le poids de l'atome d'hydrogène s'obtiendra de même et tout l'intérêt de la question est maintenant de savoir si nous allons ainsi retrouver les nombres auxquels avait conduit la théorie cinétique.

Aussi, j'ai ressenti une vive émotion quand, dès le premier essai, j'ai en effet retrouvé ces nombres que la théorie cinétique avait obtenus par une voie si profondément différente. J'ai d'ailleurs varié autant que j'ai pu les conditions de l'expérience. Par exemple, la masse de mes grains a pris une série de valeurs échelonnées entre des limites qui sont entre elles comme 1 et 40 ; j'ai changé la nature de ces grains, opérant sur diverses résines (en particulier sur le mastic) ; par addition de glycérine, j'ai accru dans le rapport de 120 à 1 la viscosité du liquide intergranulaire, changeant en même temps la nature de ce liquide ; enfin j'ai notablement changé la densité apparente des grains, qui, dans l'eau, a varié du simple au quintuple, et qui est devenue négative pour de la gomme-gutte dans de la glycérine à 10 % d'eau (dans ce dernier cas, les grains, plus légers que la liquide, s'accumulaient dans des couches supérieures).* Toujours j'ai obtenu des résultats concordants, indiquant pour l'atome d'hydrogène un poids très voisin du poids $1{,}6 . 10^{-24}$ donné par la théorie cinétique.

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* Tout récemment enfin, sous ma direction, M. Bruhat a fait varier la température de $-10°$ à $+58°$, et a encore ainsi retrouvé le même poids.

Je ne pense pas que cette concordance puisse laisser de doute sur l'origine du mouvement brownien. Pour comprendre à quel point elle est frappante, il faut encore une fois songer qu'avant expérience on n'eût pas osé certifier que la chute de concentration ne serait pas négligéable sur la faible hauteur de quelques microns, et que, par contre, on n'eût pas osé davantage affirmer que tous les grains ne se rassembleraient pas dans le voisinage immédiat du fond de la cuve. La première éventualité donnerait une valeur nulle, et la seconde une valeur infinie pour le poids de l'atome d'hydrogène. Que l'on soit tombé, avec *chaque* émulsion, dans *l'immense* intervalle qui semblerait donc *à priori* possible, précisément sur une valeur si voisine du nombre prévu, ne paraitra sans doute pas l'effet d'une rencontre fortuite.

Mais il y a plus : tandis que la théorie cinétique en raison même des simplifications qui permettent ses calculs, donne, même à partir d'expériences parfaites, des résultats d'une approximation incertaine, les nombres donnés par les émulsions correspondent à une mesure véritable dont rien ne limite la précision. Par ce moyen nous saurons vraiment peser les atomes, et non pas seulement estimer grossièrement leur poids.

Les deux séries d'expériences que je regarde comme le plus sûres m'ont ainsi donné pour le poids de l'atome d'hydrogène (après dénombrement d'environ 30.000 grains), la valeur

$$\frac{1{,}47}{1.000.000.000.000.000.000.000.000}$$

ou, plus brièvement, $1^{\mathrm{gr}}{,}47 \cdot 10^{-24}$.

Les autres grandeurs moléculaires s'ensuivent. Par exemple, vous verriez aisément que dès lors il doit y avoir dans chaque centimètre cube d'air (conditions normales) 31 milliards de milliards de molécules, et que la charge élémentaire, ou électron, vaut $4{,}2 \cdot 10^{-10}$ (unités électrostatiques C.G.S.).

Les Grandeurs moléculaires peuvent se déduire du Degré d'Agitation.—Nous avons donc pu étendre, avec un plein succès, les lois des gaz aux émulsions, et du même coup obtenir les poids absolus des atomes. Il est remarquable que, dans ces mesures, nous n'avons pas eu à nous occuper de l'activité du mouvement brownien. Les lois des gaz sont vérifiées par les émulsions aussi bien dans la glycérine, où le mouvement brownien est à peine perceptible, que dans l'eau où il est très vif, la seule différence étant que la répartition d'équilibre se réalise beaucoup plus vite dans l'eau que dans la glycérine.

D'autre part, ce mouvement brownien, étudié en lui-même, va nous donner un nouveau moyen d'atteindre les grandeurs moléculaires, moins intuitif peut-être que la précédent, mais aussi précis, et qui a été indiqué par *Einstein* en un très beau travail théorique (1905).

Sans chercher à suivre le trajet infiniment enchevêtré que décrit un grain pendant un temps donné, Einstein considère seulement son *déplacement* pendant ce temps, c'est à dire le segment rectiligne qui joint son point de départ à son point d'arrivée, et montre que, si certaines hypothèses fondamentales de théorie cinétique sont exactes, la connaissance de la valeur *moyenne* de ce déplacement permet d'obtenir les grandeurs moléculaires.

Je ne puis donner ici le détail de ce raisonnement, et me borne à dire que la principale hypothèse utilisée par Einstein consiste à supposer non seulement, comme on faisait déjà, qu' à la même température toutes les molécules ont la même énergie moyenne de mouvement, mais que même un grain déjà perceptible a encore la même énergie moyenne de mouvement. La formule d'Einstein donne alors cette énergie moléculaire moyenne, dès que l'on connait le déplacement moyen en un temps donné, dans un liquide ayant une viscosité connue, de grains sphériques de rayon connu. L'énergie moléculaire pourra donc s'obtenir par cette voie, et du même coup les masses des diverses molécules, car nous avons vu que l'on sait ce que doivent être leurs vitesses, pour que leurs chocs sur les parois expliquent les pressions observées.

Einstein fit remarquer que l'ordre de grandeur des déplacements observés concordait parfaitement avec la théorie, et laissa aux expérimentateurs le soin d'une comparaison plus précise. Puisque j'avais des grains ronds de diamètre connu, je me trouvais en état de faire cette comparaison, comme me le fit observer M. Langevin en me signalant la théorie d'Einstein que j'ignorais. Je confiai d'abord ces mesures à M. Chaudesaignes pour la gomme-gutte, à M. Dabrowski pour le mastic, puis je fis moi-même un grand nombre de pointés. Le résultat fut décisif et montra l'exactitude rigoureuse de la formule d'Einstein, le poids de l'atome d'hydrogène calculé par cette méthode se trouvant égal à $1,45 . 10^{-24}$ grammes, pratiquement identique au nombre que m'a donné la répartition en hauteur des émulsions.

Vous apercevez sur la projection ci-jointe, à un grossissement tel que 16 divisions du quadrillage représentent 50 microns, trois dessins obtenus en traçant les segments qui joignent les positions consécutives d'un même grain de mastic, de rayon égal à $0^{\mu},52$, pointé de trente en trente secondes, à la chambre claire. C'est en faisant la moyenne de pareils segments qu'on peut vérifier la formule d'Einstein.

Ces dessins ne vous donnent au reste qu'une idée très affaiblie du prodigieux enchevêtrement de la trajectoire réelle. Si, en effet, on faisait des pointés de seconde en seconde, chacun de ces segments rectilignes se trouverait remplacé par un contour polygonal de trente côtés relativement aussi compliqué que le dessin ici reproduit, et ainsi de suite.

Pour varier les conditions, j'ai cherché et j'ai réussi à préparer des grains beaucoup plus gros que ceux qui m'avaient servi jusqu'alors. Pour cela, j'ai fait arriver lentement de l'eau, par un entonnoir à pointe effilée, sous une solution alcoolique de mastic. Les grains qui se forment alors dans la zone de passage ont couramment un diamètre d'une douzaine de microns, diamètre qui se mesure directement à la chambre claire, et sont donc environ 100.000 fois plus lourds que les plus petits de ceux qui m'avaient servi. Pour que ce poids ne les maintienne pas sans cesse au contact immédiat du fond, je les ai observés dans une solution d'urée qui a presque leur densité. J'ai ainsi constaté que la formule d'Einstein s'applique encore. Enfin j'ai pu éprouver une autre formule, également déduite par Einstein des hypothèses moléculaires, et qui porte sur la rotation moyenne d'un grain en un temps donné. Cette formule prévoyant environ 100° de rotation par seconde pour des grains de l'ordre du micron, semblait difficilement vérifiable. Mais heureusement j'avais de gros grains ; en raison de la grosseur de ces grains, et parce que certains d'entre eux contiennent de petites inclusions qui servent de point de repère, *j'ai pu constater et mesurer leur rotation*. Les mesures, assez peu précises encore, m'ont donné $1{,}56 . 10^{-24}$ comme poids de l'atome d'hydrogène, en sorte que la légitimité de ce nouveau moyen d'atteindre aux grandeurs moléculaires n'est pas douteuse.

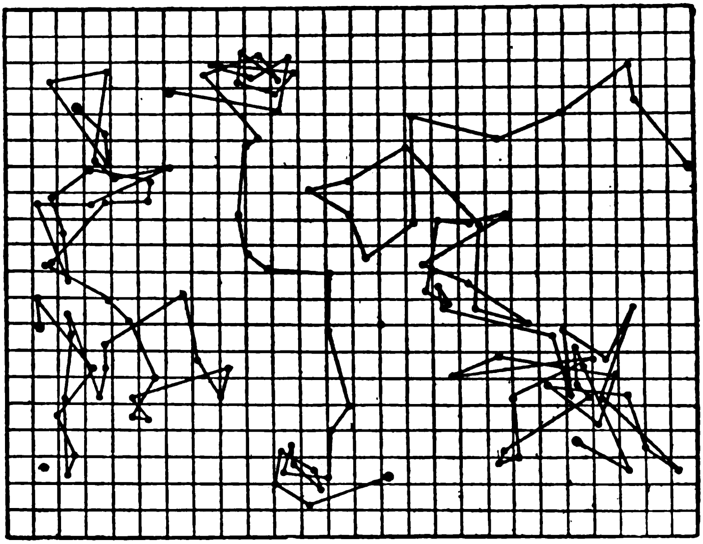

FIG. 5.

Je n'ai plus rien à vous dire sur mes recherches, mais vous me permettrez au moins de vous signaler d'autres méthodes, toutes récentes, qui ont également permis d'atteindre aux grandeurs moléculaires. Une d'elles, due à Lord Rayleigh, donne le nombre de molécules contenues dans une colonne d'air de masse calculable, d'après la diffraction qu'elles font subir à la lumière solaire, et qui, favorisant les couleurs plus réfrangibles, serait l'origine du *bleu du ciel.* Pour avoir les grandeurs moléculaires, il suffit ainsi de mesurer en même temps, pour une même radiation, l'éclat du soleil et dans une direction connue, celui du ciel. Les mesures (Bauer et Moulin), qui ne comportent pas une grande précision, à cause de la part difficilement calculable que les rayons réfléchis sur le sol prennent dans l'illumination du ciel, donnent pourtant un très bon ordre de grandeur, indiquant, à 50% près peut-être, $1{,}6 . 10^{-24}$ pour le poids de l'atome d'hydrogène.

Une autre méthode, plus précise, se fonde sur la mesure des charges électriques transportées par des gouttelettes microscopiques. Ces charges sont généralement petites et sont des multiples entiers d'un même atome d'électricité, par là directement mesurable, qui se trouve égal à ce que peut charrier un atome d'hydrogène dans l'électrolyse. Comme on sait ce que transporte 1 gr. d'hydrogène, on a encore par là le poids de l'atome. Cette méthode, imaginée par Townsend et Sir J. J. Thomson, perfectionnée par H. A. Wilson et surtout par Millikan (qui a réussi à prouver de la façon la plus nette la structure atomique de l'électricité), donne actuellement des chiffres compris entre $1{,}4 . 10^{-24}$ et $1{,}7 . 10^{-24}$.

Un autre groupe de mesures a son origine dans les propriétés des corps radioactifs. Vous savez que certains de ces corps émettent des rayons α qui excitent la phosphorescence du sulfure de zinc. Sir William Crookes, en examinant à la loupe cette phosphorescence, la vit se résoudre en un fourmillement de scintillations éteintes aussitôt qu'apparues. Les rayons qui les excitent sont matériels, car partout où ils pénètrent, on constate bientot la présence d'hélium (Rutherford) : ils sont probablement formés par des atomes d'hélium chargés positivement. Sir W. Crookes a supposé que chaque scintillation marquait l'arrêt de l'un de ces atomes qui sont lancés à la vitesse prodigieuse de 20,000 kilomètres par seconde. Si l'on compte alors les scintillations dues en un temps donné à une masse connue de corps radioactif, et si l'on mesure le volume d'hélium dégagé pendant le même temps, on saura combien ce volume contient d'atomes et les grandeurs moléculaires s'ensuivront. On peut également (je n'insiste pas sur ce point, simplement pour abréger) mesurer la charge positive rayonnée, ou la fraction détruite du corps radioactif. Ces admirables experiences conduisent, pour le poids h de l'atome d'hydrogène, à des nombres compris entre $1{,}4 . 10^{-24}$ et $1{,}6 . 10^{-24}$.

Je ne puis enfin que vous indiquer l'existence des belles théories par lesquelles Planck et Lorentz ont relié les grandeurs moléculaires à la répartition de l'énergie dans le spectre d'un corps incandescent. Les mesures, qui pourront devenir très précises, donnent dès maintenant pour *h* le poids $1,6.10^{-24}$.

Je pense que vous serez frappés de la diversité des routes qui conduisent ainsi à un même résultat. Si nos hypothèses étaient fausses, chacun de ces phénomènes pourrait donner n'importe quel jeu de valeurs échalonnées entre zéro et l'infini. Par exemple, en ce qui regarde le mouvement brownien, la mesure des déplacements moyens donnerait des nombres différents avec de gros grains et avec de petits grains. Ces valeurs n'auraient d'ailleurs aucun rapport avec celles, également variables, que donneraient les répartitions en hauteur. Qu'il y ait convergence, non seulement à l'intérieur de chaque méthode, mais entre toutes les méthodes; que des phénomènes aussi profondément différents que la viscosité des gaz, le mouvement brownien, le bleu du ciel, l'électrisation de gouttelettes, la radioactivité, la répartition d'énergie dans le spectre, expriment tous la discontinuité de la matière et imposent tous les mêmes grandeurs pour les éléments de cette matière, *c'est cela qui est la démonstration de la réalité objective des molécules.* Peut-être aussi trouverez-vous que ces concordances merveilleuses donnent un assez bon exemple des satisfactions que l'amour des belles choses trouve dans l'étude des sciences.

[J. P.]

WEEKLY EVENING MEETING,

Friday, March 3, 1911.

THE RIGHT HON. SIR HENRY BURTON BUCKLEY, P.C. M.A.,
Vice-President, in the Chair.

F. A. DIXEY, ESQ. M.A. M.D. F.R.S., Fellow of Wadham
College, Oxford.

The Scents of Butterflies.

AMONG all the country sights of Spring, Summer and early Autumn, I suppose there is none more familiar than that of the common white butterflies. They are to be seen, as we know, everywhere; haunting woods, hedgerows, lanes and gardens, crossing heaths and meadows, and visiting at times not only parks and squares, but even streets in the heart of London. Of these insects there are, in this country, as is no doubt known to many of you, three especially abundant kinds. One of these is the large Cabbage White; the other two species are smaller. These two latter kinds are much alike when seen on the wing; but on a closer view they are easily distinguished, the most obvious mark of difference being the presence in one of them of greyish-green streaks, following the course of the so-called "veins" or "nervures," on the under surface of the hind wing. From this character the form in question gets its common name of the "Green-veined White." If anyone will capture a male Green-veined White (easily distinguished from the female by the much slighter spotting of the male's upper surface), and will brush the upper surface of the fore or hind wing with a camel-hair pencil, he will become conscious of a strong agreeable odour like that of the so-called "lemon-plant." On further examination he will find that this perfume emanates from the wings of the butterfly, and is strongly perceptible on the brush with which the wings were rubbed. The rubbing process has, of course, dislodged large quantities of the minute scales with which the wings of this insect, like those of butterflies and moths in general, are clothed; and these dust-like scales, adhering to the brush, have in some way or other carried with them the characteristic odour of the butterfly. A similar scraping or rubbing of scales from the under surface of the wings does not emit the odour, nor is it found in association with any scales from either surface of the female.

We find, then, that in this butterfly the perfume attaches to the scales in one particular situation, namely, the upper surface of the wings of the male insect. This fact obviously suggests that we should examine these particular scales in order to find out whether they present any differences from the scales which appear to be odourless. On applying the microscope to the scraping which carries the scent, we find at once an answer to our question. The great majority of the scales are of the ordinary well-known kind, consisting of an elongated flattened lamina, provided at one end with a short quill-like footstalk by which they are attached to the membrane of the wing, and frequently showing at the other extremity a more or less marked indentation. But among these will be found certain scales of an entirely different appearance. These latter scales in the insect before us are somewhat heart-shaped, carrying a long footstalk which ends in an almost circular disc, and tapering at the other extremity to a point. But the greatest peculiarity of these special scales is to be found in the plume-like structure which crowns their apical portion. Under a low power of the microscope the appearance is that of a tuft of fine transparent hairs, strongly suggestive of the vibratile cilia which are so familiar in animal and vegetable histology; but these hair-like processes, unlike the cilia, have no faculty of active movement, and under a high power they bear rather the aspect of minute tubes, in many cases seeming to be open at their free extremity. On examining a similar scraping from the under surface of the male, or from either surface of the female wing, we find only scales of the ordinary kind; the special "plume-scales," as they have been called, being invariably absent. Inasmuch, then, as the characteristic fragrance is found only in scrapings which contain the plume-scales, we are justified in concluding that these remarkable structures act as carriers of the perfume.

So far we have considered only one species of butterfly, the common Green-veined White; but the question will naturally be asked—what about other butterflies, the other common whites, for example? Is this flowery perfume a peculiarity of one species only, or is the property of emitting a fragrant odour shared by other related insects? In order to answer this question, let us suppose that we make a series of trials on the second species of smaller common White, the small Cabbage or Garden White, as it is usually called. Here, again, no trace of a flowery odour is discoverable in the female or in scales from the lower surface of the wings in the male; but, as before, the upper surface of the wings in the latter sex will be found to be scented, and, also as before, the scent will be found to adhere to the scales removed by scraping or brushing from the upper surface. Examining the scented scraping microscopically, we find that here, too, are a number of plume-scales mixed in with scales of the ordinary type. These plume-scales bear a family resemblance to those of the previous species, but are easily distinguishable from them (see the diagram). In fact, it is quite as easy, perhaps easier, to discriminate between the two species of common white by comparing two scales measuring not much more than one-tenth of a millimeter in length, as it is to tell them apart by examining entire specimens of both insects.

We conclude, then, that the scent-producing function is essentially similar in the two kinds of smaller common white. But it is to be observed that the endowments of the two in this respect, though similar, are not identical. The scent in the Green-veined White (*Ganoris napi*) is, so far as my experience goes, always present and easily recognisable, this being the reason why I chose it for first mention. But with the common Garden White (*Ganoris rapae*), the case is different. You will probably find some male specimens with no appreciable scent at all; others with the scent so faintly developed that you may be doubtful about its presence; in none, probably, will the scent be nearly so strong as in the case of the Green-veined White that we began with. Moreover, the character of the perfume differs. It has been not inaptly compared to sweetbriar, and it is at all events quite distinct from that of its near relation *Ganoris napi*. Extending our observations to the Large Cabbage White (*Ganoris brassicae*), we find a plentiful supply of plume-scales of quite a different aspect, these being very much longer, tapering gradually from base to apex, and showing none of the elegant heart-shaped outline that we saw in the other two species. The regularly disposed fringe or plume of the smaller whites is here also replaced by a kind of untidy bundle. These scales, again, are present only in the male, and only on the upper surface of fore and hind wing. What about the scent? It cannot be said to be entirely non-existent, but it is certainly the case that anyone of average olfactory powers may examine many male specimens of the large white without being able to detect any characteristic odour whatever. In some individuals, however, it is unquestionably present, though it is, as a rule, only to be appreciated with difficulty. But when detected it is like a faint whiff of violet powder, or, as has been happily suggested by Dr. Longstaff, orris-root.

There is in Africa a well-marked "genus," or group of white butterflies, which goes by the name of *Mylothris*. The members of this group are in many respects much like the common whites of our own country; they are, however, as a rule somewhat brighter in appearance, many of them having a touch of vermilion, orange, or some shade of yellow at the base of the wings close to the body. This is more frequent on the under surface, but in many cases it is seen on the upper side as well. The genus also differs from our common whites in usually possessing a distinct marginal row of dark spots. The figures shown on the screen, which were photographed in colour from nature, were prepared for another purpose; they give, however, a good idea of the general features of the group.

Now all the species of *Mylothris* which I have had the opportunity of examining during life possess a strong and fragrant odour, which is developed under just the same conditions as in the butterflies we have already discussed. That is to say, it belongs to the male alone, and in that sex is confined to the upper surface of the forewings.

The scent of *Mylothris agathina*, which is abundant in many parts of Africa south of the Sahara, appears to me to be exactly that of sweetbriar. It is a good deal like that of our garden white (not the green-veined species), but very much more intense. Indeed, so powerful is it, that I have more than once perceived it when the butterfly was held in the hand at some considerable distance.

There is a pretty species of *Mylothris* which I found rather common at East London (South Africa). It has been named *trimenia*, after the distinguished naturalist who presided for many years over the South African Museum at Cape Town. It has a graceful, floating flight, and the hindwings in the male are of a delicate lemon-yellow. The general attractiveness of this insect is increased by its pleasant fragrance. This is quite different from the odour of its congener *agathina*, though at least equally powerful. It is not easy to describe, but may perhaps best be compared to the scent of sweet-pea or clover, with a suggestion of orange-peel or lemon. The females of both these species of *Mylothris* are destitute of odour. The curious shape of the male scent-scales, which is tolerably uniform throughout the genus, is shown in an outline diagram.

In a couple of months' time our country lanes will be enlivened by the presence of a butterfly known no doubt to many of you as the "Orange-tip." This very pretty insect is much like one of our common whites, but may be distinguished by the elaborate green mottling of portions of the under surface—a feature of colouring which causes the butterfly to be almost invisible when it settles with closed wings on the head of an umbelliferous plant. This green mottling is found in both sexes, but the male has in addition a large patch of bright orange at the apex of the forewing. Whether this butterfly is scented I am unable to say, as I never happen to have had the opportunity of examining live specimens since I began to search for butterfly odours, but the male certainly possesses plume-scales of the same general character as those of the common whites, though differing, as will be seen by reference to the diagram, in points of detail. I should be glad to receive information as to its power of scent-production from any of you who may make the acquaintance of the butterfly during the coming Spring.

Though I have at present no information on this point as to our English Orange-tip, I have been able to test several foreign species which are somewhat like it in aspect, and are not very far removed from it in point of affinity. These belong to the genus *Teracolus*. One of these *Teracoli*, called *omphale*, common in Africa, has a scent which I compare to that of *Philadelphus*, commonly called "Syringa," or perhaps more exactly to that of the white lily, together with a more aromatic constituent suggesting at one time chocolate, at another, musk. A second African Orange-tip, *Teracolus achine*, has a scent, not always present, which reminds me of honeysuckle. The scent of a third African species, *Teracolus auxo*, in which the general coloration is yellow instead of white, recalls that of jasmine or *Philadelphus*.

Closely allied to these tropical and subtropical Orange-tips is a group in which the orange apical patch is replaced by a brilliant crimson. The South African representative of this group has a scent which comes nearest to the garden heliotrope, commonly called "cherry-pie." In yet another group we find, instead of orange or crimson, an apical patch of glossy purple. The only species of Purple-tip which I have had an opportunity of examining in the living condition is also possessed of a sweet, flowery scent somewhat different from that of the last.

Many more examples of these perfumed white butterflies could be given, but I should only weary you by multiplying cases. For our present purpose it is sufficient to say that among the butterflies that are fairly close relatives of our common whites, the odours, though not universal, are very frequently present; that they vary much in character and intensity, though possessing in common an agreeable quality and a likeness to the perfume of certain vegetable products, particularly to the scent of flowers. They are almost without exception confined to the male sex, and to the upper surface of the wings, and they are almost invariably found in connection with the peculiarly specialised scales that we have learned to distinguish as "plumules." The only exception to the latter statement that I know of among this particular assemblage of butterflies is the yellow African Orange-tip, *Teracolus auxo*, in which I find no plume-scales, though it has a flowery odour which in some specimens is really strong.

Apart, then, from a few possible exceptions, we have certainly established a relation between the presence of plume-scales and the emission of a flower-like odour. What is the nature of this relation? To answer the question let us examine the plume-scale itself a little more closely. This diagram shows a typical form of plume-scale taken from one of the common African whites, *Belenois thysa*. This butterfly, which somewhat recalls one of our common whites, though generally larger, and still more closely resembles *Mylothris agathina* lately mentioned, has a strong, sweet odour like that of roses. The scale consists of a flattened plate, or lamina, rounded at the base and sharp at the apex. At the middle of the base is attached a slender footstalk, at the end of which furthest from the lamina we find another flattened structure, in this species shaped like a cheese-cutter, which may be called the "accessory disc," or simply the "disc." From the apex arise the cilia-like processes, which may conveniently be termed the "fimbriæ." This may serve as an example of a form of plume-scale very commonly found in the white butterflies or *Pierinæ*, but the scale in all its parts is liable to considerable modification; and, indeed, it may be said with truth that in no two species are the scent-scales exactly alike. The diagrams will show how very diverse are the forms assumed by the plumules of these white butterflies; but in all of them may be observed under various shapes the lamina, fimbriæ, footstalk and accessory disc. The scale is formed of chitin, a substance which constitutes the outer covering, or external skeleton, as it is called, of many insects, and which when met with in bulk is of a hard and horny consistence, as may be seen, for instance, in the fore-wings or "elytra" of beetles. Chitin is practically a dead material, and there is no trace to be found in the scale of any protoplasmic or living matter. The granules which are present are probably pigment granules, the presence of which in the ordinary scales imparts to the wings their characteristic colour. So far we have discovered no apparatus to which we may attribute the production of perfume.

But now let us examine the means by which the scale is attached to the membrane of the wing. The point, or rather surface, of attachment is the accessory disc, which fits into a cup-shaped depression in the wing-membrane, which cavity, however, is generally not large enough to admit the whole of the disc. In many species can be seen an orifice in that part of the disc which is enclosed, when the scale is *in situ*, within the cup-shaped cavity or socket just mentioned. And when the fimbriæ are examined with a very high power, an appearance is seen in many species which suggests that their free extremities are not closed, but open; that they are, in fact, minute tubes which put the interior of the scale into communication with the outer air. Now, can we discover any means by which, say, a vapour entering the disc by the orifice in its buried portion can be conveyed through the scale and find its way out through the patent extremities of the fimbriæ? It certainly appears that we can. Within the disc there is generally visible a chitinous structure which often bears the appearance of a convoluted tube; the footstalk which forms a bridge between disc and lamina is apparently not solid, but pervious. The lamina itself consists of two delicate chitinous layers, one of which may be called dorsal and the other ventral, enclosing a flattened cavity which contains a certain amount of interstitial material. This latter takes various forms in different species, but very often presents the appearance of a longitudinal striation, which in all probability betokens the existence of fine parallel channels or passages traversing the interior of the lamina side by side from base to apex. This longitudinal striation is frequently obscured by the accumulations of granular pigment; but in many cases there is a comparatively clear area near the apex where the striæ can be fairly well made out, and where they can be seen to correspond in number and position with the individual fimbriæ. There is, then, much reason to suppose that the cavity of the lamina is more or less completely divided into channels which communicate in one direction with the fimbriæ, and so, through the orifices of the latter, with the outer air; and in the other direction through the footstalk with the disc, and so through the aperture of the disc with the socket of the wing-membrane and its underlying structures. We have, therefore, some warrant for considering the scale to be a piece of apparatus not indeed for the manufacture, but for the distribution of scent; and to get some insight into the mode of production of the latter, it is evident that we must pursue our researches into the structure of the wing itself.

It was noticed by Weismann over thirty years ago, and more recently by Günther, that in the hypodermis, as it is called, or cellular layer immediately underlying the homogeneous surface-membrane of the wing, there occur certain cells which appear to be specialised for the production of a secretion. These cells were described and figured by Günther under the name of "Drüsenzelle," or "gland-cells." In this diagram, which is copied from one of Günther's figures, we see two of these "gland-cells" in direct connection with the sockets in the wing-membrane into which fit the footstalks of two scales partly seen in section. These scales are not plume-scales, for they possess no disc; but if secreting cells are found, as here, in connection with scales of the ordinary kind, there seems to be no reason why we should not also find them in relation with the plume-scales, supplying in that position the living and working protoplasmic element by means of which the scent-bearing secretion is elaborated. The clear spaces in these cells of Günther's figure are rather suggestive of the oil or fat vacuoles seen especially in growing cells of adipose tissue; and it may be conjectured that the scent-bearing secretion is of the nature of a volatile oil. In the case of the ordinary scales the secretion may still be oily. Probably most of us know how difficult it is to wet a butterfly's wing with water. This is no doubt partly due to the mechanical conditions involved in the coating of minute scales, but it is possible that, as in the case of the plumage of aquatic birds, some additional power of resistance to wet is afforded by the presence of an oily secretion, which may be conveyed to the surface by the scales of ordinary character. It is also possible, as has been suggested by Weismann, that the secretion formed in connection with the ordinary scales may bear an odour, though of a different nature from that of the plume-scales, and, at least in many cases, imperceptible by our senses. All this is a matter of more or less probable conjecture, and it is very clear that there is a good deal more work to be done before we can be sure that we know all about the various functions of the scales and their associated structures.

Before we go on to the next part of our subject I should like to call your attention to some figures that will be thrown upon the screen of various forms of plume scales. In these figures the chitinous sculpturing of the scale will be seen. It differs in character from species to species, but in all there is more or less visible a longitudinal striation of the lamina, which we have seen reason for interpreting as an indication of channels along which pass the odoriferous secretions or exhalations from the gland-cells buried in the substance of the wing to the fimbriæ and so into the open.

The question will no doubt have occurred, Are these plume-scales the only structures by which the scents of butterflies are distributed? They are by no means the only ones. There are many other methods of distribution of these flower-like odours, some of which we can find without going beyond the group of so-called white butterflies, or Pierines. Visitors to the south coast of England in the late summer and autumn months can hardly have failed to notice a very active butterfly of a fine bright orange colour with a dark border, which is especially given to haunting fields of lucerne and clover. This is the butterfly commonly called the "Clouded Yellow," one of the most conspicuous of the whites, or, as we ought rather to say, the Pierine butterflies. In this insect we should search for plume-scales in vain; but on examining in a male specimen the front edge of the hindwing where it is overlapped by the forewing, we find on the upper surface a patch of scales distinguished from their surroundings by their lighter colour. The microscope shows that these scales are of a different shape from those of the rest of the wing, and are packed much more closely together; moreover, instead of lying nearly flat upon the wing, like the tiles on a roof, they are set up on end, sometimes almost at a right angle. When the wing membrane is denuded of scales and examined with a high power, the situation of the patch is easily recognisable by the crowding together of the sockets for the insertion of the footstalks, and also by the fact that tracheæ, or air-tubes, are seen to be leaving one of the main "veins" of the wing and supplying this particular area, breaking up into smaller branches as they go.

Under ordinary circumstances the scent of the Clouded Yellow is not easily detected; but if in a living specimen the scales be scraped off one of the patches that have just been described, they will in many cases be found to have an odour which is somewhat like that of the garden heliotrope, or "cherry-pie." The South African Clouded Yellow, which is much like ours, though quite distinct, has a similar patch and a similar odour. The scent-producing apparatus in these Clouded Yellow butterflies presents many features of interest; in the first place, the scent-scales are crowded together into one small area, instead of being generally distributed over the wing-surface as in the common whites. Then the scales are quite unlike plume-scales, having neither fimbriæ nor accessory disc, while the footstalk is short and quill-like, instead of being long and flexible as in the plume-scales. They are, indeed, quite of the type of the ordinary scales, except that they differ a little in size and shape from the scales of their immediate surroundings. The distribution of tracheæ, or air-tubes, to the site of the scent patch is noteworthy, and so also is the fact that under ordinary circumstances the patch is covered by the overlap of the forewing, which acts like a sliding lid. It may reasonably be conjectured that this arrangement ensures economy of the perfume. The production of the scent is confined to a limited area, and its escape is prevented under ordinary conditions by the overlapping edge of the forewing. When emission of the scent is required, a slight separation of the fore and hind wings gives it exit. The special distribution of tracheæ may be a provision for pumping air into the patch from below, and so supplying a *vis a tergo* to assist the escape of the perfume.

Many other butterflies possess similar patches of scent-distributing scales. They are generally, though not always, so placed as to be covered up in the ordinary position of the insect. In some instances there are two patches on each side, one on the upper surface of the hindwing, the other on the under surface of the forewing, these being so arranged that they exactly cover one another when the butterfly holds its wings in the normal position. It is to be observed that in these patches the rule is for the scent-scales to be of the same general character as the ordinary scales of the wing, though they may differ much from the latter in shape, size and arrangement. The patches seldom contain plume-scales, and, when fitted with a sliding lid, I believe it would be correct to say that they never do.

The question may be asked: is it not necessary that the scent should be economised in the case of the plume-scales quite as much as in the case of the definite patches? No doubt it is; and a little further consideration of the typical plume-scale may show us how this is effected. The plume-scales, it is true, being as a rule generally distributed over the wing, cannot be shut down under a lid; but they are frequently scattered among ordinary scales which are a good deal longer and larger, and which may act as coverings to the individual scales, though there is no general covering for the whole. Then again, as we have seen, the plume-scale has an accessory disc and a long footstalk. The disc with its internal chitinous structure may act as a reservoir for the scent; it will be remembered that in many cases it appears to contain a convoluted tube. The footstalk seems to be flexible, and it often shows one or more sharp bends in its course. These bends may impede the passage of the scent from the reservoir in the disc to the lamina and fimbriæ, and the butterfly may be able, by some movement of its wings, to bring about a straightening of the footstalk and a consequent liberation of the odour. At any rate, it is probably significant that the apparatus of accessory disc and long flexible footstalk belongs to the plume-scale alone.

There is a handsome butterfly, common in Africa, which is not far removed in affinity from our well-known "Brimstone." This butterfly, which is known as *Catopsilia florella*, has in the male a strong fragrant scent. Now *Catopsilia florella* possesses on the hindwing a patch of special scales which is somewhat similar in aspect and position to the scent-patch in the Clouded Yellow. But, curiously enough, the characteristic scent appears to proceed, not from the patch on the hindwing, but from another structure altogether. This is a beautiful silky fringe of long hair-like scales which are set along the edge of the forewing on its underside, and are covered as a rule by the overlap of the hindwing. When the wings are parted and the fringe spread out, a scent is exhaled which appears to both Dr. Longstaff and myself to be like that of jasmine or tuberose. The portion of the wing which forms the seat of this silky fringe, as well as that underlying the scent-patch of the hindwings, is furnished, like the patch in the Clouded Yellow, with a plentiful supply of air-tubes, proceeding to it from the adjacent "vein." In the case of the patch in *Catopsilia florella*, the ramifications of these air-tubes seem to form a fine polygonal network, each mesh of which surrounds the socket of a scent-scale. A similar appearance may be seen in the scent-patch of one of the *Teracoli* (*T. fausta*), and probably in that of others.

Sessile scent-patches, which may or may not be accompanied by silky fringes, occur in many other species of the brimstone-like section of Pierines, and in several of these, including both Asiatic and American species, a flowery odour has been detected both by the late Mr. de Nicéville and by Dr. Longstaff. An observation which would be of great interest, but which, so far as I am aware, has not yet been made, would be to compare the odour diffused by the fringes with that conveyed by the sessile patches in those species where both these forms of apparatus occur together.

An accumulation of hair-like scales, no doubt serving as scent-distributors, may also be seen in another genus of Pierines (*Dismorphia*) which is remote in affinity from those butterflies which have just been considered. But these odoriferous tufts or fringes are by no means confined to the Pierines. In the very different group of Satyrines, to which our common brown hedgerow butterflies belong, the males of some species possess fringes or tufts which are clearly similar in function to those of their distant cousins, the Whites. In one of these, an African species, I found that the odour produced was like that of vanilla chocolate. Another species of the same genus, a native of India, was named by Wood-Mason and de Nicéville *suaveolens*, from its pleasant fragrance. The vanilla odour was found by the same two observers in several other Indian butterflies belonging to different groups.

Some Satyrines have plume-scales which are not unlike those of the Pierines, but differ in seldom, or perhaps never, possessing an accessory disc. At the utmost they may show a slight dilatation of the articulating end of the footstalk. Plume-scales much like those of the common Browns are also found in the Fritillaries, which belong to the great group of Nymphaline butterflies.

There is yet another kind of scent-scale, specialised in form. This is the well-known "battledore" scale, present in the male of many of the small blue butterflies belonging to the subfamily of Lycænids. These battledore scales are provided with apertures on their general surface which no doubt serve, like the apertures of the fimbriæ in the plume-scales, for the escape of the odour into the outer air. The ribs apparent in the "battledore" are in all probability homologous with the longitudinal channels seen in the Pierine plume-scale. These in *Mylothris*, as we have seen on the screen, take the form of ribs as definitely marked as those of the Lycænids.

So far, all the scents with which we have been concerned are of a kind that is agreeable to our own senses. But there is another sort of odour which is also commonly present, especially in the butterflies of tropical and subtropical regions, and which, instead of being pleasant to the human sense, is disagreeable or even repulsive. The Acræas, which are mostly reddish or brownish butterflies with small dark spots; the Euploeas, large butterflies which often show a brilliant purple gloss like that of our own Purple Emperor; the Papilios, of which a good example is the black and yellow Swallowtail butterfly of the Cambridgeshire fens, have many of them an odour which may be called disgusting. Musty straw, stable litter, rabbit-hutches, acetylene, bilge-water, these are some of the substances to which the odours of these unsavoury butterflies have been compared. In some cases, as in the instance of the agreeable perfumes, the seat of these evil-smelling odours has been found in patches or tufts of specialised scales or hairs: in others the scent appears to be emitted from the general wing-surface. But in no instance, so far as I am aware, has any structure like a plume-scale been found guilty of emitting anything but a pleasing fragrance. A very remarkable difference between the scents pleasant and the scents unpleasant is this: that the former kind usually, though not invariably, is confined to the male sex; while the latter kind is common to both sexes, being often indeed stronger in the female.

It has, no doubt, occurred to you to ask: has the presence of these scents any particular significance with regard to the needs of their possessors; and if so, what? And why should the agreeable scents be so commonly confined to the one sex, while the repulsive odours are shared by both?

The second question helps us to answer the first. With regard to the scents of the disagreeable kind, which are probably often accompanied by a nauseous flavour, there is good reason to suppose that they are in effect a means of protection from insect-eating enemies. We have much actual evidence bearing upon the point. Evil-smelling butterflies, like the Acræas or the well-known *Limnas chrysippus* (a large brown butterfly common throughout many parts of Africa and Asia), are often conspicuous, slow-flying, and given to courting observation rather than to avoiding it. These are all marks of butterflies which are more or less immune from attack by birds; and it may be added that the frequency with which many of them are copied by other butterflies gives further reason for the conclusion that they enjoy protection in virtue of their distasteful qualities—a protection which other butterflies are enabled to share by resembling them in outward appearance.

Now, granted that the avoidance of attack by birds is the object of the repulsive scents, we should, of course, expect to find them present not in one sex only, but in both alike. And this is precisely what we do find; moreover, since it is well recognised that the preservation of the life of the female is more important than that of the male for the welfare of the species, we should expect that if there is a difference between the sexes in the intensity of the odour, that difference would be in favour of the female. This, again, is borne out by observation in a number of cases. Where both sexes are repulsive, the female, as a rule, is the more repulsive of the two, and therefore (as a consolation) the safer from attack.

So much for the odours unpleasant. Now let us turn to the other kind, the fragrant flower-like perfumes with which we dealt at the outset. These, we saw, are frequently associated with specialised scales which are the exclusive property of the male sex. We cannot say quite so much for the odours themselves, for though in the great majority of cases they belong to the males alone, yet the females are not left entirely destitute. Fritz Müller many years ago found evidence of sweet scent in a female white butterfly, and since then Dr. Longstaff has detected in the females of several species a fragrance not unlike that of the male, but usually much weaker. Still, we may certainly say, speaking generally, that the pleasant odours show a vast preponderance in favour of the male. This suggests that they must have some significance in regard to the relations between the sexes, and, indeed, there can be little or no doubt that, as was first pointed out by Fritz Müller, these scents are employed by the males in courtship as a means of attraction; they may also perhaps serve as a means of recognition. That their employment is occasional, and not constant, appears from the fact that they are so often furnished with a provision for keeping them confined until wanted. There is, so far as I am aware, no direct evidence that they are more plentifully liberated during courtship; but to any one who has observed the persistent fluttering of white butterflies about and around each other under those circumstances, it can hardly fail to occur that the fanning wing-movements of the male must have the effect of encouraging the evaporation and diffusion of the odour; also perhaps of aiding it escape from the disc through the footstalk and so into the lamina of the plume-scale. The flowery scents would thus come under the head of those features which have been called by Professor Poulton "epigamic;" characters, that is, which, like the splendid plumage of some cock-birds, are believed to further the cause of matrimony. If this interpretation be correct, it is most interesting to find that the æsthetic preferences of butterflies in the matter of scents are so much like our own. In other insects, as well as in many of the higher animals, we find attraction exercised by odours that to our senses are disgusting. Butterflies themselves are not exempt from a depraved taste where food is concerned; the best bait for the Purple Emperor is well known to be a piece of putrid meat. But in matters of love-making, the butterfly seems to resort for his means of fascination to methods which recall the human lover with his gifts of flowers and boxes of vanilla chocolate.

The evil odours tend to be somewhat persistent. In some cases they may be detected for a long time after the butterfly is dead and stiff. The agreeable scents, on the other hand, are usually evanescent, becoming imperceptible very soon after the insect has ceased to live. On one occasion I was able to detect the lemon-plant odour of a Green-veined White when the butterfly had been dead for eleven days, but this is probably an extreme case. Both kinds of odour may be present in the same species; when this is so, it is commonly found that the first impression given by the butterfly is a disagreeable one, the pleasant constituent only becoming apparent when its distributing apparatus is specially exposed. These cases of a double odour follow the same rule of repulsive scents being common to the two sexes, and agreeable perfumes being confined to the male.

This, ladies and gentlemen, finishes what I have to say on the subject of the Scents of Butterflies. I am conscious that I stand in need of your indulgence; as, from the force of unavoidable circumstances, I have had but a short time in which to prepare this Lecture. But, "qui s'excuse, s'accuse," and I trust that in spite of its sketchy and imperfect character the discourse to which you have just listened may have succeeded in quickening the interest that most of us feel in these very attractive objects of nature, and in giving fresh emphasis to the fact that the study of insects in general, and of butterflies in particular, is capable of shedding light upon questions of high importance in the science of biology.

GENERAL MONTHLY MEETING,

Monday, March 6, 1911.

Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Major C. Ashburnham,
Mrs. Roy-Batty,
Colonel Edward Hugh Bethell, D.S.O. R.E.
David Maurice Edward Dent, Esq., B.A.
Martin de Selincourt, Esq.
James J. Dobbie, Esq., M.A. D.Sc. F.R.S.
Miss Isabel Ida Greaves,
D. C. J. H. Kropveld, Esq., LL.D.
Miss S. McNaughton,
Frederick William Mander, Esq., M.A.
Miss Ettie Sayer, M.B. B.S.
Charles James Tabor, Esq.
Walter Wahl, Esq., D.Sc.
Mrs. Watson,

were elected Members of the Royal Institution.

The Honorary Secretary announced the decease of Professor Julius Wilhelm Brühl on February 5, 1911, and the following Resolution, passed by the Managers at their Meeting held this day, was read :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the world of Science in the decease of Professor Julius Wilhelm Brühl, Ph.D. (Göttingen), Sc.D. (Cantab.), the distinguished Chemist, formerly Professor in the University of Lemberg, and in later years one of the most celebrated Professors in the University of Heidelburg.

Professor Brühl's name will always be associated in the Scientific world with his most enlightened and invaluable researches on refractivity and the chemical constitution of organic compounds. Professor Brühl was elected an Honorary Member of the Royal Institution in 1904. In May 1905 he delivered a Friday Evening Discourse at the Royal Institution on his own subject, "The Development of Spectro Chemistry."

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Frau Brühl and the family in their bereavement.

The following Alterations in the Bye-Laws of the Royal Institution were passed :—

In Chapter XI. (of the property of the Institution).

In Art. 1, line 3, *insert* "(except such sale as is mentioned in Article 5 hereof)."

In Art. 1, line 6, *for* "or of any stock or money of the Corporation which shall be invested in the public funds," *substitute* "or of any moneys, stocks, funds or securities of the Corporation."

In Art. 2, line 3, *for* "in the public funds," *substitute* "in or upon the stocks, funds or securities hereinafter authorised."

In Art. 5, line 3, *for* "shall either be annually invested by the Committee of Managers in the public funds in the name of the Corporation," *substitute* "and any other moneys available for and requiring investment shall be from time to time invested by the Committee of Managers at their discretion in the name of the Corporation in such stocks, funds and securities, in or upon which trust funds or cash under the control or subject to the High Court of Justice may for the time being be authorised by law to be invested, and may at such discretion be transposed into other investments of any nature hereinbefore authorised."

In Art. 5, line 7, *insert* "any sale made for the purpose of transposing an investment may be made without the approbation or vote of the Members as mentioned in Art. 1 hereof, and any such sale is hereby expressly excepted from the provisions of Art. 1."

The following Amendments to the above Alterations of the Bye-Laws were passed :—

In Chapter XI.

In Art. 1, line 3, *insert* "and no." In line 5, *insert* "the Resolution passed by a majority." In lines 5 and 6, *omit* the words "a majority of." In line 6, *for* "vote" *substitute* "resolution." In line 7, *for* "and notice of such intended" *substitute* "any such vote shall be taken only after notice in writing of the." In line 8, *for* "being" *substitute* "to be voted upon," *for* "such a" *substitute* "the" *and insert* "any such notice to be."

In Art. 2, line 2, *transfer* "in the name of the Corporation" to line 3. In line 3, *insert* "subject to the like powers of transposition as are also hereinafter authorised."

In Art. 5, line 6, *insert* "from time to time." In line 9, *insert* "whether under Article 2 or under this Article."

The following Lecture Arrangements After Easter were announced :—

J. E. C. Bodley, Esq., Corresponding Member of the Institute of France. Three Lectures on : 1. Cardinal Manning ; 2. The Decay of Idealism in France and of Tradition in England ; 3. The Institute of France. On *Tuesdays*, April 25, May 2, 9.

Professor Frederick W. Mott, M.D., F.R.S., F.R.C.P., Fullerian Professor of Physiology, Royal Institution, etc. Two Lectures on The Brain and the Hand. On *Tuesdays*, May 16, 23.

W. W. Watts, M.A. M.Sc. F.R.S., Pres. G.S., Professor of Geology, Imperial College of Science, London. Two Lectures on : 1. The Ancient Volcano of Charnwood Forest (Leicestershire) ; 2. Charnwood Forest and its Fossil Landscape. On *Tuesdays*, May 30, June 6.

Professor R. W. Wood, LL.D., Johns Hopkins University. Three Lectures on The Optical Properties of Metallic Vapours. (*Illustrated.*) On *Thursdays*, April 27, May 4, 11. (The Tyndall Lectures.)

William Napier Shaw, Esq., M.A. LL.D. Sc.D. F.R.S. M.R.I., Director of the Meteorological Office. Two Lectures on Air and the Flying Machine. 1. The Structure of the Atmosphere and the Texture of Air-Currents; 2. Conditions of Safety for Floaters and Fliers. On *Thursdays*, May 18, 25.

T. Thorne Baker, Esq., F.C.S. Two Lectures on : 1. Changes Effected by Light; 2. Practical Progress in Wireless Telegraphy. (*Illustrated.*) On *Thursdays*, June 1, 8.

Professor Selwyn Image, M.A., Slade Professor of Fine Art, Oxford. Three Lectures on: 1. John Ruskin, or the Seer and Art; 2. William Morris, or the Craftsman and Art; 3. Walter Pater, or the Connoisseur and Art. On *Saturdays*, April 29, May 6, 13.

W. P. Pycraft, Esq., F.Z.S., Zoological Department, British Museum. Two Lectures on Phases of Bird Life; 1. Flight; 2. Migration. On *Saturdays*, May 20, 27.

W. L. Courtney, Esq., M.A. LL.D., Fellow, New College, Oxford. Two Lectures on Types of Greek Women; Nausicaa and the Homeric Women; Sappho and the Aeolian Poets; Aspasia and Pericles. On *Saturdays*, June 3, 10.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Library Catalogue (Natural History), Vol. III. 4to. 1910.
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American Academy of Arts and Sciences—Proceedings, Vol. XLVI. Nos. 13–17. 8vo. 1911.
American Geographical Society—Bulletin, Vol. XLII. No. 12; Vol. XLIII. No. 1. 8vo. 1910–11.
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British Astronomical Association—Journal, Vol. XXI. No. 4. 8vo. 1911.
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Canada Geological Survey—Reports, Nos. 1006; 1008. 8vo. 1910.
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Chemical Industry, Society of—Journal, Vol. XXX. Nos. 3–4. 8vo. 1911.
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Dax, Société de Borda—Bulletin, 1910, Parts 2–3. 8vo.
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American Journal of Science for Feb. 1911. 8vo.
Athenæum for Feb. 1911. 4to.
Author for March, 1911. 8vo.
Chemical News for Feb. 1911. 4to.
Chemist and Druggist for Feb. 1911. 8vo.
Concrete for Feb. 1911. 8vo.
Dyer and Calico Printer for Feb. 1911. 4to.
Electrical Engineer for Feb. 1911. 4to.
Electrical Engineering for Feb. 1911. 4to.

Editors—continued.

Electrical Review for Feb. 1911. 4to.
Electrical Times for Feb. 1911. 4to.
Electricity for Feb. 1911. 8vo.
Engineer for Feb. 1911. fol.
Engineer-in-Charge for Feb. 1911. 8vo.
Engineering for Feb. 1911. fol.
Horological Journal for Feb. 1911. 8vo.
Illuminating Engineer for Feb. 1911. 8vo.
Journal of the British Dental Association for Feb. 1911. 8vo.
Law Journal for Feb. 1911. 8vo.
Model Engineer for Feb. 1911. 8vo.
Motor Car Journal for Feb. 1911. 4to.
Musical Times for Feb. 1911. 8vo.
Nature for Feb. 1911. 4to.
New Church Magazine for March, 1911. 8vo.
Page's Weekly for Feb. 1911. 8vo.
Science Abstracts for Jan. 1911. 8vo.
Zoophilist for March, 1911. 8vo.

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Florence, Biblioteca Nazionale—Monthly Bulletin for Feb. 1911. 8vo.

Florence, Reale Accademia dei Georgofili—Atti, 1910. Disp. 4. 8vo.

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Gowland, William, Esq., F.R.S. M.R.I. (the Author)—The Art of Working Metals in Japan. 4to. 1910.

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Imperial Institute—Bulletin, Vol. VIII. No. 4. 8vo. 1910.

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Manchester Literary and Philosophical Society—Memoirs, Vol. LV. Part 1. 8vo. 1911.

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Royal Irish Academy—Proceedings, Vol. XXIX. B, Nos. 1-2. 8vo. 1911.

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Arkiv: Botanik, Band X. No. 1; Kemi, Band III. No. 6, Band IV. No. 1; Matematik, Band VI. Nos. 2-3; Zoologi, Band VII. No. 1. 8vo. 1910-11.
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Vienna Imperial Geological Institute—Verhandlungen, 1910, Heft 13-16. 8vo.
Abhandlungen, Band XX. Heft 3: Band XXI. Heft 2. 4to. 1910.

Warsaw, Society of Sciences—Comptes Rendus, Vol. III. Nos. 8-9. 8vo. 1910.

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Western Society of Engineers—Journal, Vol. XVI. No. 1. 8vo. 1911.

WEEKLY EVENING MEETING,

Friday, March 10, 1911.

SIR WILLIAM CROOKES, O.M. LL.D. D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

The Hon. SIR CHARLES A. PARSONS, K.C.B. J.P. M.A. Sc.D. LL.D. F.R.S.

Recent Advances in Turbines.

ON two previous occasions I have addressed this Institution on the steam turbine. At the time of the first lecture, in 1900, the turbine may be described as having been in the "advanced experimental stage." Six years later it was meeting with "general acceptance" in certain fields. To-night I propose to review its progress from 1906 to the present time; but before doing so I shall, with the view of leading up to the subject, and at the risk of some repetition, briefly explain the chief features of interest, and recapitulate some of the earlier steps in its introduction.

The first turbine of which there is any record was made by Hero of Alexandria 2000 years ago, and it is probably obvious to most persons that some power can be obtained from a jet of steam either by the reaction of the jet itself, like a rocket, or by its impact on some kind of paddle-wheel. It is, however, not so obvious that an economical engine could be made on this principle. In the year 1888 Dr. de Laval, of Stockholm, undertook the problem with a considerable measure of success. He caused the steam to issue from a trumpet-shaped jet, so that the energy of expansion might be utilised in giving extra velocity to the steam. Recent experiments have shown that by such a device nearly the whole of the available potential energy in the steam is converted into kinetic energy of velocity in a straight line, the velocity attained into a vacuum being about 43,000 feet per second. Dr. de Laval caused the steam to impinge on a paddle-wheel made of the strongest steel, which was allowed to revolve at the highest speed consistent with safety, for the centrifugal forces are enormous. Unfortunately, materials are not strong enough for the purpose (in the large sizes the speed is nearly half that of a rifle bullet), and the permissible speed of the wheel can only reach to two-thirds of that necessary for good economy, as we shall presently explain.

Dr. de Laval also introduced spiral helical gearing for reducing the enormous speed of his wheel to the ordinary speeds of things to be driven, and we shall allude to this gear later as likely to play a very important part generally in future turbine developments.

In 1884, or four years previously, I dealt with the turbine problem in a different way. It seemed to me that moderate velocities were essential if the turbine motor was to receive general acceptance as a prime mover. I therefore decided to split up the fall in pressure of the steam into small fractional expansions over a large number of turbines in series so that the velocity of the steam nowhere should be great, and consequently, as we shall see later, a moderate speed of turbine suffices for the highest economy. This principle is now universally adopted in all except very small turbines, where economy is of secondary importance. This arrangement of compounding turbines also appeared to me to be surer to give a high efficiency, because the steam was caused to flow in a non-expansive manner through each individual turbine, and consequently in an analogous way to water in water turbines, where high efficiency at that date had been proved. I was also anxious to avoid the well-known cutting action of high-velocity steam on metal.

The close analogy between laws for the flow of steam and water under small differences of pressure have been confirmed by experiment, and the usual formula = $\sqrt{2gh}$, where h is the hydraulic head, gives the velocity of issue from a jet for steam with small heads and also for water, and we shall presently follow this part of the subject further in dealing with the design of turbines.

Having decided on the compound principle, it was necessary to commence with small units at first, and in spite of compounding the speed of revolutions was still high.

Though, as we have said, the de Laval turbine appeared four years later, the de Laval cream separators were in use prior to 1884, and I had the advantage of seeing their beautiful means of balancing—the supporting of the bearings in elastic rubber sleeves, which at 6000 revolutions absorbed vibration and allowed the bowl containing the milk to rotate about its centre of gravity instead of its geometric centre. The first compound steam turbine ran at 18,000 revolutions, and had slightly elastic bearings. The turbine teeth or blades were like cog-wheel teeth, set at an angle and sharpened at the front edge, and the guide blades were similar. Gradually the form of the blades was improved—curved blades with thickened backs were introduced. The blades were cut off to length from brass material rolled and drawn to the required section, and inserted into a groove with soft brass packing distance pieces between and caulked up tightly, and dummy labyrinth packings of various types were introduced. The design was improved so as to reduce steam leakages and provide for greater expansion ratios.

The construction of a suitable dynamo to run with the turbine involved nearly as much trouble as the turbine itself; the chief features were the adoption of very low magnetic densities in the armature core and small diameters and means to resist the great centrifugal forces. The dynamo was also mounted in elastic bearings. Now that the turbine has found its most suitable field in large powers, and the speed of revolution is consequently reduced, elasticity in the bearings is less essential, and in large land plants and in marine work rigid bearings are now universal. I have said that steam behaves like an incompressible fluid in each turbine of the series, but as it is highly elastic, its volume increases with the succession of small drops of pressure, and the turbines have to be made larger and larger. This enlargement is secured by increasing the height of blade, by increasing the diameter of the succeeding drums, and by altering the angles and openings between the blades. All three methods are generally adopted to accommodate the expanded volume of one hundredfold in the condensing turbine.

Now as to the best speed of the blades. It will be easily seen that in order to obtain as much power as possible from a given quantity of steam, each individual row of blades must work under appropriate conditions. This, as has been found by experiment, requires that the velocity of the blades relatively to the guide blades shall be about one half the velocity of the steam, or, more accurately, equal to one half the velocity of issue from rest due to the drop of pressure in guides or moving blades. The curve for efficiency in relation to the velocity ratio has a fairly flat top, so that the range of velocity ratio for high efficiency is wide, and the speed of the turbine may be varied considerably about that for maximum efficiency without materially affecting the result.

In compound turbines the efficiency of the initial rows may be generally assumed as about 65 per cent., and of the latter rows at 75 to 85 per cent., and, considering the whole turbine, approximately 75 per cent. of the energy in the steam is delivered on to the shaft. The expansion curve may be expressed approximately by $pv = \log p_1/p_2$, where $p_1 - p_2$ is the drop in pressure across any turbine; pv is obviously not quite constant, but if a mean value is assumed the error is small. The expansion curve therefore lies between the adiabatic and isothermal curves for steam, but nearer the former, and the errors in these assumptions are found by experiment to be of much less importance than the errors in workmanship and imperfections of materials that are unavoidable in practical mechanics. The differential thermal expansions of the metal of which the turbine is made are the chief reason for large working clearances and loss by leakage, though every available means is taken to mitigate such loss.

In turbine design, the expression of the velocity ratio between the steam and blades may be represented by the integral of the square of the velocity of each row through the turbine, which is a coefficient called K. If K, for instance, as usual in land turbines, equals 150,000, then we know that with a boiler pressure of 200 lb. and a good vacuum the velocity ratio is 0·55, and the turbine is working close up to its speed for maximum efficiency. In large marine work, where weight and space are of importance, K varies from 80,000 to 120,000 or even to 140,000. With K = 80,000, a loss of efficiency of about 9 per cent. below the highest attainable is accepted. With K = 120,000, the deficit is only about 1½ per cent.

There are many forms of turbines now on the market, but we need only consider four chief types, which are :—

First, the compound reaction turbine, with which we have been dealing, representing more than 90 per cent. of all marine turbines in use in the world, and about half the land turbines driving dynamos.

Secondly, the de Laval, which is only used for small powers.

Thirdly, the "multiple impulse compounded," or Curtis, which has been chiefly used on land, but which has been fitted in a few ships.

Lastly, the compound reaction type, with one or more "multiple impulse elements" added to replace the reaction blading at the high-pressure end.

We may dismiss the numerous other types as simply modifications of the original type, without any scientific interest.

Let me explain the latter types. The multiple impulse principle is the only substantial innovation since the compound reaction and the de Laval turbines came into use. It was proposed by Pilbrow in 1842, and first brought into successful operation by Curtis in 1896. A little consideration should be given to it as involving some characteristic points of difference from what has been said about reaction blading. It will be seen that Curtis used the de Laval divergent nozzle, and that he also uses compounding, but generally only 5 to 9 stages as compared with 50 to 100 in the compound type. The same principles as regards velocity ratio apply, but owing to the repeated transfer of the steam between fixed and moving buckets at each velocity-compounded stage, the best velocity ratio in a four-row multiple impulse is only one-seventh, and the best obtainable efficiency 44 per cent., and therefore much lower than reaction blading under favourable conditions.

The good points of the multiple impulse type are that there is very little loss by leakage, and that therefore, in spite of its low efficiency, one or more multiple impulse wheels can in certain cases usefully replace reaction blading at the entry to the turbine, because in slow revolution turbines of moderate power the blades are short at the commencement, and there is consequently much loss by leakage through the clearance space. As a rule, one multiple impulse wheel is generally preferred, and is followed by reaction blading ; the expansion ratio on to the wheel is about threefold, and it generates about one quarter of the whole power. Occasionally several wheels in separate chambers are placed in front of the reaction blading, but there are serious practical drawbacks to this arrangement. The multiple impulse wheel at the commencement has a further advantage in that, when highly superheated steam is used, the temperature is much reduced by expansion and work done before it passes to the main turbine casing.

The highest efficiency yet attained by land turbines has been with the pure compound reaction turbine of large size, where the high-pressure portion is contained in a separate casing of short length and great rigidity; the working clearances can then be reduced to a minimum.

The first turbine imported into Germany in 1900, of 2000 horse-power, was on this principle, and also the latest turbines, of 12,000 horse-power, which generate current for the Metropolitan Railway in London.

In marine work the same arrangement has been almost universal since 1896, when the original single turbine of the *Turbinia* was replaced by three turbines in series (on the steam) on different shafts.

Here there is the additional advantage that, owing to the power being subdivided over three shafts, smaller screws are admissible, and the speed of revolution may be increased in the ratio of 1 to $\sqrt{3}$.

Generally, the turbines are placed two in series, as in cross-Channel boats, the *Mauretania* and *Lusitania*, torpedo craft, battleships, and cruisers, or sometimes three in series, as in the liner *La France* and the latest and largest Cunard liner now building. Four in series have been proposed, but not constructed.

The war vessel in commission is working at reduced power for most of the time, and on long voyages economy of fuel is of great importance. For this purpose, additional turbines are fitted in front of the main full-power turbines. They are naturally of small size, and may be in separate casings, or the main high-pressure turbine may be lengthened by the addition of a cruising portion added on in front. All these cruising turbines or cruising elements are more or less by-passed, according as additional power is required, and at full speed they are entirely by-passed, and when in separate casings are connected to the condenser and rotate in vacuum. In some instances of modern naval construction one or more multiple impulse wheels have constituted the cruising element.

Before passing to the consideration of other applications of the turbine, I should like, with your permission, to repeat an experiment which illustrates the phenomenon of cavitation. The chief difficulty in applying the turbine to marine propulsion arose in the breaking away of the water, or the hollowing out of vacuous cavities in the water when it was attempted to force the screw above certain limits. The phenomenon was first observed by Sir John Thornycroft and Mr. Sydney Barnaby. In order to avoid cavitation, which involves great loss of power, propellers in all fast vessels are now made with very wide blades covering about two-thirds of the disc area, which gives a very wide bearing on the water, and prevents its giving way under the force.

In models, and in vessels of moderate speed, the forces are not sufficient to tear the water asunder, but if the pressure of the atmosphere is removed, a model screw will cavitate at a comparatively moderate speed.

The marine turbine, with the modifications we have so far described, is only suitable for vessels of more than 16 knots sea speed, and to make it suitable for the remaining two-thirds of the tonnage of the world has been our constant aim. The first plan to this end to be adopted is somewhat in the nature of a compromise, and is called the combination system, because the reciprocating engine is used to take the first part of the expansion and the turbine to complete it. From what we have said it will be apparent that this coalition of the reciprocating engine and turbine is a good one, because they each work under advantageous conditions. The reciprocating engine expands the steam to about atmospheric pressure, and the turbine carries on the expansion with high efficiency down to the pressure in the condenser. Now, though a large and high-speed turbine deals with the high-pressure portion of the expansion as economically as a reciprocating engine, a slow-speed turbine cannot be made to do so; but, on the other hand, a slow-speed turbine expands low-pressure steam much further and better than any reciprocating engine. In this system the turbine develops about one-third of the whole power.

About fifteen years ago I filed a patent for the system, but, with the exception of fitting the British Admiralty destroyer *Velox* in 1902, few steps were taken towards its application until the turbine had become firmly established for fast vessels, because we feared the technical public would say, " You are trying to bolster up a failure of the turbine." About three years ago Messrs. Denny, of Dumbarton, who in 1901 built the first mercantile turbine vessel, the *King Edward*, built the first combination vessel, the *Otaki*, of 9900 tons deadweight capacity and 13 knots sea speed. She has ordinary twin screws driven by triple-expansion engines exhausting into a turbine driving a central screw. The initial pressure at the turbine is 9 lbs. absolute, and it generates one-third of the whole power. The combination vessel was found to consume 12 per cent. less coal on service than her sister vessel *Orari* on the same service, fitted with quadruple reciprocating engines.

The next combination vessel was the *Laurentic*, of 20,000 tons, built by Messrs. Harland and Wolff, a sister vessel, the *Megantic*, being fitted with quadruple engines, and on service at the same speed the saving in coal by the combination is 14 per cent.

Messrs. Harland and Wolff are also fitting the combination system in the White Star liners *Olympic* and *Titanic* of 60,000 tons displacement, and some other companies at home and abroad are also adopting the combination system.

There is another alternative solution which promises to extend the field of the turbine further over that of the reciprocating engine.

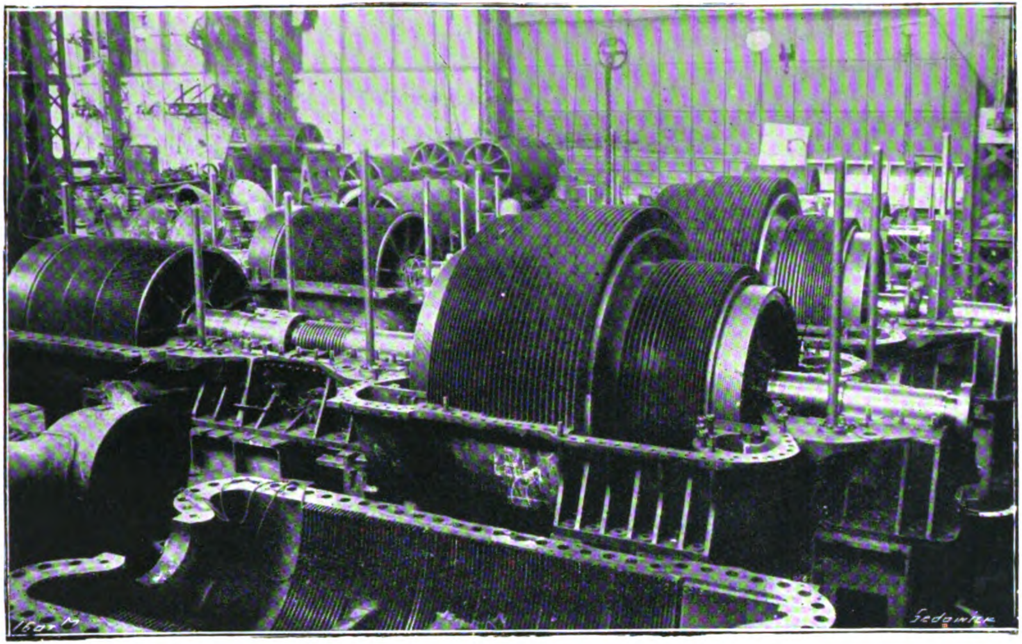
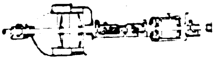
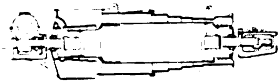


FIG. 1.—TURBINES BEING COMPLETED. (From *Engineering*.)



T.S.Y. TURBINIA



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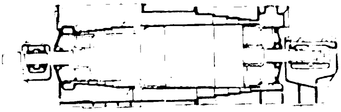
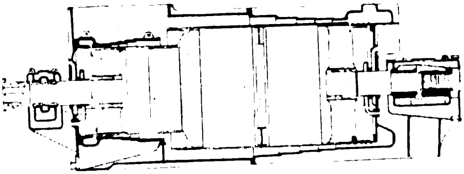
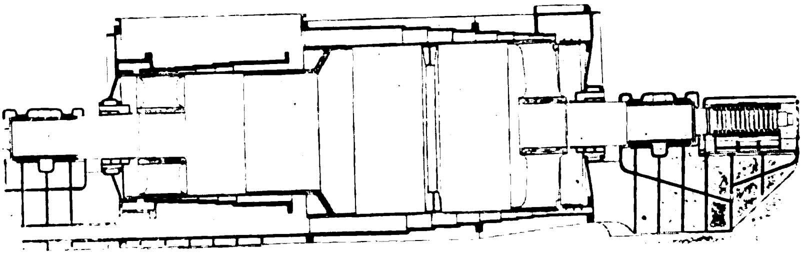


T.S.S. THE QUEEN



T.S.S. VIRGINIAN



T.S.S. ATLANTIC LINER 20.000 I HP

Fig. 2.—Progress in Size of Turbines. (From *Engineering*.)

We mentioned before that de Laval had in the 'eighties introduced helical tooth gear for reducing the speed of his little turbines. For twenty-three years it has worked well on a small scale. Recent experiments, however, have led to the assurance of equal success on a large scale for the transmission of large powers of many thousand horse.

After some preliminary experiments some years ago on helical reduction gear, which showed a mechanical efficiency of more than 98 per cent., a 22-feet launch was constructed in 1897; the working speed of the turbine was 20,000 revolutions per minute, which was geared in one reduction of 14 to 1 on to the twin-screw shafting driving the propellers at about 1400 revolutions. The speed attained was 9 miles per hour, and this little boat was many years in use as a yacht's gig.

The next step was the purchasing of a cargo boat in 1908, the *Vespasian*, of 4350 tons displacement, and triple expansion engines of 900 horse-power. After thoroughly overhauling and testing her existing machinery for coal and water consumption, the engines were replaced by geared turbines, the propeller, shafting and boilers remaining the same. On again testing for economy a gain of 15 per cent. was shown over the original machinery, and subsequent minor alterations have increased this gain to 22 per cent. There are two turbines, a high pressure and a low pressure, each driving a pinion at 1400 revolutions, gearing into a main wheel on the screw shaft making 70 revolutions per minute. The gearing is entirely enclosed in a casing, and is continually sprayed with oil by a pump. Ordinary centrifugal governors on the turbines control the speed, and because of the enormous angular momentum of turbines (some fifty times that of an ordinary marine engine) the acceleration is so slow that the governors have time to act, and consequently no racing has ever occurred in the heaviest weather, and it is certain that if geared turbines come into use there will in their case, as in other turbine vessels, be no more cases of broken screw shafting as has hitherto been common with reciprocating engines.

The vessel has now been carrying coal from the Tyne to Rotterdam for about a year, and has covered about 20,000 miles and carried 90,000 tons of coal across the North Sea. The pinion on the lecture table was specially removed from the vessel last week for this lecture, and shows a wear on the teeth of under $\frac{2}{1000}$ in. in this time, and its life will therefore be equal to or greater than that of a vessel.

Gearing promises to play an important part in war vessels for increasing the economy at cruising speeds. We explained the difficulty in obtaining good economy at the high-pressure end of marine turbines, and in replacing such portions by geared high-speed turbines we have a complete solution. The Turbinia Company are now constructing two 30-knot destroyers of 15,000 horse-power, wherein the high-pressure portion and cruising elements are geared in the ratio of 3 to 1 and 5 to 1 respectively to the main low-pressure, direct-coupled turbine. Their use will increase the radius of action of the vessels at cruising speed to a very considerable extent over that of any similar destroyer without gearing. Similar gearing is proposed for warships, with similar prospective advantages.

Gearing may also find a place in cross-Channel boats and liners for the high-pressure portion of their turbines, but the greatest material gain will be in extending the use of turbines to vessels of slow speed.

Gearing enables very high coefficients to be used in marine work at full speed, and good coefficients at all speeds without much increase in weight, and under such conditions a geared high-speed reaction turbine is much more efficient at the high-pressure end than the multiple impulse wheel or wheels we have considered, and will probably dispense with their use generally. Gearing in marine and land work promises to give to the turbine a level consumption curve like that of the gas and oil engine. Half a century ago nearly all screw vessels had mechanical gearing, one element being composed of wooden teeth, for gearing up the speed of the engine. Subsequently the speed of engines was increased, and gearing abandoned. Now a very slow-speed turbine is an impossibility, and accurately cut steel gearing seems to be a permanent and satisfactory solution.

Low-pressure turbines worked by the exhaust steam from other engines are coming into general use on land under the name of "The utilisation of exhaust steam," for they utilise what was formerly a waste product, the exhaust steam from non-condensing engines.

They are generally employed in the generation of electricity or in the working of blast-furnace blowers and centrifugal pumps and gas forcers, but recently an exhaust turbine of 750 horse-power has been applied to driving an iron-plate mill in Scotland. The turbine revolves at 2000 revolutions per minute, and by a double reduction of helical gears drives the mill at 70 revolutions. A fly wheel of 100 tons weight revolving at the same speed as the rolls equalises the speed. During each rolling the turbine and flywheel collectively exert 4000 horse-power, the maximum deceleration at the end of each roll being only 7 per cent.

So satisfactory has gearing proved up to the present on a small and also comparatively large scale, that it seems probable that by its use turbines will be more widely adopted in the future for power purposes generally.

There are at the present time just above 6,000,000 horse-power of marine turbines completed and building, and also an equal horse-power of land turbines of the compound reaction type.

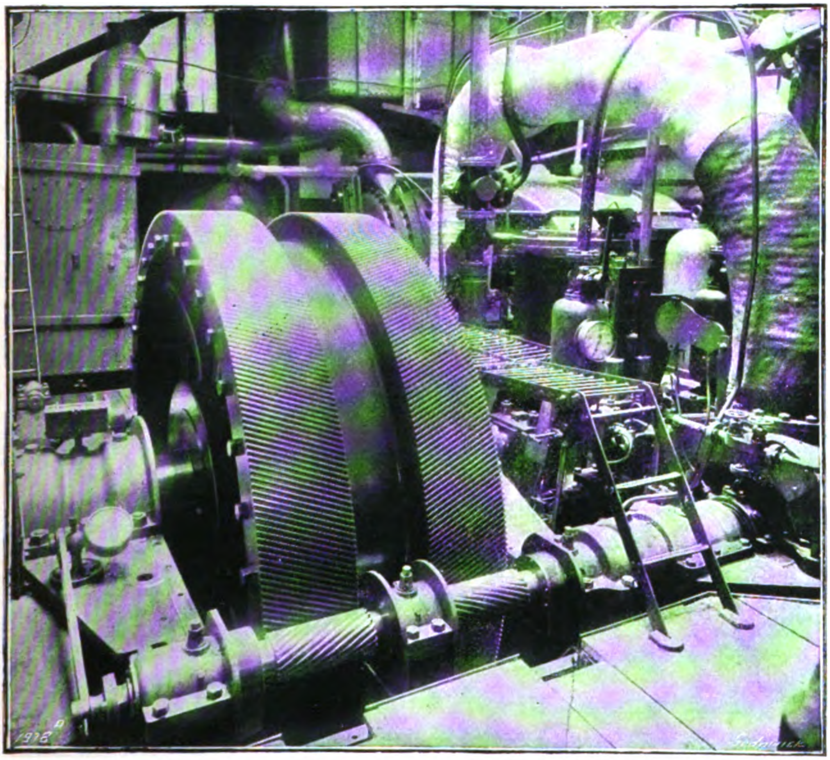


FIG. 3.—THE GEARING OF THE *Vespasian*. (From *Engineering*.)

WEEKLY EVENING MEETING,

Friday, March 17, 1911.

GEORGE MATTHEY, Esq., F.R.S., Vice-President, in the Chair.

JOHN H. BALFOUR BROWNE, Esq., K.C. D.L. J.P. *M.R.I.*

Water Supply.

THE question of water supply is in one aspect a scientific one, and in another aspect a political one. The source of all water supply is evaporation, which raises and purifies water which is taken up from the land and the sea, which after condensation is returned to us as rain, dew, snow and hoar frost, and these waters are to be found ready to our hand in springs, streams, lakes, and in the envelope of earth which is tapped by means of wells. In early days the water supply was a matter of hand to mouth. In the matter of water, at any rate, men drank water when they were thirsty—unlike the characters in Maeterlinck's "Palace of Happiness," who were, you will remember, the Luxury of Drinking when you are not Thirsty and of Eating when you are not Hungry. In the old days people, in relation to these ordinary articles of diet, acted upon the advice given in that old-world book "Sandford and Merton," and "only drank when they were dry." But even in the old days men in this country used water occasionally for washing, although the modern passion for baths had not developed in the dark ages, but even then there seems to have been a physiological cleanliness which the best advertised soap does not give. We find, however, that even in these early days there was a political aspect in water supply. The existence of springs in many cases determined the sites of cities. Many towns have been built on rivers partly because they were sources of water supply, but mostly when the rivers were navigable and afforded a highway for ships. Now, however, it is found that populations have increased to such an extent in certain localities, owing to the gregariousness of men and other political considerations, that the immediate sources have proved inadequate, and great towns in this country—like Rome in ancient days—have had to go a distance for their water supplies, and have had to construct great engineering works for the conveyance of water to the area of distribution. Water is at present collected and sold in England to a value of nearly £8,000,000 annually, and when it is delivered at the house of the consumer it costs him about 2*d.* a ton.

Aqueducts, or channels by which water is conveyed along an inclined plane, were known to the Greeks, but there are no remains of those they constructed. The Roman aqueducts were amongst the most important of their great works, and the present supply of Rome is still carried by these artificial rivers, sometimes through passages cut in the hills, sometimes on arches bridging the valleys and carrying the water across the plains. One of these aqueducts is 62 miles in length. We in this country have had to go even further afield for our water sources. A large portion (56 per cent.) of the supply of Liverpool is brought from the river Vyrnwy, in North Wales, a distance of 68 miles. Leicester is 60 miles from the sources of the Derwent Valley Water Board supply; Birmingham gets its water from Radnorshire, a distance of 74 miles; and Manchester from Thirlmere, by means of pipes and aqueducts, a distance of 96 miles. Paris derives some of its water from the Champagne district through pipes and aqueducts 80 miles in length, and some from Vanne, a distance of 104 miles. There has, too, been a suggestion that London should draw its public water supplies from Wales, which would involve carrying the water about 200 miles. This scheme was first suggested by Mr. Bateman in 1867. He proposed to collect the rainfall on 204 square miles, and, by means of an aqueduct 173 miles in length, to bring 230 million gallons of water a day to London, and he estimated the cost at £11,400,023. About the same time, too, there was a suggestion to carry the water of Ulleswater and Hawswater, which it was said could supply 550 million gallons a day from an area of 100 square miles to the Metropolis, supplying Liverpool, Leeds, Bolton, Bury, Blackburn, Huddersfield, etc., on the way. These great ideas were of course too large to be realised in these small times, and many of these towns have, since the suggestion was made, supplied themselves with water by means of comparatively small scale works instead of becoming parties in a national undertaking.

The difficulty of meeting the demands of such large towns is obvious, from the fact that it involves such great works and such heavy expense to secure an adequate supply. To-day, the ratepayers of Birmingham are paying not only water rates, but contributing out of the ordinary rates £64,000 a year to meet the heavy annual charges in connection with their Welsh scheme. Such a fact indicates that for places far from the sources of supply, and with wealth comparatively small to that of the great Midland towns, the difficulty of securing any supply for their future wants has become increasingly difficult, and may soon become impossible; and in this aspect the question of the future water supply of our populations becomes a significant political question, and because it is a matter of real importance to the health and trade of our great town populations, it has received no attention at all at the busy hands of our platform politicians. And yet, in my view, no matter is more worthy of serious consideration and attention, none which is more urgently practical, than the question of the future water supply of England. At one time England was able with its rich fields to feed its own populations, but, as trade prospered and populations increased, it was found impossible to produce food-stuffs sufficient for our people, and at present probably five-sixths of the total food of the people is imported from abroad. It is in this connection that current politics has taken in hand the problem how we are to continue to obtain these supplies from abroad; and while one school of politics thinks that the future is assured to us so long as the price of the loaf is not increased, another recognizes the necessity of earning sufficient here to enable us to buy our food in other markets. But in relation to water supply we are in a worse predicament. We must depend for that on the rainfall of our own lands, and the improvident way in which our sources have been squandered in the past, the way in which the long arm of wealth has been allowed to appropriate sources which may not naturally or geographically belong to the community in question, the exhaustion of local sources, and the waste of underground water which takes place in connection with the mining operations of England, has much complicated the great question, and has made the future of Britain as to water supply both precarious and serious.

I have pointed out that the sources of supply are from springs, streams, and wells which tap the underground sources. There are in some quarters objections to rivers—full grown rivers—as a source of supply, largely due to the fact that communities with insanitary rashness and short-sightedness have thrown their refuse and filth into streams, and made them the carriers of sewage. This matter was fully discussed recently in Parliament when the Great Yarmouth Water Company endeavoured to secure an additional supply of water to the town beyond that which it then drew from Ormesby Broad. We know, of course, that the rainfall in the Eastern counties is much less than in the West of England and Wales, and the company had been advised by most competent engineers that the most suitable source of supply was from the River Bure. The population above the proposed intake was very small, only one person to four acres, but still it could not be said that no sewage did find its way into the river. But even this insanitary indiscretion is condoned by Nature, and rivers, especially rapid and turbulent streams, have a way of burning off effete matter which is put into its liquid charge. Whether this process of purification is absolutely effective or not is still a moot point, and chemists and bacteriologists are divided as to the safety in any case of drinking water which has been subject to sewage pollution. One gentleman holding that in a few miles a river will get rid of all sewage by its "cold ablution"; another that there is not a river in England long enough to get rid of a pathogenic germ. The great experiment of London has failed to convince some of these experts. London derives the bulk of its water from the Thames. In the Thames watershed, above the Water Board's intake, there are at least 1,000,000 people and about 800,000 other animals. The London water is supplied to nearly 7,000,000 people. This is obviously a large experiment, for there are about as many people in Water London as in the two kingdoms of Norway and Sweden, about the same population as there is in widespread Canada. The water mains of London, according to the chairman of the Board, would reach from London to New York and back; and yet, notwithstanding the supply of river water, the health of London is exceptionally good. Indeed, there are some persons who seem to think that river water is really more wholesome than any other, and there is an interesting statistic produced in proof of this assertion. The death rate of Great Yarmouth, which, as I have said, now takes its water from the Bure, is 15 per 1000; Chester, which is supplied from the Dee, 15·4 per 1000; and Greater London, which drinks water from the Lea and the Thames, has a death rate of 13·3. But against this the death rate of the great towns which have pure hill waters for their supplies are, in the case of Birmingham, 15·9; Manchester, 18·2; and Liverpool, 19·2. If statistics were absolutely convincing, the case for river water as against hill water would seem to be made out. But we must weigh statistics, and not allow them merely to count. It might almost as reasonably be suggested by anyone, who was an opponent of municipal trading, that the results were due to the fact that in the three first cases the water was supplied by companies and in the three last by corporations.

But, apart from any such questions, it is obvious that Thames and Lea water as supplied to London is far from being an unsatisfactory drinking water. But it is only fair to remember that just as in economics there is no such thing as "raw material," so in the case of our raw waters the water as delivered is in most cases a manufactured article.

It was always understood that mere sedimentation carried down a certain number of the germs which were contained in water, but the experiments of Dr. Houston and others show that millions of these germs in water artificially infected with cholera vibrios are dead at the end of a week's storage.*

It is under these circumstances that the Metropolitan Water Board has abandoned the idea of going to Wales for its supplementary water supply, and proposes, by a Bill in the present Parliament, to obtain power to construct a chain of reservoirs for the purpose of decanting the raw river water at Staines, and to spend £6,900,000 on this great scheme which is to supply the wants of Greater London for the next thirty years—until, indeed, the population of the Metropolis may be twelve millions.

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* Dr. Houston has found, too, that even a week's storage of raw river water is an enormous protection against the "cultured" and "uncultured" bacilli of typhoid fever, and "that less than a month's storage is an absolute protection against typhoid fever."

But I was referring to the immense difficulty that any comparatively small town has in our days of securing a pure supply of hill water. So great is the difficulty that, as I have said, the Metropolitan Water Board has properly hesitated to go to Wales, having spent £47,000,000 in the acquisition of the London Water Companies—the enormous difficulties of a Welsh scheme seem to have been too great even for the gigantic financial resources of London. It is not, therefore, a matter for wonder that a town like Great Yarmouth has to look to some near source of supply for the further wants of the town, and as I have said they were advised to have recourse to the Bure. There were the usual objections to river water, and in this case it was urged that the river which drains the Broads is in summer the home of a large floating population in house-boats and other craft. It was, on the other hand, said that the same objection might be made against the Thames, for the Thames above the in-takes has, in a momentary lapse into poetic diction, been called "the water park of London." But here again the health of London was in evidence, and in the case of Yarmouth power was taken to prevent any house-boat anchoring within a considerable distance of the in-take.

There was another objection urged to the taking of water from the Bure for town supply. When the wind was in the north-west the waters of the German Ocean were heaped up by the spade-work of the gusts, and when that happened at the same time as a spring-tide the waters of the Bure were held or backed-up, and it was said that owing to the mixing action which takes place between sea and river water, the waters of the river at the point of intake would be salt or brackish. It was argued that it was ridiculous to supply a river water impregnated with chloride of sodium to two towns like Yarmouth and Lowestoft. But here science came to the help of the water company. The occasions when the north-west winds and the high spring-tide synchronised were of course very rare, and it was proposed by the Bill that whenever such an event took place and when there were more than 20 grains of common salt to the gallon (that is in 70,000 parts) in the Bure water, the company should cease to pump from the river, and supply the town only from the stored water of the Ormesby Broad. Sir William Ramsay, too, invented for the occasion a little instrument. It was a small glass cell, containing two copper plates. This was to be sunk in the river, and as long as the plates were in contact with the fresh river water no electric current passed between the plates. But when salt water was substituted for fresh, and it was sufficiently salt to have 20 grains of chlorine to the gallon, an electric current passed and rang a bell. If this little apparatus was placed in the river two or three miles below the intake, there would be timely warning of the uprush of the sea water, and it was explained that it could be made not only to ring a bell but to stop the pumping-engine. This apparatus was exhibited to the Lords' Committee, and, upon salt being added to the water, the bell rang.

I remember one of the Counsel in the case who was a cynic said, "Ah! Sir William, if you had been before the Royal Institution it would not have worked." That is the reason I have for not repeating the experiment before you to-night.

That 20 grains of chlorine to the gallon was not a serious matter was proved by the statement that Apollinaris water, of which some of the members were partaking at their frugal lunch, contained 59 grains of chlorine to the gallon.

It may be of some use if I say something about the sources and methods of supply. There is nothing very new about water supply. Even in deep wells we have been anticipated; Joseph's Well at Cairo is 297 feet deep, and some of the wells in China have gone to a depth of 1500 feet. In modern times we have in some cases been abandoning our well supplies. At one time almost the whole supply of Liverpool was drawn from wells in the New Red Sandstone. To-day she only draws 7·36 per cent. from wells—36·42 per cent. from Rivington, and 56·22 per cent. from Vyrnwy. There is a well at Passy, near Paris, 1923 feet deep, and it delivers 5½ million gallons of water a day. In South Dakota there is a well which penetrates the earth's crust 725 feet and raises 11½ million gallons a day. In relation to the purity of such underground supplies, many of them in this country are derived from the chalk, and it is interesting to note the precautions which Nature has taken to purify such supplies. It is found that such soils as chalk breathe air and expel gases just as the human lungs do. The breathing is long-drawn and irregular, and depends mainly on the barometric pressure of the atmosphere. But that such breathing takes place can be shown by the simple experiment of closing the folding doors over a chalk well and holding a lighted candle to the bucket rope-hole, and the sensitive flare will show the indraft or outdraft as the case may be; which varies, of course, in intensity according to the extent of recent barometric changes. When water has to be got from underground sources, then a well has to be sunk, adits driven, a pump established, and the water raised to the clear-water tank. In some cases, however, Nature not only does the purification of our water by its chalk lungs, but does our pumping for us. The artesian well which gets its name from Artois in France, is a well which is sunk to a pervious water-bearing strata lying between two impervious layers, as in diagram. In that case the water falling on the surface and getting into the ground at O will, when the well is sunk through the surface impervious stratum at the points A and B, rise to the surface, Nature doing the lifting for us; and even at the point C the water is within easy reach of our pumps.

But not only have we been anticipated in the matter of wells, in aqueducts we are mere imitators of our predecessors, who even understood, it is obvious, the principle of the inverted syphon, as it is called, by means of which water is carried in pipes across valleys running down hill on the one side and up hill on the other; for Lyons, in France, was supplied long ago by means of lead pipes from 12 to 18 inches in diameter, 9 miles in length, and worked under a head of 200 feet. It is true that the favourite method of engineers before the nineteenth century, when cast-iron pipes came into use, was to cross valleys by bridge aqueducts, and this, apparently on the ground that the materials of their pipes (either trunks of elm-trees hollowed out from which our word "trunk main" has survived to us, or lead) did not lend themselves to the conveyance of large volumes of water as the ordinary aqueduct did. The favourite system of supply in this country to-day is undoubtedly by means of a gravitation system, either from natural lakes like Loch Katrine or Thirlmere, or from artificial lakes—or, as they are called, "impounding reservoirs." These reservoirs, by means of a dam formerly formed of earth, with a core of impervious puddled clay, but now more frequently of masonry, catch and impound the water which falls upon the gathering ground A. Of course it is desirable to avoid a gathering ground which consists of cultivated land or upon which there is any considerable population. The best gathering ground is one which is composed of impervious rocks—for porous strata steal too much water—and which is covered only with mountain pasture or moorland. In many cases no objection is made to the existence of sheep upon the gathering ground; but Liverpool, in the case of its Rivington works, has purchased the whole of the gathering ground, and, after destroying and pulling down many of the farms and buildings, has kept a great part of the gathering ground free from sheep and let it to a sporting tenant. The largest gathering ground in this country dealt with by water works is the Birmingham gathering ground, which will collect the water from 44,000 acres, while the Thirlmere scheme of the Manchester Corporation has only a contributory area of 11,000 acres, and the Vyrnwy works of Liverpool Corporation only 22,000 acres.

Hundreds of dams have been built in this country to collect the waters from gathering grounds in connection with water supply to towns, or water supply to canals and waterways. The wall which impounds the water at Vyrnwy is 85 feet high. The Manchester Corporation, which had to deal with a natural reservoir, constructed a dam only 50 feet in height, but the masonry embankment at Caban Coch, the reservoir of the Birmingham Corporation, is 122 feet high above the bed of the river; and some of the other dams of that great scheme, when complete, will be 128, 120, 101, and 98. These walls, of course, have behind them immense quantities of water varying from 8000 million gallons, in the case of Birmingham, down to very small number of gallons which run down a mill goit. The tensile strain or tear of such a mass of water as that at Caban Coch is enormous, and in that case the work is strong enough to bear such a strain up to 12 tons per square foot. The Bouzay dam, near Epinal in France, which was bad in design and faulty in construction, gave way with the pressure of $1\frac{1}{4}$ tons to the square foot.

On January 13 of this year a dam of a reservoir containing 250,000 cubic metres of water, which belonged to the Huelva Copper and Sulphur Company, Spain, owing, it is said, to a hidden spring under the masonry, gave way, and eleven people were drowned. That is near to-day, and affects us like a new wound. But even if none of us have memories which go back to 1849, tradition has told us of the bursting of the Bradfield Reservoir, which drowned the town of Sheffield, and put the country round for a distance of 12 or 14 miles under water. In that catastrophe 250 lives were lost, and property was destroyed to the extent of £327,000 in value. The Holmfirth Reservoir, which had an embankment 90 ft. high and 150 yards long, after heavy rains burst in 1854, and 100 people were drowned in that night, and property valued at £600,000 was destroyed. In most of these cases, however, the calamity can be traced to the defective engineering skill which went to the construction, or subsequent carelessness in the maintenance. But many engineers would tell you that they have sleepless nights when one of these great cauldrons are filling with water for the first time—and people in the valleys below may well hold their breath until the stability of these great walls has been proved. In the case of the Bradfield Reservoir the burst reservoir filled the Don Valley with a mad flood. In the town of Sheffield the water rose to the height of the roofs of low buildings. Dead cattle were carried down by the waters, and in some cases deposited on house-tops, and as in the days of Horace "fishes roosted in elms." One incident of that great calamity is not uninteresting. When the flood was at its highest an old box-cradle came sailing on the dirty waters, and in it there was an infant safe asleep. No one claimed the little Noah, or his ark; no one knew what had become of its father or mother, although it was assumed that they and all who knew the new comer, were amongst that 250 dead. The parish had to adopt the foundling, and with a sense of propriety which is rare in parish authorities, they had the boy christened John Flood.

The dam on the diagram is shown at B, and the impounded water in the reservoir C. The water there stored in the supposed case has to supply the town, which the artist has indicated by a church spire, but has also to supply what is called the compensation water to the stream R. Before the reservoir was formed, the riparian owners had all the water that fell on the gathering ground A passing their land. Now, however, that the reservoir is there, the greater part of the water is to be carried away in pipes and supplied to the towns. The Waterworks Clauses Act, 1847, seemed to contemplate the compensation of any person who was injured by the construction of the works, by which the water was diverted, in money; but it has been found more convenient to compensate them in water. The arrangement usually made in such cases is that by a careful compilation from the rain gauges on or near the gathering ground A. or from gaugings of the stream at the site of the embankment, the amount of water which flowed down the stream and is available for supply is ascertained. If this amount is calculated from the careful statistics of Dr. Mill, or from rain gauges, calculations have to be made as to the amount that is lost to the stream by evaporation or absorption, and even then the calculations have to be carried further to ascertain the amount which could be obtained by storage in three consecutive dry years. When the amount of storage water has been arrived at in inches of rainfall per annum, it is usual to give one-third of the rainfall in compensation to the stream, and take two-thirds for the town supply. At first sight this seems an unfair bargain, like the lawyer who takes the oyster and gives the client the shell. Here is the town taking the whole of the water and paying the owner by giving him back one-third. But the effect of such a bargain is clearly beneficial to the riparian owners. The promoters have to make a greater reservoir than would be necessary for the town supply in order to store the winter floods, and have to find the capital for that. By storing the winter floods in the reservoir, the riparian owners, who have land liable to flooding, are benefited, and those who use it for power are doubly benefited, for their mills are not drowned for so long a period in winter, and by reason of the compensation water, which, of course, much exceeds the minimum flow of the stream before the construction of the reservoir, they get more worth out of the water in the summer. It means, therefore, that the bargain which has been given effect to in a hundred Acts of Parliament is a fair one after all. But even when the amount of compensation has been determined, there have to be elaborate provisions and arrangements to secure the due discharge of the right number of gallons. The scheme has, even after the compensation has been discharged, to provide for the conveyance of the water to the area of distribution. That is done by means of a conduit, shown upon the diagram by figures 1, 2, 3, 4, 5. One to two, and five to six is conduit, open cut, or covered cut. When a hill intervenes, it is tunnelled at a gradient, as between 3 and 4, and when valleys have to be crossed, the ground which falls below the hydraulic gradient, by pipes 2 to 3 and 4 to 5 under pressure. At 6 the water is delivered by gravitation into a high level service reservoir, thence it is put upon the filter beds, and from there it is drawn into the clear-water tank, and it is from that tank supplied to the town.

In case of spring waters, these are collected where they issue from the earth from their underground recesses in protected tanks, and from these the water, which, in many cases has been sufficiently filtered by Nature, is conveyed to the clear-water tank as in the case of surface water. But when, as in the case of surface water, Nature does not produce a ready-made article, and the water is liable to surface pollution, then filtration is a necessity, and as in many other cases in this connection our ancestors were wiser than they knew. Indeed, experience is often more valuable than science, and is universally the foundation upon which all safe science is built. Sand filters have been used in Britain since about 1829. These filters are tanks from 6 to 8 feet deep. Over the floor drain pipes or channels lead to the outlet pipe, and over these we lay a layer of broken stones and gravel. These stones are laid to a depth of 2 or 3 feet, the larger stones being at the bottom of the tanks, and the smaller bearing the 2 feet of sand which was supposed to be the filtering and purifying medium. The water is allowed to percolate downwards at a certain slow rate, and the effect is to remove mechanically certain matters in suspension. For many years our chemical experts saw little or no value in sand filtration, because in chemical analyses they found little or no difference between the filtered water and the unfiltered raw water. But it was left to bacteriologists to find the real significance of sand filtration. We know now that after use a jelly-like deposit is formed on the top of the sand, and that that film has prevented the water getting through the filters at the ordinary vertical rate of 4 to 6 inches an hour, or about 2½ million gallons a day per acre of sand. One ingenious engineer had the jelly scraped off the surface of his filter, and the water flowed more freely certainly, but there were within a few hours urgent telephonic messages from the bacteriological department announcing the arrival of thousands of bacillus coli, and that the water supplied by that company was not fit to drink. The fact is that it is the organic slime which is formed on the top of the sand, and which the sand is only useful in supporting, that is the effective agent in filtering the water, for the organic slime destroys the micro-organisms which are in impure water. It is in this respect that our ancestors in using their sand filters were wiser than they knew.

We know that Nature used to be thought red in tooth and claw, and no doubt lions and tigers and ravening wolves appeal to our trembling imaginations. But it is the still small animals that are the real danger. We are taught by science to tremble at the sight of the common house fly—the missionary of typhoid and diarrhœa ; or the flea from a certain rat which inoculates the plague. But it is the micro-organisms, which are to be found in contaminated waters, and which are so readily distributed by our system of town supply, that are in fact the most dangerous foes of the human race. We have seen, however, that Nature purifies our chalk waters, and also works with a will to save us pumping, but here again Nature by her bacteriological laboratory toils for the health of the human race. M. Duclaux has well said, " Whenever and wherever there is decomposition of organic matter, whether it be the case of a weed or an oak, of a worm or a whale, the work is exclusively performed by infinitely small organisms. They are the important, almost the only agents of universal hygiene; they clear away more quickly than the dogs of Constantinople or the wild beasts of the desert the remains of all that has had life, they protect the living against the dead. They do more; if there are still living beings, if since the hundreds of centuries the world has been inhabited life continues, it is to them we owe it." So, too, it comes about that while the micro-organisms which cause cholera, typhoid, and tubercle, which are so rapidly conveyed to us by means of water, and then exercise their fateful activities, which bring us disease and death—there are beneficent bacteria which "come to succour us who succour want," and these are applied as a bastion or a defence against our enemies by means of the at one time despised sand filter. I think it was Napoleon who said that a wise general should not fight too often with the same enemy, for the enemy was apt to learn too much from his implacable foe. We have fought often with the bacillus of cholera and typhoid, and have learned of our battles. Nothing could be more tragic in the way of instruction than what took place in Hamburg and Altona in 1892. Both Hamburg and Altona are dependent for their water supply on the Elbe, but the intake for the Hamburg supply is above the town; the intake for the Altona supply is below Hamburg, at a place below the point where the sewage of that town, with its 800,000 inhabitants, is discharged into the river. The Hamburg supply had, therefore, a great initial advantage over that of its neighbouring town. But the cholera epidemic scourged Hamburg, and the Angel of Death passed very lightly over Altona. In Hamburg the deaths from cholera amounted to 1250 in 100,000, and in Altona to only 221 per 100,000 of the population. Where the division between the two towns was only the imaginary line down the centre of a street, and the houses on one side of the street, being in Hamburg, were supplied with the above-town water, and the houses on the other side were supplied by the below-town water, the cholera visited with fatal results the houses on the Hamburg side, while those on the Altona side were free from the disease in this new Passover.

These haggard statistics are to be accounted for only by the fact that the foul sewage-polluted water of the Elbe which was supplied to Altona was carefully filtered, while the comparatively pure water taken above Hamburg for the supply of that town was not. Altona had the protection of the micro-organisms in its sand filters. Hamburg had no such protection, and suffered accordingly.

A similar experience in relation to another water-borne disease—typhoid—has been put on record by the Massachusetts Board of Health. There in 20 years, from 1856 to 1876, the death rate from typhoid in that State was 8·6 per 10,000 of population; in the years between 1876 and 1895, when private wells had been given up and a public supply of filtered water been substituted, the death rate was only 4·1 per 10,000, and between 1896 and 1899 the death rate went down to 2·6 per 10,000. The State Board report: "The death rate from typhoid fever has generally fallen as the percentage of the population supplied with public water has risen, for the reason that the majority of the deaths from this disease have occurred among communities and portions of communities not supplied with public water."

But an examination of our own Thames water at Hampton showed there were 1644 micro-organisms in 20 drops of water, and the Thames water, after passing through the sand filters, was found to contain only 13 such organisms in the same number of drops. The discovery of the 1644 germs was at the time so startling that the shares of one of the water companies dropped in value; and I think these 1600 did valiant duty in the arbitration which had to determine the value of the company's undertakings when they were being transferred to the Water Board.

But notwithstanding these startling vindications of sand filters, the great town of Chicago takes its water from Lake Michigan, which receives the untreated sewage of various towns having in the aggregate a population of over two million people, and has not thought it necessary to subject its water to any preliminary purification before distribution. That town has, however, with curious inconsistency, diverted its own sewage from the lake, but it has undertaken that sanitary improvement only in connection with the commercial undertaking of the drainage and ship canal.

We know that one of the riddles of the politico-economic platform is "What is raw material, and what is a manufactured article?" But we have seen sufficient to see that "raw water" is quite a rare commodity, and that most of our waters are manufactured articles. Even Eastern countries like China and India have long "doctored" water with alum to get rid of clay by coagulation. But in this country not only do we get rid of the turpidity of water by sedimentation, but we purge the waters of micro-organisms, including pathogenic germs, by storing in large reservoirs as well as by filtration. Very hard waters, waters containing lime and magnesia, are treated and softened by what is called Clarke's process. Everyone knows that chalk waters fur boilers and kettles, and that the bicarbonate of lime is precipitated by boiling; but Clarke's process consists of a chemical method which expels chalk by chalk, and is an ingenious application of science to the practical purpose of softening water which lengthens the life of boilers and saves soap. But, again, water may be too soft. Hill waters are sometimes too soft to be palatable. Water at Keswick is under half a degree of hardness; Loch Katrine water, 1 degree; Thames and New River water is about 14 degrees.

But it is found that distilled water or soft lake or river water acts with extreme rapidity upon lead, and many cases of lead poisoning have occurred in consequence of persons drinking waters which have been in contact with the lead of which distributing pipes are, for the most part, constructed. It has been said that sand filters remove the lead, but at present it is the rule to treat, or "doctor," these very soft waters, as is done at Sheffield, where, to overcome this difficulty, from half to (rarely) three grains of powdered chalk is added to each gallon of water with excellent results. It has been found, too, that loam or clay remove by merely carrying down organic or inorganic impurities from water ; and it is certain that the precipitation of lime which takes place in Clarke's process has some effect in the same direction, although, from experiments made by Dr. Percy Frankland in connection with the Cambridge Water Bill of 1910, it does not seem to be a thoroughly effective method of purification. In connection with that Bill, a suggestion was made that suspicious waters—waters drawn from wells in the chalk in close proximity to certain villages—could be made perfectly safe by the chlorinization of water ; and it was stated that the ozone process which has been adopted in relation to certain waters forming part of the supply to Paris is also a useful and protective process in cases where waters are liable to organic pollution, and in which the danger-signal of bacillus coli are found. These instances are sufficient to show that water to be potable must pass through the hands not only of the engineer with his filter and storage, but also through the hands of the chemist and bacteriologist ; in fact, water, unlike the poet, is made, not born.

It is little more than a year ago that Paris was suffering from the drowning floods, which called, I think, for a Mansion House Relief Fund, and just before Christmas the south of Lincolnshire was under water, and the town of Newark stood out like an island in a sea which stretched 25 miles or nearly, from Nottingham on the one side to Collingham on the other. At the same time, after the rains there was a leak in the artificial bank of the Glen, which caused the flooding of the whole of the Bourne Fen. The whole country was a sea, with a sad archipelago of villages and farms.

Here, then, there was enough of water flowing down from the hills of Derbyshire to spoil the prospects of a harvest, to ruin the potato crops of these fat fens, and in a few months from now we may hear, as we often do, of some great Lancastrian town having been put on the short commons of two hours a day for its water supply, and of its having only a small number of days' supply in its reservoirs. These two aspects of the rainfall surely call for some national action. As to the Fens, all that is suggested is the deepening of the rivers which take the waters to the sea. Within the last two years the Trent Navigation Company have taken 300,000 tons of material out of the River Trent, which, of course, has had the effect of modifying the floods to some small extent. But, of course, almost the whole of the lands in these districts have been won from the sea and are kept from the clutches of the sea, and land waters only by means of embankments and drains.

The creed of Socialism is that as no one made the land, or the minerals in the land, no one can be the owner of them, and the reverse of that curious coin is that labour makes all wealth; therefore, to labour all wealth belongs. But these gentlemen should see the lands of Lincolnshire, and they will discover that they have been as much created by the labour of men and the capital of owners as a steam-engine or a boat. But the lesson we should learn from these floods, I suggest, is not only that we must find a means of getting rid of land waters, which is poor economy in a country which, in parts, suffers at times from severe periodic droughts, but that we must come to some comprehensive conclusion as to a scheme by which, while training the fury of the floods of winter—by domesticating, as it were, our wild animal "water," and holding it in reservoirs—we may have such a deposit as will prevent floods, and tide us over periods of dry weather, and so make what is at present the "curse" of rains, the blessing of abundant water supply. This principle has been recognized in all our great impounding schemes, but it wants to be carried into higher politics, as an economic principle in relation to our water supply. But to do this effectively it must be done not on a parochial, but on a national scale. The importance of the management of water sources and of distribution upon a large scale is only now, it seems, beginning to dawn upon the obscurity of public opinion. In the early days water seemed to be everywhere, and even when that delusion had been routed by dry summers, the matter was still treated as a purely local question. A town was on a river, and it dipped into it. It was on a water-bearing strata and it sunk a well, but as I have shown by instances it is evident that the purely local treatment of the water question must be abandoned. Great towns, as we have seen, have gone to great distances and have appropriated areas which may in the words of the Duke of Richmond's Commission of 1869 "naturally and geographically belong to districts nearer to such sources." And these enterprises have raised the large question which, as I said, is in a sense political—the question of national water. The importance of this larger treatment of the great subject first dawned upon the commercial instincts of men. Reservoirs were formed in many of the Yorkshire and Lancashire valleys in which the mill-owners banked, as it were, the winter floods to keep their wheels going in the summer droughts, and before the days of railways when canals were still real highways and not the peaceful sluggards of rural districts, where with the lazy bulging barge they form the easy subject of the sketcher's art. In those days when they were the sole highways of commerce, many reservoirs were formed in the hills to feed the canals with the water which was lost in lockage. Nothing could better illustrate the importance of "large maps" in relation to water supply than a passage from recent commercial history. During the last year the trade of our eastern ports diminished. These ports are largely dependent on the trade of the Rhine, the Elbe, and the Weser, which with their important canal connections still form *the* important highways for the commerce of Germany. That falling off was accounted for mainly by the fact that the various Continental canals had been too scantily supplied with water, and people were surprised to find that that want of water was coincident with, and was in fact caused paradoxically, by the wetness of the summer. The explanation of the paradox is easy enough. These great waterways, some of which bear barges of 600 tons burden, are dependent for their water supplies not upon local rainfalls, but upon the great bank of Europe—the snows of the Alps where, as in a reservoir, the winter's moisture is kept in a safe of ice until the summer with its heat melts the lock. But in the wet, cold, inclement summer the ordinary amount of melting had not taken place. The rivers ran low notwithstanding the rainfall, and commerce was impeded for want of water in these great ducts of trade. That fact should teach us the importance of larger views of the question than pettifoggers have been entertaining. Here is the commerce of Grimsby and Hull and Goole dependent upon the storing of the snows of the Jungfrau, and the height of the thermometer in Switzerland. We in England do not possess such a natural reservoir as the snows of the Alps, and must have recourse to other expedients if we are to do the best for this matter of water supply.

There is, at the present time, a good deal of sporadic information as to the water supplies and resources in various localities, and mining engineers have, from their experience, some knowledge of the subsoil or underground waters, for these, of course, are the enemy with which they have to contend in their operations, but there is no general survey to determine what are the supplies and what are the water resources of this country; there is no general knowledge as to the underground water supplies. We know that in many districts these are being pumped for supply; in many where mining is going on they are, with reckless economy, being pumped to waste. But what is required is a comprehensive knowledge both of the overground and underground reserve forces for water supply, and until that is prepared any legislation with regard to water supply must be merely hand to mouth, unscientific, and futile; and this seems to have been the wise opinion of Mr. Lithiby, of the Board of Trade, who gave evidence before the Joint Select Committee on the Water Supplies Protection Bill, which sat and reported during the last session of Parliament.

The necessity for the acquisition of such knowledge is emphasised by the proceedings and Report of the Royal Commission which has been inquiring and reporting upon canals and waterways since the year 1906. No one can say that the investigations of that Commission have not been exhaustive, although many may think that the Reservations of Lord Farrer and three other Commissioners seem to show that their labours will prove absolutely futile. But the Commission has gone further, and proposes to improve the waterways of England, and these great new or improved canals are to connect the Midlands and South Staffordshire with the estuaries of the Thames, the Humber, the Mersey, and the Severn. These four routes, which are, after all, only to be large barge canals, suited for barges of, in one scheme, 100 tons burden, and in another of 300 tons burden, are, in the Report, referred to as the "cross," and if this gigantic scheme is carried out at an expense, according to Sir John Wolfe Barry's estimate, of, for the small scheme, £13,393,483, or for the large scheme of £24,513,823, certainly England would be financially crucified. But criticism of that imaginative proposal forms no part of my present purpose. It is only interesting to me to note that after the Commission had adumbrated this idea, and ascertained approximately the cost of constructing the "cross," which, as I have said, would be a cross greater than England could bear, they bethought themselves how they were to get water for their canals—in the deplorable absence of the Alps—and they instructed an engineer to survey and inquire and to give them an estimate of the cost of getting the water. I have no doubt he did his work as well as he could. He found ready to his hand the admirable statistics as to rainfall which are collected by Dr. Mills, but complains, rightly enough, that "other questions connected with the national water supplies appear to receive less attention." Of course, it is quite an exception to find anywhere river gaugings, and the engineer in question says:—"This inquiry has shown the necessity, if such problems as those which the following Reports attempt to solve are to be thoroughly investigated in future, of some public authority being charged with the duty of recording the flow of rivers, and of the proportion of the rainfall available or run off in catchment basins overlying different geological strata in various parts of the country."

But this claim to water for canals which, according to the reporter, would involve an expenditure of £1,194,000, without including the cost of obtaining the power or the cost of water compensation, and which is, of course, in addition to the sums estimated for construction by Sir John Wolfe Barry, raises again in an acute form the whole question of our national supplies, and points to the absolute necessity now of some systematic dealing with this great question. The nation is being forestalled by municipalities, and here is a suggestion that a Canal Board should lay a gigantic hand upon some of our sources of supply. The time for dealing with the matter is *now*; but, as in other cases, it is quite likely that the matter will be postponed until it is "too late." Indeed, in many directions, those last tragic words of Gordon at Khartoum seem to be adopted as the motto of England.

We all know that the British pride themselves on being a practical people. But we often believe ourselves to be possessed of virtues we have not got. One of the best indications of the practical ability of the English people as a nation is to be found in connection with legislation. When a matter begins to stir the minds of the public, the politicians invariably think that postponement is the best policy, and, acting upon this principle, they appoint a Royal Commission, a Departmental Committee, or a Joint Select Committee of the two Houses, and after that the matter is never heard of again. There are more questions buried in blue books than bodies in the catacombs. The Committee or Commission thinks it is furthering the great purpose of reform, and it makes a Report, and the sound dies away and silence resumes her disturbed possession. As you will understand, the question of water supply was just one which would be interred with the bones of a Royal Commission. The matter has been in the minds of restive people—who really are like the angels of the Pool of Bethesda, and stir the waters of healing—for many years. There was a Royal Commission on water supply in 1869, which saw that there were large questions involved. There was also a somewhat similar recommendation in the Report of the Select Committee on the Public Health Act (1875) Amendment Act in 1878. Nothing has resulted from these recommendations, but the introduction of some dead letter clauses into the Manchester, the Liverpool, the Birmingham, and the Birkenhead Acts. But the recommendation was, in effect, a recognition of the necessity of considering the needs of districts, and a more comprehensive treatment of the whole subject.

The Sewage Commission's Third Report contains a statement of the Commissioners that "in our opinion a properly equipped authority is essential, and we unhesitatingly recommend the creation of such an authority." That recommendation was made in 1903 (Report, para. 44), and I need scarcely say nothing has come of it. There was recently introduced into Parliament a somewhat crude measure called "The Water Supplies Protection Bill," which sought to alter the law of underground waters to endow landowners with rights to waters which were not in defined streams which they do not at present possess, and a few other trifles of legislation. The Bill was referred to a Joint Select Committee, and the inquiry was useful and instructive. The Report for the most part gave the go-by to the fads of the measure, but the Committee took an exceedingly sensible view in relation to the great question of National Water Supply. They reported on the desirability of conserving in all reasonable ways the water supply of the country. They recommended "the creation of an organisation empowered to inquire into the whole question of surface and underground water supplies from a comprehensive standpoint, to supervise the future allocation of supplies, and to serve as an authoritative adviser of Parliament in the consideration of particular schemes." This was such a reasonable proposal that I have no hesitation in predicting for it the oblivion such proposals mostly achieve.

But this Committee, not deterred by the wrecks of other reports which lie along the whole shore of legislation, went on to say that there was "an urgent need for a survey at once comprehensive and in detail of the water supplies and water needs of the country, and for the adoption of measures for conserving the supply and disposing of it to the best advantage."

This, again, seems a dictate of common sense, and will probably be disregarded by any Government which is in, or comes into power, for Governments naturally look more to the conserving and disposing of votes than of waters, even although it seems certain that in the past we have, like a spendthrift nation, been playing ducks and drakes with our water capital. Here I find myself in eager agreement with that parliamentary voice crying in the wilderness of contemporary politics. I know, however, that it is not in the graveyard of the lobbies, but in the streets of public opinion, that reforms are inaugurated and pushed forward. It is because there is a real Referendum to public opinion—and because public opinion makes an echo which reverberates futilely for a time in St. Stephen's, but when the diapason becomes loud enough makes itself heard even in these lethargic quarters which think not of the necessities of the nation, but of the loaves and fishes of office—that I thought it worth while to appeal to you to-night upon this great matter of our national water supply.

WEEKLY EVENING MEETING,

Friday, March 24, 1911.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. LL.D. F.R.S., President, in the Chair.

SIR DAVID GILL, K.C.B. LL.D. D.Sc. F.R.S. Pres. R.A.S. *M.R.I.*

The Sidereal Universe.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, March 31, 1911.

The Right Hon. Sir Henry Buckley, P.C. M.A.,
Vice-President, in the Chair.

Professor H. S. Hele-Shaw, LL.D. F.R.S. M.Inst.C.E. *M.R.I.*

Travelling at High Speeds on the Surface of the Earth and above it.

"The Spirit of the time shall teach me Speed."—*King John.*

There are few things so important to man from a material point of view as the power of locomotion; seeing, therefore, that in this respect he is far less well endowed by nature than many, if not most, living creatures, it is no wonder that he has striven from the earliest times to overcome his inferiority by means of mechanical devices. The marvellous results of these unceasing attempts which to-day we enjoy, or, as some people would prefer to say, "take advantage of," are accepted by most of us as a mere matter of course, and we are further apt to assume that the progress which has been so marked during the last century, and particularly in recent years, will continue indefinitely. Now, quite apart from mere locomotion, the question of speed is one of great scientific interest, and, more than this, it is the real test of the power of locomotion. This is not a mere accident, but has its root in something far deeper. The desire for speed is a quality inherent in man, and is doubtless a primordial instinct, the reason for which we see in all other animals, being derived from prehistoric ages. Speed was from the first a necessity of life to enable the weak to escape from the strong and to enable the strong to prey upon the weak, and men depended, just as much as the animals did, for their very existence on fleetness and speed of motion.

From what few and somewhat uncertain records we have of the achievements of man in running in the ancient sports, it does not seem there is very much difference between his powers then and in modern times. As to modern times, we find that for the short distance of 100 yards, and for the longer distance of a mile, the records of twenty-five years ago still stand, notwithstanding the strenuous efforts made to improve upon them on many scores of occasions each subsequent year. Thus we have for the former the record of E. Donovan in 1886, 21·3 miles an hour, and in the same year the record of W. G. George for the mile, 14 2 miles an hour, which have never been beaten; while for one distance, that of 200 yards, the record of Seward in 1847, or sixty-four years ago, still stands. In fact, a study of all the records of twenty-five distances shows that several of them remain unbroken after comparatively long periods, viz. from a quarter to half a century.

Thus, so far as his own unaided powers of locomotion are concerned, man may be considered, for all practical purposes, to have reached long ago the limit of speed possibility. From earliest times, however, he has brought the muscular effort of other animals into his service, and has devoted his intellect towards improving their speed for his own uses. You will see graphically recorded in Fig. 1 the speeds of all the Derby winners from the year 1856, i.e. for more than half a century. The average speed, which may be taken as somewhere above 30 miles an hour, has doubtless slightly increased, but it will be seen from the dotted line which has been drawn at the top of the maximum speeds what comparatively little increase has been obtained for an expenditure of the many millions represented directly and indirectly in the training and breeding of these horses, and it may be reasonably assumed that here again the limit has been reached for the fleetest animal, by the aid of which man can increase his speed of locomotion by using muscular power other than his own.

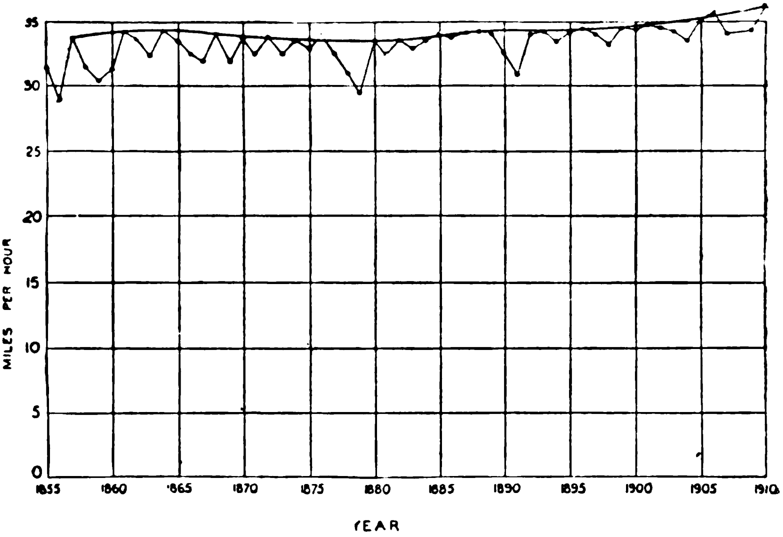

Fig. 1.—Derby Winners for 55 Years.

What, then, are the physical reasons for this limitation? It is not due to the chief cause, which we shall see later puts a practical limit to very high speeds in mechanical locomotion, namely, the resistance of the atmosphere. Neither is it due to the effective work done in movement, since with a body moving along a level plain, i.e. at a constant distance from the earth's centre, this effective work is nil. To understand the matter we must study the nature of animal locomotion. The surface of the earth is rough, sliding along it being obviously out of the question; nature has made provision for animal movement as follows:—one part of the body first rests on the ground, another part supported by this is advanced, being raised clear of the ground, to rest in turn upon the ground and serve in turn as a support, so that the part behind may be raised and advanced to a fresh position. In man and other animals the feet form the points of support for this process; but the same method of locomotion is employed by creatures without feet, which have to crawl or glide, such as snakes or worms.

This process, whether with animals or reptiles, as you will see, involves in the raising of the body an expenditure of work which is not recovered, and further an expenditure of work in stopping and starting some portion of the body in its movements. My assistant now walks in front of the blackboard holding a piece of chalk level with his head, and you will see the rising and falling motion. I have prepared a wooden model to represent the action of his legs, and you will see that these legs, being equal to his in length, produce almost exactly the same curve underneath, so that you have a complete explanation of this movement, viz. the rotation of the hip about the ankle as a pivot. There is a third case of loss, namely, the energy involved in swinging the legs. About thirty years ago the distinguished French professor, Marey, actually investigated the loss involved from each of these three causes, and I have on the wall a diagram in which you will see all three given graphically. The number of steps per minute, you will notice, increases until a pace is reached when it becomes painful to walk faster, and you will also notice from the diagram that at about ninety steps per minute the gait changes to a run, that is to say, a springing action takes place, the hind foot leaving the ground before the front is put down upon it.

I have another diagram showing how the length of stride at first increases with the pace, and afterwards begins to fall off before the walking breaks into a run. The reason why a man or an animal changes his pace at this point is obvious, and it is because a faster speed is possible with a less effort. As the speed of running is increased the total effort becomes greater, but the three elements shown on the diagram are differently divided; the rise and fall element is less, but the work done in swinging the legs is more, while the chief element, in the muscular effort expended, is the loss of energy involved in stopping and starting as each spring reaches a maximum. Time does not permit me to pursue this interesting subject further except to point out that exactly similar causes operate in the natural locomotion of other animals which move on legs.

We therefore now know that the limit of speed is controlled by two factors :—

(1) Physical endurance, owing to the expenditure of work occurring at an increasing rate as the speed is increased.

(2) The physical impossibility of giving a reciprocating movement to the legs quicker than a certain limited period of time.

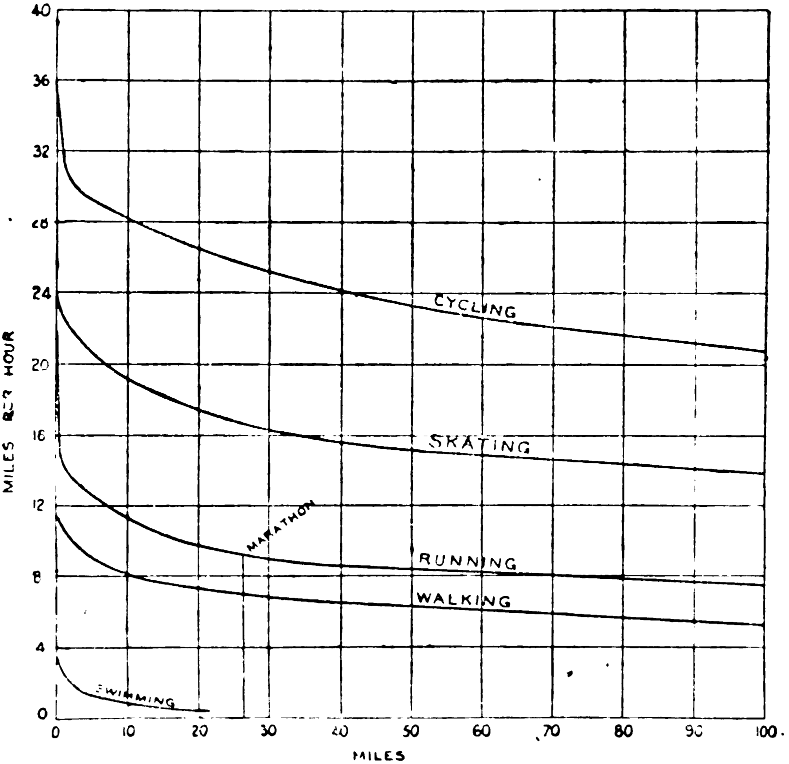

Fig. 2.—Speed Records for Human Muscular Effort.

I have prepared a chart, Fig. 2, which shows the maximum recorded velocities of man's progression in walking and running. The speeds are set up as vertical ordinates, and the abscissæ represent the distances over which the respective speeds were maintained. It will be seen that the maximum speed of walking is about 9 miles an hour for a short distance, but when the long distance of 100 miles is covered, the quickest rate recorded falls to 5½ miles an hour. For running, the quickest speed which I have mentioned, viz. 21½ miles an hour for 100 yards, falls to 7½ miles an hour as the average speed for a distance of 100 miles.

We do not know the speed of the original historical run from Marathon to Athens, but we do know that Dorando ran the modern Marathon from Windsor Castle to the Stadium at Shepherd's Bush, a distance of 26 miles 385 yards in (to be exact) 2h. 55m. 18⅗s., or at the rate of 9 miles per hour, which, you see, fits very well on our curve.

We may notice in passing that in walking fast and starting to run the arms swing in time with the opposite leg, as in the modern picture on the diagram exhibited. In the picture, however, copied on the same diagram from an ancient Greek vase, although the attitude of the legs is the same, it might appear at first sight as if the arms were swinging in the contrary way. As a matter of fact, a closer examination shows that in all the figures on the vase the arms are in the same position, although the legs are in different phases. This seems to indicate that the arms of a Greek runner were held in a fixed position as shown, and, from the position of the hands, with the evident intention of cutting the wind. If this is true, it indicates that even then it was clearly recognised that if there was any effect of the wind it was just as important behind as in front, a matter I shall have to allude to hereafter.

What man can do by his muscular effort in the water is shown by the small curve in the corner. The greatest distance shown (Fig. 2) is about 21 miles by Captain Webb at about 1 mile per hour, although for a short distance it will be seen that a man can swim at about 4 miles per hour. I do not put in flying, because man has not yet flown by his own muscular effort, and flying men to-day are using engines of from 20 horse-power to 100 horse-power, i.e. from 200 to 1000 *man-power*. Gliding *per se* is no more than falling through the air (more or less) gradually, as in a parachute.

Before proceeding to see what man has done to increase his powers of purely muscular locomotion by means of mechanical devices we will study the details of locomotion in the other animals. We are able to do this by the method of Mr. Muybridge, since developed in the invention of the kinematograph, and which was explained by Mr. Muybridge for the first time in this country about thirty years ago in a lecture in this hall.

Take, first, the galloping horse. The lantern diagram shows clearly the various phases in the action of a horse, and shows how the animal is not only able to attain its high speed by its length of stride, but by doing what man cannot do to the same extent—drawing up its body and in springing forward, using alternately its fore and hind feet, so as to get a stride which no two-footed creature could attain on the level ground. I may point out that the kangaroo, though using only two legs, makes effective use of its tail in the spring. The horse springs clear of the ground off its forefeet, only you will notice that it uses both its fore and hind legs as the spokes of a wheel on which it rolls when walking (exactly as man does), though it rolls and swings alternately in galloping. The same kind of diagram could be constructed for the effort exerted at different speeds by the horse, as has been produced by Marey for the man, only the distribution of energy would probably be very different.

Turning next to other animals, it is interesting to observe that a greyhound gets its high speed in proportion to its size owing to the great flexibility of its long body, which enables it to draw its hind legs forward each time for the next bound, and also bound forward both from its fore and hind legs. The other animals in galloping have each the same general kind of movement, although the deer, curiously enough, only bounds from its hind legs, and differs in this respect from the horse; and also it will be noticed the want of flexibility in the body of an animal may be one of the causes of its relatively slow speed. But whether it be man, horse, dog, or any other animal, the same characteristic is found, namely, that locomotion, apart from the bounding action, takes place by a sort of rolling action on the ground. The idea which had persisted since the delineation of horses in Assyrian and Egyptian pictures, that both the fore or both the hind legs are put on the ground simultaneously, is thus exploded. As Mr. Muybridge truly said: "When during a gallop, the fore and hind legs are severally and consecutively thrust forwards and backwards to their fullest extent, their comparative inaction may create in the mind of the careless observer an impression of indistinct outlines; these successive appearances were probably combined by the earliest sculptors and painters, and with grotesque exaggeration adopted as the solitary position to illustrate great speed." As a matter of fact, each leg in turn, as it rests on the ground, stops for a moment just as much as in the forward position above mentioned, and if you watch a dog galloping you can see quite clearly the rolling stroke action I have mentioned.

With the above facts in mind, we can understand exactly the limitations to animal locomotion. In the words of Mr. Muybridge: "When the body of an animal is being carried forward with uniform motion, the limbs in their relation to it have alternately a progressive and a retrogressive action, their various portions accelerating in comparative speed and repose as they extend downwards to the feet, which are subjected to successive changes from a condition of absolute rest, to a varying increased velocity in comparison with that of the body." Hence, all animal locomotion absolutely lacks that continuity of movement, the production of which we shall see is the distinguishing feature and the direct cause of the high speeds attained in mechanical locomotion.

The exchange of the intermittent movement of nature for one having the desired continuity of movement has been effected by means of what is possibly the greatest and yet the simplest of all human inventions, namely, the wheel. The wheel was made and used probably thousands of years before man learnt to replace muscular effort by that of steam and the other forces of nature, the origin of the wheel being absolutely lost in antiquity.

From the models which I now show will be noticed the way in which the wheel acts and how it overcomes the defect of animal locomotion, giving a rotary and continuous movement instead of a reciprocating and variable one. At one and the same time the wheel, therefore, does away with the three causes of loss shown in the diagram as occurring with animal locomotion. The mere use of the wheel has enabled man himself, by his own muscular effort, enormously to increase his individual power of locomotion. The top curve on Fig. 2 shows, in comparison with the other curves of walking and running, his unpaced records on a bicycle, in using which it will be realised that all three causes of loss, which occur in running and walking, are obviated. You will notice a similar difference in speed as the distance varies to that which is made evident in the curves for walking and running. For the distance of 100 miles the average speed is thus only 21 miles an hour, while that for a ¼ mile is more than 35 miles an hour. In view of the results shown by the curve, it is not surprising that the bicycle has entered largely into the conditions of modern life. I am not able to give you any exact figures of the quantity of bicycles turned out each year in this country, but I can tell you that in the Post Office alone there are now 12,000 bicycles employed, and their number is always on the increase; the distance covered on them by men and boys in the year is more than 120,000,000 miles.

I have not dealt with *paced* bicycle records, as such are not the result of muscular effort, but of being pushed along by the current of wind which follows up the pacing machine, such as occurs when a man on a "push" bicycle is paced by a motor vehicle. In a record first set up in America for 60 miles an hour on a bicycle, a man was paced by a locomotive engine, running at 60 miles an hour along a special track; the rider was nearly killed when he tried to drop behind, owing to the whirlwind which was being dragged along by the engine; ultimately his life was saved by his being lifted bodily off his bicycle on to the locomotive. There is no record as to what became of the bicycle.

Curiously enough, records for ice skating and roller skating are almost the same, and far below that on the bicycle, which I think proves distinctly that the reciprocating movement of the limbs limits man's powers, whether he is sliding on the ice or using wheels as with roller skates. This is so, notwithstanding that he carries along with him when on a bicycle the extra weight of the bicycle, but the reciprocating movement of his legs is so slow, owing to the gearing up of the driving wheel, as to give him the material advantage shown by the respective curves. Further, in skating, there is no doubt that the movement of his limbs entails a certain amount of rising and falling, as well as reciprocating motion and consequent loss which occurs in running.

Now, in theory, the wheel is perfect, and in the case of a perfectly hard, circular wheel, rolling on a perfectly hard track, there should be no resistance. This you can well imagine from the lantern model which I now show in operation. In this there is no appreciable resistance, but it is just in this direction that the wheel has defects unknown to nature's methods, since men and animals move upon the ankle joint in a quite superior way to the rolling of an ordinary wheel. In passing I may remark that the more man improves the roads, and the higher his standard of locomotion becomes, the more will he feel the need of a mechanical walking machine (it will be a *walking* machine, though possibly moving at 20 miles per hour) to progress over parts of the earth where roads do not exist, or are still in an evil condition. The better his mechanical appliances for producing such a walking machine, the sooner will this come about, as this is really a vital factor in the solution of the problem. No wheel, however, is quite hard and round, and no road is quite hard and smooth, and there is always an arc of contact, more or less appreciable, which causes a loss, since rubbing takes place instead of true rolling, as shown in the next lantern slide. The next lantern working model I show illustrates the other effect, in which the wheel meets obstacles and is deflected by them from its course, giving exactly the same kind of loss which I showed you takes place with a man in walking, and which is made apparent by making the car write its own record on a piece of smoked glass, exactly as my assistant wrote his record of rise and fall on the blackboard.

Thus there are two ways in which the wheel can be improved :—

(1) Is by perfecting the wheel and hardening the track—and that is the secret of the development of the railway system.

(2) The other is by causing the obstacle to be absorbed in the tyre of the wheel—that is the real secret of the success of the pneumatic tyre.

The working model now on the screen illustrates the latter point, and shows at once how the three causes of resistance to animal locomotion are overcome.

To-day we can replace the muscular energy of man by almost unlimited mechanical power, and Fig. 3 is a comparative speed chart, which I have prepared and which indicates the enormous advance in the speed record which has been made over the best unaided muscular efforts of any animal. It is curious to see that the highest speed ever attained on a railway is closely approached by that obtained with motor vehicles. The records for the latter are as follows :—

The Darracq car of 200 horse-power has done 122½ miles an hour for 2 miles. A Fiat car, driven by Nazarro at Brooklands, 126 miles an hour. A Stanley steam car, 127 miles an hour, and a Benz car has done 127½ miles an hour.

The maximum recorded speeds of a railway were on the experimental line of Messrs. Siemens, on the Berlin-Zossen High Speed Railway, where a speed of rather more than 130 miles an hour was attained. The electric current employed was 10,000 volts, 400 horse-power motors being used. On the Marienfel-Zossen experimental line, the speed attained with 250 horse-power was apparently rather less, though in that locomotive four motors were employed, the current being, as in the other case, 10,000 volts. The lantern diagrams are a picture of the vehicles actually employed, and of the track on which the experiments were made. As a set-off against the

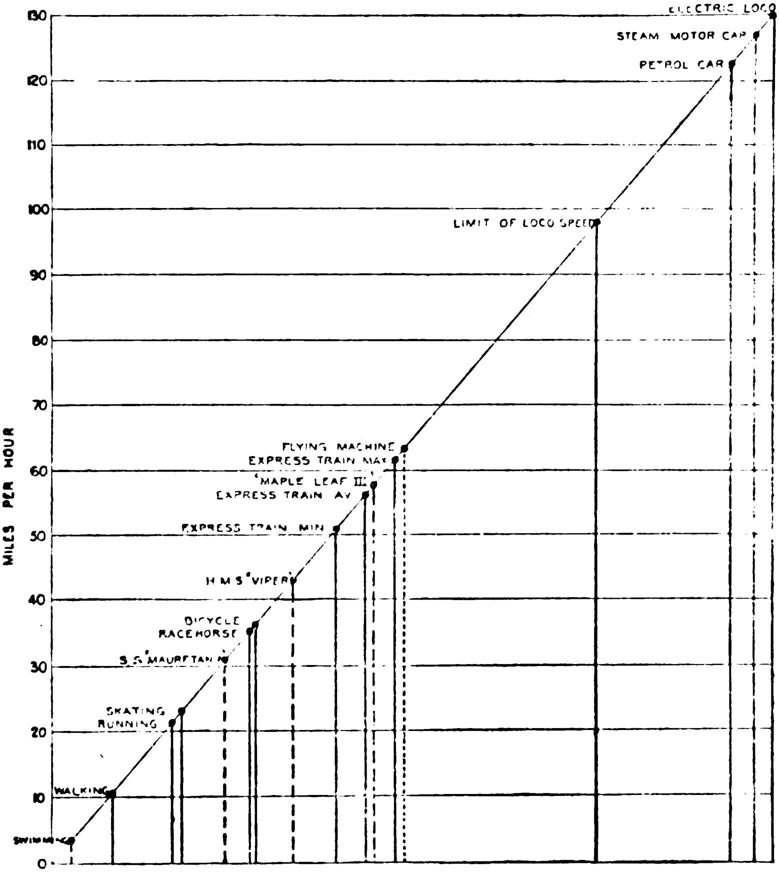

Fig. 3.—Graphical Table of Maximum Speeds.

sober engineering pictures, I show a picture taken from an American motor journal, illustrating a motor vehicle and locomotive at top speed, the former passing on a level crossing in front of the latter.

The foregoing are the record speeds so far obtained of mechanical locomotion, and it will be interesting to see what are the record speeds attained in the other elements. Until the other day, as Mr. Parsons told us in his lecture, the speed on water which has never been exceeded was that of the ill-fated turbine boats, *Viper* and *Cobra*, of about 43 miles an hour. The ship which at present holds the record for speed is the torpedo destroyer *Tartar*, built by Messrs. John Thornycroft, this, under Admiralty tests, giving a speed of 41 miles an hour.

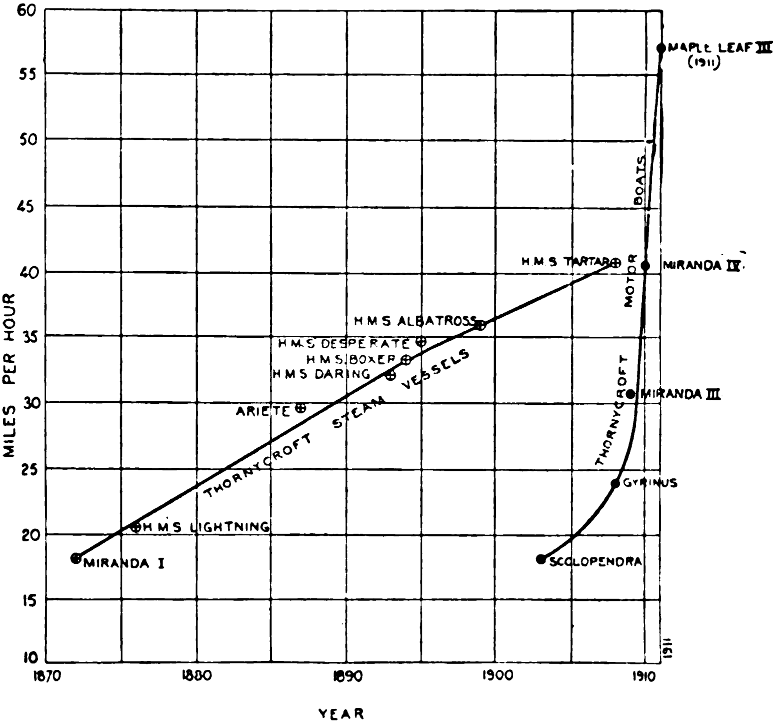

FIG. 4.—SPEED RECORDS FOR THORNYCROFT WARSHIPS AND MOTOR BOATS.

The diagram, Fig. 4, shows in an interesting manner what the progress in speed has been for this class of boats during the last few years, and may be taken as typical, and about which curves Sir John Thornycroft writes as follows :—

"I do not think the curve would be materially altered if vessels of other builders were brought in, although there would naturally be more points on it."

I am able, however, to give you the results to-night of something which has altogether put in the shade even the speeds of the two first-mentioned boats. This has been attained by a boat which, though corresponding in some respects with previous hydroplane boats, has been designed by Sir John Thornycroft to possess a certain amount of seaworthiness. The rate of progress in the increasing speeds in this class of boat is shown on a separate curve, Fig. 4, from which you will see that the celebrated *Miranda* held as a hydroplane the record with the *Tartar* for speed, the *Ursula* also holding the record of about the same speed as a motor-boat. Only a few days ago, however, the new boat *Maple Leaf III.* has attained the extraordinary speed of nearly 50 knots, that is to say, a speed approaching 60 miles an hour, using 600 horse-power to effect this speed. To use a vulgar expression, this certainly smashes all previous records for speed. I do not pretend to give exact figures in this case, because such have not been officially taken, but the statement is probably on the low side as the boat has not been yet properly tuned up. You will see one remarkable thing from the curve, namely, that the rate of progress has been so rapid in this class of boat, and the curve rises so steeply, that in about three months' time there is due from Sir John Thornycroft a boat which will travel at about 100 miles an hour. I am afraid, however, it would not be fair to press this graphical argument quite so far.

Through the kindness of Sir John Thornycroft, and Mr. Edgar, the owner of the *Maple Leaf*, I am able to show both the *Miranda* and the *Maple Leaf III.* The latter, you will see, is travelling at such an extraordinary rate that the water which is lifted up does not fall to the surface again until the boat itself has travelled several lengths away. You may be interested to see a model of this last boat, which has been kindly prepared for me to show to-night, as well as the *Tartar* and *Miranda.* You will notice the form of the *Maple Leaf III.* is that of a steeped hydroplane, which in a modified form was first suggested by Mr. Ramus many years ago; it is the secret of placing the weight, and also the development of light engines giving large horse-power, which has enabled the dream of Mr. Ramus to be fulfilled.

Turning to the last of the three elements, namely, air, it was my intention to have dealt with it at greater length than I now find it is possible to do, but, thanks to the daily Press and illustrated journals, this subject is as fresh in the minds of everybody as it is familiar. It is not necessary in this room to remark that the wild talk of almost incredible speeds has very little foundation. Bodies move quickly enough in the air, and very often far too quickly, but what is generally overlooked is that the difficulty of the problem lies in the matter of supporting the body in the air rather than moving through it, a problem which is very much simpler for land and water. The human body itself, while of about equal specific gravity with water, is about 800 times as heavy as air, and probably, taken in conjunction with the motor and aëroplane, the weight which has to be supported is several thousand times as heavy relatively to the air which it displaces. Inasmuch as the support of the air necessitates the use of an inclined plane and a corresponding expenditure of energy, the speeds made horizontally and independently of the wind have, at the present time, barely exceeded half the record speeds made on wheel vehicles. As a matter of fact, only the other day the record for passenger flight was broken by M. Nieuport at Mourmelon, when he flew with two passengers for 1h. 4m. 58 1–5s., and covered 68·35 miles at an average speed of 63 miles per hour. It is difficult to say exactly what the true record speed at present is round a course, but we may safely take it as probably under 70 miles an hour, the record being, so far as I have been able to ascertain, by M. Nieuport on March 9 this year at Chalons—68 miles 168 yards in the hour.

We now see the relative position of the record speeds in the three elements on our speed chart, Fig. 3, and it is obvious that while on land the speed has been far exceeded of the fastest animal, on water it has probably only recently surpassed that speed, while in the air, in all probability, it is still considerably below it. We must not, however, from this argue that flying speeds will for safe flying machines rise so far beyond that of birds as land locomotion has risen above the speed of animals, for it looks as if the speed records on land would be at least equal for some time, if not greater, than that possible with safety in the air. At the same time, there is no doubt that speed is the one great factor of safety in flying, and aërial speed records are sure to go on rising year by year, but time does not permit me to pursue this subject further to-night.

Instead of vague surmises as to what may be done in the future, let us spend a few minutes looking into the question of these limits.

The two chief things on which the limit of speed in locomotion will depend are:—

(1) The motive power available.

(2) The resistance, and the manner in which those resistances operate.

But inasmuch as we are not merely considering the human body as a projectile, we do not take into account such speeds as have been attained by man in such ways as, for instance, in a high dive, say, of nearly 100 miles an hour, or even the thrilling descents such as are made in a bobsleigh. We must really consider speeds which can be made with safety; and there are two further questions which arise:—

(1) Knowledge as to possible obstacles, coupled with a power of safely stopping within the distance to which our knowledge extends, i.e. signalling and brakes.

(2) Vibration.

These two latter really limit conditions of high speed for practical travelling.

In daily life, the limiting conditions of speed in travelling depend largely on the distance in which we can safely come to rest. As the population increases and there is less room for everybody, the question of brake-power becomes more and more important, and with it, of course, the power of starting from rest quickly, or, to put it in scientific words, the power of rapidly effecting both positive and negative acceleration. We are very differently constructed from the particles of air in which we live, and do not yet travel as fast, but fortunately, as yet, we are not quite so crowded, since, according to Lord Kelvin, they move about amongst each other at the ordinary atmospheric temperature and pressure at an average speed of 1800 miles an hour, and they cannot avoid fewer than five thousand million collisions in every second. As you see in the streets, and as I shall show you with regard to suburban traffic, high speed is becoming more and more a question of starting and stopping rapidly. I remember in the early days of cycle racing, in order to lighten the machine, the racing men had no brake, until they found what is now well recognised—that the speed at which you can travel depends upon the safe distance in which you can stop. I can illustrate this by dropping an egg from the dome of this building, which I can do without causing it any injury, even when it is travelling at 30 miles an hour, if I have proper means for bringing it to rest. I also drop a wineglass from the same height, and bring it to rest quite safely.

Owing largely to the perfection of the continuous brake, the speed records obtained on several railways are from 96 to 98 miles an hour, which I have put down on the diagram, and it is possible that 100 miles an hour has been reached, and even exceeded ; but this is a very different matter from the highest express running which is found really practicable. You will see on the speed chart, Fig. 3, a line indicating the average railway speeds of the fastest running (without stopping) for the fifteen principal railways of the country. The average distance of the quick runs is 51·7 miles, and the average fastest running is 56·2 miles per hour. On either side of this line are the two fastest speeds, namely, 61½ miles per hour for 44½ miles on the North-Eastern Railway from Darlington to York, and the lowest of these is 51 miles an hour, over the 51 miles from Victoria to Brighton on the London, Brighton and South Coast Railway. This shows how little the high speeds of all the railways of this country differ from one another, and indicates, at any rate for the present conditions, the highest speeds of travelling found suitable to our wants.

I will take as another illustration of actual travelling the case of suburban traffic : and we have only time for one example, namely, the traffic from the Mansion House to Ealing on the Metropolitan and District Railway, the details of which have been kindly provided by Mr. Blake, the superintendent of the line. Fig. 5 shows in graphical form the quickening in speed from the opening of the line in 1880 to the present time. You will notice that this increase of speed has been followed by remarkable results; the first immediate result is the possibility of a greater number of trains, and the curve of the rise in the number of trains is shown on the diagram; but the really significant feature is the rise in the number of passengers carried, 35,000,000–72,000,000, which is the direct result of the increased facility in travelling. Now it is in such a case that the importance of the signalling and braking come to be almost pre-eminent, quite apart from the mere mechanical problem.

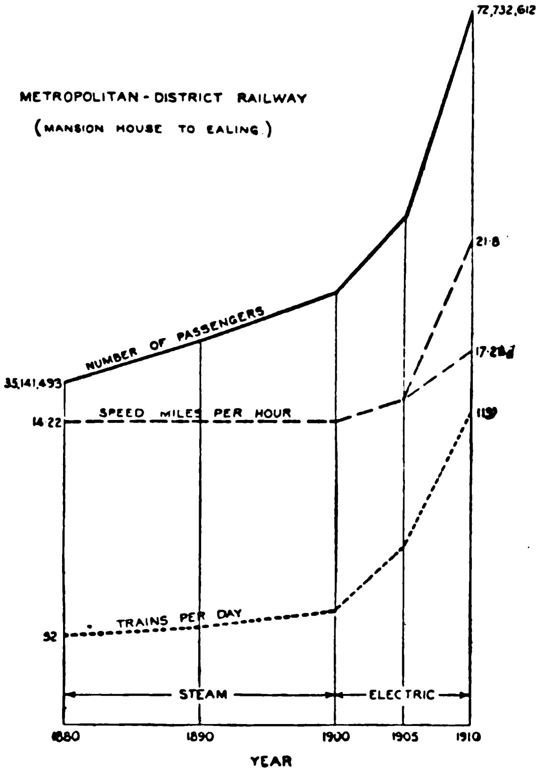

Fig. 5.

I may point out that the District Railway, in common with most other electric railways of this country, has what is known as a "track system of signalling," which, apart from the fact that the driver holds what is known as "the dead man's handle," which upon being released causes the train to stop, the train independently stops itself upon coming to a portion of the line not cleared by the previous train.

I have given you some examples that this country is not so far behind as we are so often told ; and we have another in the fact that the District Railway has created a most beautiful system, by which the signalman is now absolutely independent of fog or darkness ; he can see every train, or rather its picture, as it moves along the track in an illuminated diagram in front of him. No one could watch, as I have had the privilege of doing, the operation of this system in a signal-box without feeling certain that it must become universal in a very short time. You may like to see an actual panel from a signal-box and a view of what the interior is like with the signalman operating, instead of cumbrous levers, only a few small handles.

With regard to the question of vibration and oscillation, these are gradually being diminished as machinery is perfected, and you will see from the model illustration that they are important, and may become very serious. They have for instance, given Mr. Brennan much trouble in perfecting his wonderful mono-rail, with which we shall yet perhaps see every record broken ; and you will remember Mr. Parsons' statement in this hall a week or two ago that an ounce out of balance on the Laval turbine represents an actual pull at the axle of no less than a ton.

There are many other features which I have not time to enter into. There is one, however, which I will briefly touch upon, as it is the secret of our safe railway travelling. I will illustrate the matter by an experiment in which a pair of wheels connected by an axle keyed firmly to both are made to run along a pair of rails. You will notice that the wheels are " coned " instead of having cylindrical rims, and it is easy to see that any movement sideways is at once corrected automatically, and within certain limits no rim at all is required for the flanges in order to keep the wheels upon the rails. The same model illustrates the important property of " super-elevation " applied to the outer rail of a curve. You will see, with proper super-elevation, the wheels run safely round this sharp curve even at a high speed. Time does not permit me to enter at any length on the question of development of power or the nature of resistance to motion. I will content myself with saying that, with regard to the former, we have already seen that the power of flight has been made possible by the invention of the small high-power internal-combustion engine, and it is to the same invention that the marvellous speeds obtained with small boats is due. We can scarcely realise what will be the result when the internal-combustion engine has been developed further for the purpose of locomotion. Our prospects of a further great advance in speed record-breaking appears to lie in this direction, and we already hear of a new car of 250 horse-power with which a speed of 140 miles per hour is confidently expected.

On water, as on land, our actual speed of travelling falls far below maximum speed records, and we do not commercially travel at much more than half the possible speed, as you see from Fig. 3, where the speed of the *Mauretania* is shown graphically. Fig. 6 is a chart of the progress of Atlantic shipping, taking the Cunard line as an example, and these curves indicate that the rate of increase of horse-power and tonnage is rising far faster than the rate of speed, and indicates how relatively highly the rate of power has increased for the gain of speed.

We have now passed briefly in review the nature of the problems which confront us in our continuous efforts to increase the safe and practical speeds of mechanical locomotion. We see that at the root of it all lies the question of artificial power and the harnessing in compact and convenient form the stored-up sources of energy in nature in order to overcome the opposing resistance, and we can realise that, although we have obviously reached the limits of animal locomotion, we are far from having reached any limitation in regard to the speed of self-propelled machines. We see that in all three forms of locomotion, earth, air, and water, the advance has been far more rapid during the last few years than ever before, and we can realise that there is yet a considerable margin by which speed of travelling could be increased as the demand for it is made; and nothing is more certain than that the demand will be made.

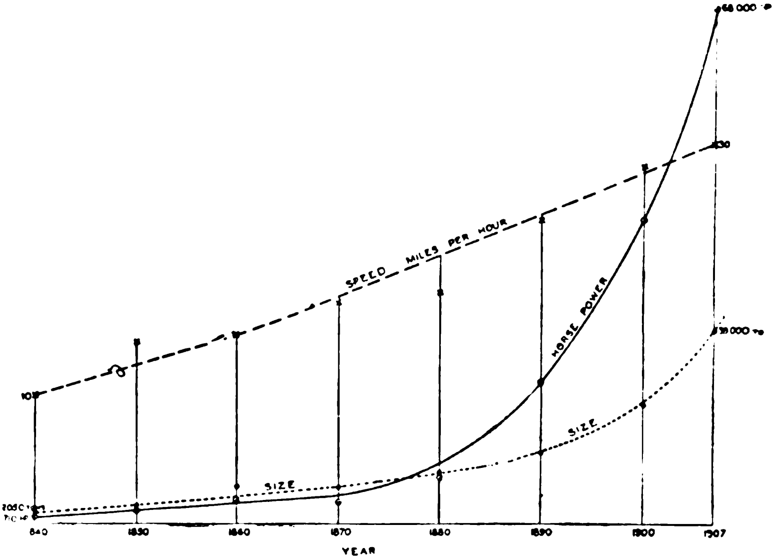

Fig. 6.—Progress in Atlantic Steamers (Cunard).

I began my lecture by pointing out why speed was instinctively taken as a test and a measure of locomotion from the earliest times. Shakespeare makes one of his characters say, "The spirit of the time shall teach me speed," but he might have said this of any period equally with that of King John, though never more so than of to-day, for the changes in the requirements of civilisation have only altered in detail, and speed is of as much importance as ever in the struggle of life. The probably unconscious recognition of this fact has always led questions of speed to be raised as prime factors in proposals for new modes of locomotion, and it is interesting to look back only a comparatively few years to see, in raising these views, this was always the case, but how little any ideas of future possibilities were realised. When George Stephenson, backed up by a few courageous and enterprising men, was fighting the battle of the railway, and in particular trying to secure the passing of the Bill for improved communication between Liverpool and Manchester, the question of speed was the most important one raised; the opposing counsel, Mr. Harrison, spoke as follows:—"When we set out with the original prospectus, we were to gallop, I know not at what rate; I believe it was at the rate of 12 miles an hour. My learned friend, Mr. Adam, contemplated—possibly alluding to Ireland—that some of the Irish members would arrive in the waggons to a division. My learned friend says that they would go at the rate of 12 miles an hour, with the aid of the devil in the form of a locomotive, sitting, as postilion on the fore horse, and an honourable member sitting behind him to stir up the fire, and keep it at full speed. But the speed at which these locomotive engines are to go has slackened: Mr. Adam does not go faster now than 5 miles an hour. The learned serjeant (Spankie) says he should like to have 7, but he would be content to go 6. I will show he cannot go 6; and probably, for any practical purposes, I may be able to show that I can keep up with him *by the canal*. . . . Locomotive engines are liable to be operated upon by the weather. The wind will affect them; and any gale of wind which would affect the traffic on the Mersey would render it *impossible* to set off a locomotive engine either by poking the fire or keeping up the pressure of steam till the boiler was ready to burst." The committee, after hearing the arguments of Mr. Harrison, threw out the Bill for the Liverpool and Manchester Railway by a majority of 19 to 13. In order to realise that the above ideas were general, the following may be quoted from the great journal of the day, *The Quarterly*:—"What can be more palpably absurd and ridiculous than the prospect held out of locomotives travelling *twice as fast as stage coaches?* . . . . We trust that Parliament will, in all railways it may sanction, limit the speed to *eight or nine miles an hour*, which we entirely agree with Mr. Sylvester is as great as can be ventured on with safety."

Even in more recent times we see the struggle for the road locomotion question turned on one of speed, and the supporters of the new departure were unable to make any headway for many years, partly because the speed limit was put at between 3 and 4 miles an hour, that is, the limit of a walking man. A few years ago the speed of 12 miles an hour which, after a great struggle, was obtained, gave place to 20 miles an hour. You can see from the diagrams which Mr. Legros gave in a recent paper before the Institution of Mechanical Engineers, and which have been brought up to date, how the speedier self-propelled vehicle is leading to the disappearance of the horse, at any rate in London, and the difficulty which most people seem to feel is not how to get above the speed limit, but how to keep within it, and the papers show, by a daily crop of sad examples, how only too painfully easy it is not to do so.

Nothing points more clearly to what I have indicated as the basis of our instinctive desire for speed, as the fact that our measure of speed is entirely relative. Thus 60 miles an hour would be a slow speed for a motor-car on a racing track, as seen by the speeds of the motor races at Brooklands last Saturday (April 25th), but this speed, which would be even quite good along the open road to Brighton, would be considered decidedly on the high side for motoring along the Strand. Our ideas of what is slow and what is fast are largely derived from habit, and particularly from surrounding conditions and from our mode of estimation. For instance, we have been carried in this hall during the last hour with the surface of the earth round its axis a distance of about 600 miles. This speed would require a line on our speed chart about as high as the dome of the hall to represent it graphically. But if we judge the speed from observing the apparent rate of motion of the moon and stars overhead, we could never realise this. Far less could we realise by the change in the seasons the speed at which we are travelling with the earth round the sun, accomplishing a distance, as we do, of 540 million miles in 365 days, which represents roughly a distance of 60,000 miles per hour. We have thus travelled together, since we came into this hall, a speed of 60,000 miles. The line required on our chart for this speed would be about as high as St. Paul's Cathedral. But these speeds fall far short of those of certain heavenly bodies with which we are familiar, such as the meteors, some of which are travelling at 160,000 miles an hour, and the recent comet, which probably exceeded this speed one part of its journey round the sun; whereas the fastest speed which man has, up to the present, been able to produce, even in a projectile, amounts to between 2000 and 3000 miles an hour (the Krupp 10·7 centimetre having a velocity of 3291 metres per second, and a 6-inch Vickers, 3190 metres per second). The highest projectile speeds we have attained are thus only about one-tenth of the speed at which Jules Verne fired M. Barbicane and his friends off, in order to overcome the earth's gravity and reach the moon, since the speed he required was 12,000 yards per second, or 24,000 miles per hour.

Such an idea we are quite justified in thinking absurd, but we might have been justified in thinking many of the things absurd which Jules Verne wrote about, only forty years ago, and which have since come to pass. Take "Round the World in Eighty Days." In that case it cost Phineas Fogg 19,000*l.* to take himself and his servant round the world in eighty days. A telephone enquiry of Messrs. Cook an hour or two ago elicited the fact that anyone present can start to-morrow morning and go round the world, with a servant, in less than half the above time, and for less than one-fiftieth of the above sum.

Thus though, impelled by instinct, man will ever continue to strive to increase his speeds of travelling, and with the refinement of machinery and invention doubtless succeed in doing so, it may be safely said that, notwithstanding the still increasing upward angle on some of the speed lines of the charts I have shown to-night, this rate of increase will before long begin to take place at a continually diminishing rate. Such feats as the journey from Paris to London within the hour may be regarded as quite a feasible engineering proposition in the future, though possibly a tube will be used for the purpose, without the employment of wheels, and with a modification of the pneumatic system of that great genius Brunel. We should, however, in doing this journey, be only travelling at half the rate we are actually moving at this spot round the earth's axis, while to do it at the rate we are travelling round the sun, we should only occupy a quarter of a minute. This latter speed, apart from the fact that it is getting very near the point at which meteors fuse with the friction of the earth's atmosphere, seems to be quite outside the limit of the possibilities of artificial locomotion by man ; but how far we shall go towards it who can tell.

NOTE.

Since delivering the above Lecture, M. André Jager Schmidt, of the "Excelsior," has made a tour round the world in 39 days, 19 hours, 43 minutes, 37 seconds. The cost of doing this was 242*l.* 8*s.*, the actual cost of the railway ticket round the world being 115*l.*, the rest being extra payment to ensure expedition.

GENERAL MONTHLY MEETING,

Monday, April 3, 1911.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S.,
President, in the Chair.

James Devonshire, Esq., M.Inst.C.E.
George Herbert Griffin, Esq.
William Elliot Lawson Johnston, Esq.
Mrs. C. Guy Pym,
Albert Edwin Reed, Esq., J.P.

were elected Members of the Royal Institution.

The Honorary Secretary announced the decease of Professor Jacob Henry Van 't Hoff, on March 1, 1911, and the following Resolution passed by the Managers at their Meeting held this day was unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the Scientific World in the decease of Geheimer Regierungs Rath Professor Jacob Henry Van 't Hoff, Ph.D. D.Sc.(Vict.), Hon.F.R.S. Hon.F.C.S., Corresponding Member of the Academy of Sciences, Paris, Member of the Royal Prussian Academy of Sciences, Berlin, Member of the Prussian Ordre pour le Merite, a Nobel Laureate; late Professor of Chemistry in the University of Amsterdam and Professor of Chemistry in the University of Berlin.

Professor Van 't Hoff was one of the Founders of the Science of Stereo-Chemistry, the author of epoch-making works entitled "Stereo-Chemistry," "Chemical Dynamics," "Arrangement of Atoms in Space," "Theoretical and Physical Chemistry," "Physical Chemistry in the Service of the Sciences," and "Chemistry in Space."

Professor Van 't Hoff delivered at the Royal Institution the Raoult Memorial Lecture of the Chemical Society in 1902. He was elected an Honorary Member of the Royal Institution in 1906.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Dyer and Calico Printer for March, 1911. 4to.

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WEEKLY EVENING MEETING,

Friday, April 7, 1911.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Professor Sir J. J. Thomson, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*

A New Method of Chemical Analysis.

I have had on several occasions the privilege of bringing before the Members of the Royal Institution some of the results of the experiments on the positive rays on which I have been engaged for the last few years. I wish this evening to draw your attention to some applications of these to various chemical problems.

The first application I shall consider is the use of these rays to determine the nature of the gases present in a vacuum tube, to show how they can be used to make a chemical analysis of these gases—an analysis which, as we shall see, will enable us to determine not merely whether an element, say, for example, oxygen, is present in the tube, but will tell us in what form it occurs, whether, for example, it is present in the atomic as well as the molecular condition, and whether there are allotropic modifications present, such as ozone, O_3, and other still more complex aggregations.

The method is as follows: the positive rays after passing through a fine tube in the cathode are exposed simultaneously to magnetic and electric forces, the magnetic field being arranged so as to produce a vertical deflection of the rays, while the electric field produces a horizontal deflection. Thus, if when neither electric nor magnetic fields are present, the rays strike a screen placed at right angles to their direction at a point O, they will, when both electric and magnetic forces are at work, strike it at a point P, where the length of the vertical line P N is equal to the deflection produced by the magnetic field, and the horizontal line O N to that produced by the electric field.

We know from the theory of the action of electric and magnetic fields on moving electrified particles, that

$$PN = A\frac{e}{mv} \qquad ON = B\frac{e}{mv^2}$$

where A and B are constants depending on the strength of the magnetic and electric fields and the geometrical data of the tube, e is the charge on the particle, m its mass, and v its velocity.

From these relations we see that

$$\frac{m}{e} = \frac{A^2}{B}\frac{ON}{PN^2}$$

When these rays strike against a photographic plate, they affect the plate at the point against which they strike, and thus when the plate is developed we have a permanent record of the deflections of the rays. The methods of taking these photographs and the details of the experiment are described in my paper in the 'Philosophical Magazine,' Feb. 1911. The values of A and B can be determined accurately by the methods I have given in previous papers, and hence if we measure on the photographs the values of O N and P N, we can determine the value of m/e. If we wish to compare the values of m/e for two different rays, it is not necessary to determine A and B, all we have to do is to measure the values of O N and P N; and thus the photograph alone gives us the means of comparing the value of m/e.

For the same type of carrier m/e is constant, so that $\frac{PN^2}{ON}$ is constant whatever may be the velocity, and therefore the locus of P, i.e. the curve traced on the photographic plate by this carrier, is a parabola. The reason we get a curve instead of a point is that the rays are not all moving with the same velocity, and the slower ones suffer greater deflection than the quicker ones. Each type of carrier produces its own line on the plate, and there are as many curves on the plates as there are kinds of carriers; from an inspection of the plates we can find not merely the number of kinds of carriers, but from the dimensions of the curves we can at once determine the atomic weight of the carrier, and thus determine its nature. This is one of the great advantages of this method. To illustrate this advantage, let us compare the method with that of spectrum analysis. If the spectroscopist observes a line unknown to him in the spectrum of a discharge tube, the most he can deduce without further investigation is that there is some unknown substance present in the tube, and even this would be doubtful, as the new line might be due to some alteration in the conditions of the discharge. But if we observe a new curve in the positive ray spectrum, all we have to do is to measure the curve and then we know the atomic weight of the substance which produced it. To take an example, I have photographed the positive ray spectrum for nitrogen prepared from the atmosphere and that for nitrogen prepared from some nitrogenous compounds, and have found that the former contains a line * which is not in the latter, and that the value of m/e for this line is 40 times that for the atom of hydrogen. We thus know that atmospheric nitrogen contains an element of atomic weight 40, which is not present in chemical nitrogen—this element is, of course, argon. We might by ordinary spectrum analysis have found lines in the spectrum of atmospheric nitrogen which are not in the spectrum of chemical

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* As a matter of fact, there is a second, very faint line for which m/e is about 20 times that for the atom of hydrogen. This is probably due to an atom of argon with two electric charges.

nitrogen, and might thus have suspected the presence of another element, but spectrum analysis could not tell us anything about the nature of this element, whereas the positive ray spectrum at once gives us its atomic weight.

The positive-ray method is even more delicate than that of spectrum analysis, for by it we can detect the presence of quantities of a foreign gas too minute to produce any indication in the spectroscope. I have, for example, often been able to detect the presence of helium by this method, when no indication of its presence could be detected by a spectroscope.

Again, when a line in the positive-ray spectrum can be seen, the atomic weight of the carrier which produces it can be determined with great accuracy. Though the method is only a few months old, it is even now sufficiently developed to determine with an accuracy of 1 per cent. the atomic weight of a gaseous substance, without requiring more than $\frac{1}{100}$ milligramme of the substance. Another very important advantage of this method is that it is not dependent upon the purity of the material; if the material is impure the impurities merely appear as additional lines in the spectrum, and do not affect the parabola due to the substance under examination, and therefore produce no error in the determination of the atomic weight. The method would seem to be peculiarly suitable for the determination of the atomic weights not merely of the emanation from radio-active substances, but also those of the products into which they disintegrate.

The rays, too, are registered within less than a millionth of a second after their formation, so that when chemical combination or decomposition is occurring in the tube, the method may disclose the existence of intermediate forms which have only a transient existence, as well as of the final product, and may thus enable us to gain a clearer insight into the process of chemical combination.

I will now show a few slides prepared from photographs we have taken of the positive-ray spectra. The first is that of nitrogen prepared from air; the measurements of the photograph showed that the atomic weights of the carrier producing these curves were as follows:—

Positive.		*Negative.*	
1	H_+	1	H_-
1·99	H_{2+}	11·20	C_-
6·80	N_{++}	15·2	O_-
11·40	C_+	..	
13·95	N_+	..	
28·1	N_{2+}	..	
39	Arg_+	..	
100	Hg_{++}	..	
198	Hg_+	..	

The symbol H_+ denotes that the carrier is an atom of hydrogen with one charge; H_{2+} that it is a molecule of hydrogen with one charge; N_{++} that it is an atom of nitrogen with two charges; and so on.

With nitrogen from NH_4NO_2 the lines were as follows (the magnetic force was so large that some of the lines corresponding to the lighter particles were thrown off the plate):—

6·1	C_{++}	44·2	CO_{2+}
7·02	N_{++}	65·5	Hg_{+++}(?)
12·08	C_{+}	100	Hg_{++}
14·01	N_{+}	204	Hg_{+}
27·9	N_{2+}		

The next slide is the positive-ray spectrum for CO, and again the magnetic field is so great that the lighter carriers do not appear.

From the measurement of the lines we find that the atomic weight of the carrier is—

Positive.		*Negative.*
6·00	C_{++}	12 C_{-}
6·95	N_{++}	16 O_{+}
7·95	O_{++}	..
12·02	C_{+}	..
13·9	N_{+}	..
15·95	O_{+}	..
28·05	CO_{+}	..
43	CO_{2+}	..
69·5	Hg_{+++} (?) very faint	..
100	Hg_{++}	..
202	Hg_{+}	..

The spectrum for CO_2 is represented in Fig. 6; the atomic weights are:—

5·98	C_{++}	43·9	CO_{2+}
8·00	O_{++}	62·5	Hg_{+++} (?) very faint
12·00	C_{+}	99·6	Hg_{++}
16·00	O_{+}	200·0	Hg_{+}
28·02	CO_{+}		

The spectrum of CH_4, of which a small region with five lines close together is shown in Fig. 4, is interesting, because the measurement of these lines shows that their atomic weights are 12, 13, 14, and 15, 16, and thus that we have here C, CH, CH_2, CH_3, CH_4. If I am not mistaken, this is the first occasion when the atoms CH, CH_2, CH_3, have been observed in a free state.

The spectrum of the analogous compound chloroform $CHCl_3$

is represented in Fig. 7. The atomic weights represented in this are :—

1	H_+	18·5	Cl_{++}
1·5	(?)	27·7	CO
2	H_{2+}	36·0	Cl_+
3	(?)	46·5	CCl_+
6	C_{++}	63	(?) faint
8	O_{++}	81	CCl_{2+}
11·9	C_+	102	Hg_{++}
13·7	N_+	201	Hg_+
16·0	O_+		

The carriers with atomic weights 1·5 and 3 have not been identified. They are of frequent occurrence. I have here two slides, one of SiH_4 and the other of the residual gas in tube after the air has been pumped out, in which they are well marked, though at their best they are only faint lines.

Let us now consider some of the results brought to light by these photographs. In the first place, they show that a gas through which an electric discharge is passing is a much more complex thing than a collection of molecules all equal to each other. Even an elementary gas becomes under these circumstances a mixture of a great many different substances. Thus, to take oxygen as an example, the photographs show that when a current of electricity passes through it, we may have present simultaneously oxygen in the following states :—

1. Ordinary molecular oxygen, O_2.
2. Neutral atoms of oxygen, O.
3. Atoms of oxygen with 1 positive charge, O_+.
4. Atoms of oxygen with 2 positive charges, O_{++}.
5. Atoms of oxygen with 1 negative charge, O_-.
6. Molecules of oxygen with 1 positive charge, O_{2+}.
7. Ozone with a positive charge, O_{3+}.
8. O_6 with a positive charge, O_6.

And, in addition, there are free negative corpuscles. Thus in the elementary gas there are at least nine (the list has no claim to be exhaustive) separate substances present when the discharge passes through it. Each of these substances has almost certainly different properties, possibly a characteristic spectrum. If we took any other gas we should find that the same thing would be true: thus in hydrogen we have H, H_2, H_+, H_-, H_{2+}, even if we do not ascribe to hydrogen the lines giving $m/e = 1·5$ or 3. In nitrogen we have N, N_2, N_+, N_{++}, N_{2+}, carbon occurs as C_+, C_{++}, C_-, chlorine as Cl, Cl_2, Cl_+, Cl_{++} and Cl_-, mercury as Hg, Hg_+, Hg_{++}, and probably as Hg_{++}, as there is a very persistent line for which m/e is about 66.

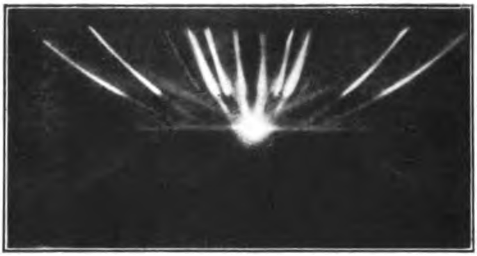


Fig. 1 Nitrogen from air.

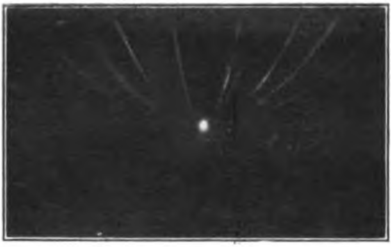


Fig. 2. Air.

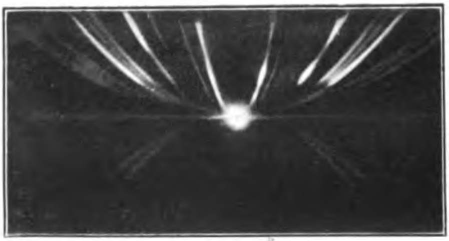


Fig. 3. CO.

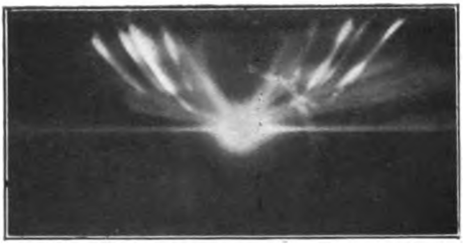


Fig. 4. $CHCl_3$.

As these figures are printed, the magnetic deflecti

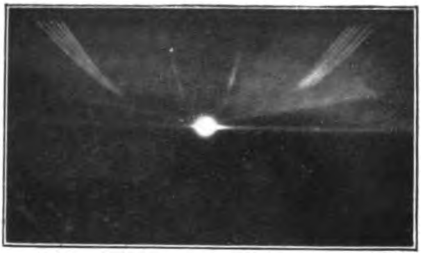


Fig. 5. CH_4.

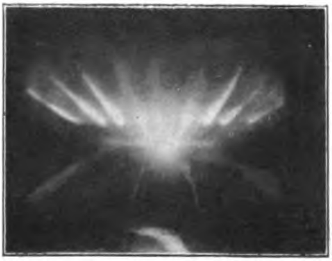


Fig. 6. CO_2.

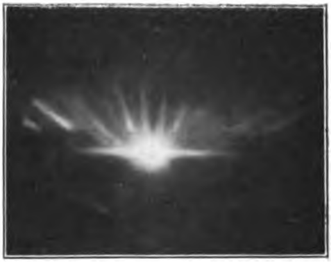


Fig. 7. SiH_4.

c horizontal, the electrostatic deflections vertical.

Thus, whenever the electric current passes through a gas, and probably whenever a gas is ionized, the gas becomes a mixture of many different substances. We can thus readily understand why in the spectra of many elements many of the lines may be grouped together so as to form different series—the principal series, the first co-ordinate series, and so on—and the spectrum of the discharge-tube regarded as the superposition of a number of different spectra whose relative intensities may be subject to very great variations. This, indeed, is just what would happen if some or all of the substances which are present when the gas is in the ionized state gave rise to different spectra.

Another feature which I think is of great interest from the point of view of the theory of chemical combination is the occurrence of particles with negative charges. Let us consider for a moment how these are formed. They are formed after the particles have passed through the cathode; the path between the cathode and the photographic plate contains abundance of corpuscles produced by the ionization of the gas; a neutral particle after passing through the cathode picks up a negative corpuscle and so becomes negatively charged. For this to occur the attraction between the corpuscle and the neutral particle must be exceedingly strong, for it is not a question of a particle at rest attracting to itself a negatively electrified corpuscle sauntering about in its neighbourhood; in our case the neutral particle is rushing past the corpuscle with a velocity of the order of 10^8 cm. per sec. In order that the particle may under these circumstances be able to drag the corpuscle along with it, the attraction between the two must be so great, that to move a corpuscle against this attraction from the surface of the particle away to an infinite distance must require an amount of work of the same order as that required to communicate to the corpuscle a velocity of 10^8 cm. per sec.: this is equal to the work required to move the atomic charge against a potential difference of about 3 volts, and is therefore comparable with the work required to dissociate some of the most stable chemical compounds.

The fact, then, that some particles get negatively charged shows that in the neutral state these particles have an exceedingly strong affinity for a negatively electrified particle, while the absence of a particular particle from the negative side shows that its affinity is much less, but does not imply that it vanishes altogether. From what we have said, it should follow that the more slowly the neutral particles are moving relatively to the corpuscles, the more easily will the negatively electrified systems be formed. This is confirmed in a very striking way by our experiments, for when the discharge is passing very easily through the tube, and the velocity of the neutral particles is relatively small, the number of negatively electrified particles is very much increased; indeed, in some cases the brightness of the part of the photograph corresponding to the negative particles is as great as that corresponding to the positive, whereas when the discharge is passing with great difficulty, and the velocity of the neutral particles is very high, the negative part is very faint compared with the positive.

The particles which have been observed on the negative side are the hydrogen atom, the carbon atom, the oxygen atom, and the chlorine atom. The presence of the oxygen and chlorine atoms might, perhaps, have been expected, as these are universally regarded as strong electro-negative elements, i.e. as elements which have a strong affinity for negative electricity. The presence of the hydrogen atom is more remarkable, for hydrogen is generally considered to be a strongly electro-positive element, yet on these photographs we find it more persistently on the negative side than any other particle: often when no other line on the negative side is strong enough to be detected, the line corresponding to the hydrogen atom is distinctly visible. This is all the more remarkable, because the hydrogen atom, being the lightest of all the particles, is moving with the greatest velocity relatively to the corpuscles, and therefore would, other circumstances being the same, be the least likely to capture them. The heavier the particle, the slower is its velocity, and the greater chance it has of capturing the corpuscles; the fact that heavy complicated particles are conspicuous by their absence on the negative side shows that the attraction of these for the corpuscles must be exceedingly small compared with that exerted by a neutral atom of hydrogen. It will be seen that the atom of carbon, also regarded as an electro-positive element, is also conspicuous on the negative side.

On looking at the list of the particles which occur on the negative side, we are struck by the fact that they are all atoms: there is not a molecule among them. Thus, although the curve corresponding to the negatively electrified hydrogen atom occurs on every plate, there is not a single plate which shows a trace of a curve corresponding to a negatively electrified hydrogen molecule, although that corresponding to the positively electrified molecule is always present, and on some of the plates is stronger than that due to the positive hydrogen atom. Again, on some plates the positive oxygen molecule shows stronger than the oxygen atom, but on the negative side only the atom is visible.

Thus neutral atoms, but not neutral molecules, can exert on the negative corpuscles those enormous attractions which, under the conditions of these experiments, are required to bind the corpuscles to these rapidly moving particles. We may compare this result with the properties ascribed by chemists to bodies when in the nascent condition, i.e. when they have only recently been liberated from chemical combination, and when they are likely to be partly in the atomic state; for atoms, as we have seen, exert forces on electric charges in their neighbourhood vastly greater than those exerted by molecules.

We may compare the forces exerted by a neutral atom on the corpuscles with those exerted by an unelectrified piece of metal on a charged body in its neighbourhood. In consequence of electrostatic induction, the charge and the metal will attract each other. This attraction is dependent on the electricity in the metal being able to move under the electric forces exerted by the charge, and to re-arrange itself in such a way that if the charge is positive, the negative electricity in the metal moves to the part of the metal nearest to the charge, while the positive electricity moves to the part remote from the charge. The force between the metal and the charge depends on the freedom of the electricity to move about in the metal under the action of the electric field. If the metal is replaced by a substance of high specific inductive capacity, like sulphur, in which the electricity has an appreciable amount of freedom, though not so great as in a metal, the attraction, though still appreciable, is very much less than it was with the metal. A very simple experiment will illustrate this point. I have on this card-board disk, which is suspended from a long string, a number of magnets, such as are used for compasses; if I mount the magnets on pivots, so that they are free to turn round, the system of magnets is strongly attracted when another magnet is brought near it; if, however, I take the magnets off their pivots so that they are no longer free to turn, the magnet exerts very little attraction upon them.

A view of chemical combination which I gave some time ago in the 'Philosophical Magazine,' and also in my Corpuscular Theory of Matter, suggests that there is a very close analogy between the causes at work in the experiment we have just made and those which produce the difference between the behaviour of atoms and molecules. On that theory the atom was supposed to consist of a large number of corpuscles arranged inside a sphere of positive electricity; the corpuscles arranging themselves so as to be in equilibrium under their mutual repulsion and the attraction of the positive electricity. The configuration depends on the number of corpuscles, and the stiffness and stability of the system also change as the number changes. For some particular numbers of corpuscles the system is very rigid, and any movement of the corpuscles would be strongly resisted; since the movement of electricity inside the atom is brought about by the movement of the corpuscles, the electricity could only move with great difficulty inside these atoms, and they would therefore not be able to exert more than feeble forces on electrical charges outside the atom: they would therefore not enter readily into combination with other atoms. We may ascribe such a constitution as this to the atoms of the inert gases, helium, argon, and neon. A system with one, two, or three more corpuscles than the system we have just described would not be nearly so stable, and there would be a tendency to discard the extra corpuscles from the atom so that it might return to the more stable form. We may roughly picture to ourselves the atom with one extra corpuscle as consisting of a number of fixed corpuscles plus one which is free to move about; the freedom of this corpuscle would enable the electricity in the atom to move about, and would endow the atom with the property of attracting any electrical charges which might be near it. If there were two corpuscles in the atom more than the number required for the most stable form, we can picture the atom as having two corpuscles free and the rest fixed. Similarly, if we had more than two extra corpuscles. Thus we may regard the atom as possessing 0, 1, 2, 3 corpuscles which are able to move about with more or less facility, and the free corpuscles will give to the atom the power of exerting attractions on electrical charges to an extent which depends on both the number of corpuscles and the freedom with which they can move about. On the theory to which I have alluded the number of these "free" corpuscles determines the valency of the atom.

Now let us suppose that two such atoms come into such close connexion that the corpuscles in the one exert considerable forces on those in the other. The system consisting of the two atoms will rearrange itself so as to get into a more stable form, if necessary corpuscles passing from one atom to the other to enable it to do so. The greater stability, however, implies a loss of mobility; the free corpuscles have become parts of a more stable system, and have therefore lost to a greater or less extent their mobility. But with the mobility of the corpuscles goes their power of exerting forces on electrical charges; and thus the combination of the atoms diminishes to a great extent the attractions they exert outside them. Speaking generally, we may say that on this view the combination of atoms to form molecules, either of compounds or elements, fixes corpuscles which were previously mobile and converts the atoms from conductors of electricity into insulators with a small specific induction capacity.

I have brought these illustrations before you with the object of showing that we have now methods which are capable of dealing with much smaller quantities of matter than the methods now used by chemists, methods which are capable of detecting transient phases in the processes of chemical combination, and which I am hopeful may be of service in throwing light on one of the most interesting and mysterious problems in either physics or chemistry—the nature of chemical combination.

[J. J. T.]

WEEKLY EVENING MEETING,

Friday, April 28, 1911.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

PROFESSOR W. M. FLINDERS PETRIE, D.C.L. Litt.D. LL.D. F.R.S. F.B.A.

The Revolutions of Civilization.

[ABSTRACT.]

THE meaning of life has in all ages been the goal of human thought; and the great extension of our knowledge of the past, by the researches of recent years, enables us now to view the course of human changes with more completeness than has been the case before. In place of looking on the fall of the Roman Empire as a monstrous and inexplicable fact, we now see that civilisation is not only intermittent, but is a regularly recurrent phenomenon.

Of all the activities of man, that which can be compared over the greatest range is carving and sculpture. It is also one of the most definite, and can be estimated at a definite stage—that of the close of archaism and acquirement of freedom. Hence this stage is the best defined point for study.

In Egypt we can trace eight civilisations: the first pre-historic, the second pre-historic, the early dynastic, the pyramid builders, the Middle Kingdom (XIIth dynasty), the Empire (XVIIIth dynasty), the Greek, and the Arab. The rise and fall of art in each of these periods can be well illustrated.

Similar periods are found in Europe, the Early, Middle, and Late Cretan, the Classical and the Mediæval. These periods are contemporary with those in Egypt, so far as we can ascertain. Hence the Mediterranean civilisation seems a distinct unit, rising and falling together as one body. The Mesopotamian civilization is in a different phase to that of the Mediterranean, but its period is about the same. The sculpture stage is in:—

EGYPT.	MESOPOTAMIA.
3450 B.C.	3750 B.C.
1550	2100
450	640
1240 A.D.	820 A.D.

Hence Mesopotamia always has the lead of Europe in a rising civilization, but is subordinate to Europe in the later stages.

A similar period of about 1500 years has occurred in India and Mexico. The Etruscan Sages assigned 1100 years as the period of a race, after which it must be succeeded by another.

The various activities of man are related in the same order in each successive period. This can be best traced in the Mediæval and Classical periods, where the stages of sculpture, painting, literature, music, mechanics, science, and wealth spread over some seven centuries. These stages were nearer together in earlier ages, but the order was always the same so far as can be traced.

The starting point of each civilization—as of each generation—is a mixture of blood. Without a fusion of race no fresh start can be made. About six or eight centuries is needed for the rise to the sculpture stage. The growth of civilization largely consists in a lengthening out of the subsequent stages of activity, and diminution of the stagnant period before a new mixture is started. The period seems to belong to the people and not to the country, and is kept by the people when they go to a land of a different phase.

Though, no doubt, climatic periods have a precipitating effect in throwing one people on to another, yet the general regularity of interval of the stages of growth of civilization point to a racial determinant. This may be the time required to promote the maximum mixture of different strains in two races which are in contact.

We have now co-ordinated the facts of civilizations, and see how general is the type of development and the period. We can take an intelligent view of what we know of mankind as a systematic whole, following a regular series of seasonal changes age after age. The details, and the illustrations of the rise and fall of each stage, are published in a small volume (Harpers) under the title of this lecture, being too voluminous for these Proceedings.

ANNUAL MEETING,

Monday, May 1, 1911.

THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1910, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Thirty-nine new Members were elected in 1910.

Sixty-one Lectures and Seventeen Evening Discourses were delivered in 1910.

The Books and Pamphlets presented in 1910 amounted to about 239 volumes, making, with 604 volumes (including Periodicals bound) purchased by the Managers, a total of 843 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.

TREASURER—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.

SECRETARY—Sir William Crookes, O.M. LL.D. D.Sc. F.R.S.

Henry E. Armstrong, Esq., Ph.D. LL.D. F.R.S.
Sir John Wolfe Barry, K.C.B. LL.D. F.R.S.
John B. Broun-Morison, Esq., D.L. J.P.
John Mitchell Bruce, Esq., M.A. M.D. LL.D. F.R.C.P.
The Right Hon. Sir Henry Burton Buckley, P.C. M.A.—*V.P.*
The Right Hon. Earl Cathcart, D.L. J.P.—*V.P.*
Donald William Charles Hood, Esq., C.V.O. M.D. F.R.C.P.—*V.P.*
Alexander C. Ionides, Esq.
Sir Francis Laking, Bart. G.C.V.O. M.D. LL.D.—*V.P.*
Henry F. Makins, Esq., F.R.G.S.—*V.P.*
Rudolph Messel, Esq., Ph.D. F.C.S.
Robert Mond, Esq., M.A. F.R.S.E. F.C.S.
The Hon. Sir Charles A. Parsons, K.C.B. M.A. Sc.D. LL.D. F.R.S.
Alexander Siemens, Esq., M.Inst.C.E.—*V.P.*
The Right Hon. Sir James Stirling, P.C. LL.D. F.R.S.

Henry Edmunds, Esq., M.Inst.E.E.
Sir Frederick Fison, Bart., M.A. F.C.S.
John William Gordon, Esq.
Major-General Sir Coleridge Grove, K.C.B.
Charles Edward Groves, Esq., F.R.S.
William A. T. Hallowes, Esq., M.A.
Arthur Croft Hill, Esq., M.D. M.R.C.S.
H. Robert Kempe, Esq.
A. Kirkman Lloyd, Esq., D.L. K.C.
Major Percy A. MacMahon, R.A. Sc.D. F.R.S.
Frank K. McClean, Esq., F.R.A.S.
Emile R. Merton, Esq.
Sir Charles Day Rose, Bart., M.P.
William Stone, Esq., M.A. F.L.S. F.C.S.
Harold Swithinbank, Esq., J.P. F.R.S.E.

WEEKLY EVENING MEETING,

Friday, May 5, 1911.

SIR WILLIAM CROOKES, O.M. LL.D. D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

PROFESSOR MARTIN O. FORSTER, D.SC. PH.D. F.R.S. *M.R.I.*

New Organic Compounds of Nitrogen.

[WITH EXPERIMENTAL ILLUSTRATIONS.]

IT may be stated without fear of contradiction that the most versatile form of elemental matter is nitrogen. Rivalled only by argon and its associates in reluctance to take part in chemical action, its entrance into combination with other elements leads to interesting forms of activity in great profusion. Union with hydrogen in different proportions, for example, produces ammonia, hydrazine and hydrazoic acid, three highly reactive substances having characteristics which stand in marked contrast with one another. If oxygen be brought into the system, hydroxylamine, nitrous acid and nitric acid may be mentioned as typical materials capable of entering into chemical changes of the most diverse order.

Organic derivatives of nitrogen, however, present an even greater display of individuality. Prussic acid, the alkaloids, nitro-glycerine, gun-cotton, celluloid, artificial musk, lyddite, indigo, the azo-dyes, hæmoglobin and the enzymes are a few of the conspicuous nitrogen compounds which suggest themselves in this connection, and a survey of their activities would justify a reference to nitrogen as

> "An element so various it seems to be
> Not one, but all Hermetic Art's epitome."

It is not the occasion, however, to discuss the foregoing materials, my present purpose being rather to deal with some new organic derivatives of nitrogen which, although not associated with any important industrial development, nevertheless display properties of considerable interest to chemists.

The extraordinary inertness of elemental nitrogen has been already mentioned, and the underlying cause of this feature is the tenacity with which two atoms of the substance remain in combination with each other. But in circumstances which will be explained later, three atoms of nitrogen may be brought into and maintained in combination, the resulting complex being known as the triazo-group. It is a remarkable fact that although, as has just been seen, nitrogen is distributed among naturally occurring substances almost as widely as carbon itself, there is no recorded example of a triazo-compound being obtained from natural sources. Clearly then, the union of atoms in the triazo-group is not among those marriages which are made in Heaven.

That being the case, it becomes necessary to consider the materials and processes which underly the synthetical production of triazo-compounds. Proceeding from ammonia, and replacing one of the hydrogen atoms by an amino-group, diamide, or hydrazine, is produced, but although discovered by Curtius in 1887, the method by which he prepared it is of historical interest only, it having been left to the ingenuity of Raschig, as recently as 1907, to accomplish the production of this substance by a simple and inexpensive process. This consists in first replacing an atom of hydrogen in ammonia by an atom of chlorine, and then, by direct action of more ammonia on the resulting chloramine, replacing the halogen by an amino-group:

$$NH_3 + NaOCl = NH_2Cl + NaOH.$$
$$NH_3 + NH_2Cl = NH_2{\cdot}NH_2, HCl.$$

The new process illustrates in remarkable fashion the effect which physical condition may exert on chemical change. Although chloramine and ammonia may act together as already shown in a constructive direction, an alternative, destructive course is open to them, which is accelerated if the viscosity of the liquid is diminished:

$$2NH_3 + 3NH_2Cl = N_2 + 3NH_4Cl.$$

Thus Raschig was able to increase the yield of hydrazine by increasing the viscosity of the solution with addition of glue, whilst the undesirable effect, namely, liberation of nitrogen, was shown to follow the addition of acetone.

Just as ammonia may be doubled on itself to produce diamide or hydrazine, the latter might be expected to allow one of its hydrogen atoms to be replaced by another amino-group and furnish triamide, or triazane. This, however, has not been accomplished, and there is evidence to suggest that such a substance would be most unstable. Whenever attempts are made to add another nitrogen link to the chain of two atoms in hydrazine, the terminal atoms of the three-link chain are found to have combined with one another, forming an enclosed association or ring of atoms. This is the triazo-group, and its simplest known form is the compound with hydrogen called azoimide, or hydrazoic acid, HN_3. The latter name emphasises the first point of interest in connection with the triazo-group. Whereas the simpler compounds of nitrogen with hydrogen, namely, ammonia and hydrazine, are both strong bases, forming very stable salts with acids, azoimide is a well-defined acid, forming salts with bases including ammonia and hydrazine themselves, the products then having composition expressed by the curious formulæ, N_4H_4 and N_5H_5. Another salt, that with ferric iron, provides a very delicate test for hydrazoic acid, the red colour being noticeable in solutions containing only one part per million.

Hydrazoic acid, discovered in 1890, also by Curtius, may be shown by experiment to follow an attempt to add another atom of nitrogen to hydrazine. Whilst the action of nitrous acid on ammonia causes both atoms of nitrogen to be liberated in elemental form,

$$HNO_2 + NH_3 = N_2 + 2H_2O,$$

hydrazine is converted by the same agent into hydrazoic acid,

$$HNO_2 + NH_2{\cdot}NH_2 = HN_3 + 2H_2O.$$

This process, however, is not adapted to the production of hydrazoic acid or its salts in large quantities. Two methods are available for this purpose. One of these, due to W. Wislicenus (1892), consists in passing a current of dry nitrous oxide over shallow layers of powdered sodamide at about 200°, and by means of certain modifications good yields of sodium azide may be obtained:

$$NaNH_2 + N_2O = NaN_3 + H_2O.$$

A later method, elaborated by Stollé and Thiele working independently (1908), is based on the experiment just mentioned, namely, the action of nitrous acid on hydrazine, but instead of using free nitrous acid, an ethereal nitrite, such as ethyl or amyl nitrite, is employed. These materials, in presence of alkali, transform hydrazine into sodium azide, the yield being excellent; and this discovery has resulted in the commercial production of that salt, which may now be purchased at less than 40*s*. per pound,

$$NH_2{\cdot}NH_2 + Alk{\cdot}ONO + NaOEt = NaN_3 + Alk{\cdot}OH + EtOH + H_2O.$$

The free acid is a dangerous and disagreeable substance to manipulate. When free from water, it condenses to a colourless mobile liquid, which boils at about the same temperature as ether (37°); but although harmless looking, it is a frightful explosive, and requires but slightly elevated temperatures for the display of this property. Moreover, the vapour is extremely poisonous, and when inhaled in even trifling quantities causes distressing headache; it is, in fact, a powerful protoplasmic poison, its activity in this direction being comparable with that of prussic acid. As might be expected, therefore, its effect on the blood-spectrum is immediate and pronounced.

To chemists, however, the most conspicuous property of hydrazoic acid is the resemblance it bears to the halogen acids, a solution containing only one part per million giving a faint turbidity with silver nitrate. The resulting silver azoimide, in common with other hydrazoic salts of heavy metals, is dangerously explosive, but the salts of the alkali metals may be handled without risk. The latest recruit to the metallic azoimides is radium azide, described by Ebler about six months ago, and utilized in attempts to prepare metallic radium, which appears to follow the decomposition of radium azide by heat, just as barium azide may be shown to yield the metal when deprived of nitrogen. Pursuing the analogy between the triazo-group and the halogen atoms, a study of the refrangibility and dispersion of light by organic triazo-compounds deserves attention. The following measurements have been carried out by Philip, and show that while the mean increment of refraction for an atom of bromine is 8·93, that of the triazo-group is 8·91, and whilst the atomic dispersion of bromine is 0·35, the increment traceable to the azoimide nucleus is 0·36.

	Contribution of N_3-Group to	
	Refraction.	Dispersion.
Ethyl Triazoacetate	8·74	0·35
Ethyl α-Triazopropionate	8·87	0·36
Ethyl β-Triazopropionate	8·86	0·37
Ethyl Bistriazoacetate	9·07	0·38
Triazoethyl Alcohol	8·97	0·35
Bistriazoethane	8·98	0·36
Benzylazoimide	8·85	0·35
Contribution of N_3 (mean) . .	8·91	0·36
Contribution of Br	8·93	0·35

Moreover, on comparing the temperatures at which a number of typical triazo-compounds boil with the constants for halogen derivatives having similar structure, it appears that the triazo-group exerts an elevating effect considerably greater than that of chlorine, somewhat greater than that of bromine, and approximating to that of iodine. Furthermore, it has been shown by Philip that the introduction of a triazo-group into a molecule of acetic acid has an effect on the strength of the acid which is rather less than that due to a bromine atom and rather greater than that caused by an iodine atom.

	Dissociation Constant.
Acetic Acid	0·000018
Iodoacetic Acid	0·00075
Triazoacetic Acid	0·00093
Bromoacetic Acid	0·00138
Chloroacetic Acid	0·00155
α-Triazopropionic Acid	0·00086
α-Bromopropionic Acid.	0·00108

Thus the physical evidence supports the chemical indications, and serves to classify the triazo-group as a complex radicle with a strong family resemblance to the halogens.

Bearing this resemblance in mind, it was natural to anticipate the possibility of constructing a composite molecule in which the azoimide nucleus is united with one atom of a halogen, for example chlorine, just as molecular chlorine is composed of two similar atoms. Such a synthesis has been lately accomplished by Raschig (1909), by the interaction of hypochlorous and hydrazoic acids,

$$HClO + HN_3 = ClN_3 + H_2O,$$

but the union between the two components is very easily broken, and chloroazoimide is highly explosive.

Proceeding now to study the behaviour of the triazo-group when placed in an organic environment, it is necessary first to examine the operations by which typical organic azoimides may be prepared. Although too numerous to be even summarised on the present occasion, some points in connection with the changes involved require attention. We have seen that the action of nitrous acid on ammonia sets free completely the nitrogen present in both materials. If, instead of ammonia there is taken its benzene derivative, aniline, in the form of the hydrochloride, nitrogen becomes added to that of the aniline, and although it is true that the product, when heated, gives up all its nitrogen just as ammonium nitrite does, the intermediate compound is perfectly stable at the temperature of melting ice:—

(cold) $C_6H_5 \cdot NH_2, HCl + HNO_2 = C_6H_5 \cdot N_2 \cdot Cl + 2H_2O$

(hot) $C_6H_5 \cdot NH_2, HCl + HNO_2 = C_6H_5 \cdot OH + N_2 + HCl + H_2O.$

This intermediate compound and related substances are among the most important, from both practical and theoretical standpoints, known to chemists. They are called diazonium salts, and their discovery, with that of many remarkable changes undergone by them, is due to Peter Griess (1858). It is not the least illuminating feature of Griess's work that the study of the diazo-compounds, underlying as it does the immensely valuable coal-tar colour industry, was pursued by its author while employed as chemist in a well-known firm of brewers, to whose particular undertaking the work in question had no application; such far-sighted encouragement of apparently useless research for its own sake is an object lesson which is not, unhappily, without value even in these days.

Fascinating as are the problems, solved and unsolved, within the boundaries of diazo-chemistry, it would be inappropriate to encroach on them this evening, but the diazo-compounds forming a natural stepping-stone to the triazo-derivatives, it is relevant to draw passing attention to one recent application of their activity. The great class of artificial colouring matters known as aniline dyes depend largely, though by no means exclusively, upon the process known to chemists as "coupling." If an aromatic base which has been treated with nitrous acid is added to one which has not been so diazotized, combination usually takes place in such a manner that a derivative of azobenzene is produced, and in the factory or laboratory coupling is effected with aqueous solutions maintained at zero to avoid decomposition of the diazo-group. A class of substances recently described by Morgan and Micklethwait, however, make it possible to administer the diazo-complex in a solid form, thereby greatly facilitating a demonstration of the coupling process; on mixing a few decigrams of toluene-*p*-sulphonyl-*p*-phenylenediazoimide with the same quantity of α-naphthylamine, adding a few c.c. of pyridine, and diluting the solution with absolute alcohol, concentrated hydrochloric acid develops an intense permanganate coloration.

We now arrive at the stage immediately preceding the organic triazo-compounds. Just as hydrazine itself, when treated with nitrous acid gives hydrazoic acid or azoimide, so phenylhydrazine with nitrous acid gives phenylazoimide,

$$C_6H_5{\cdot}NH{\cdot}NH_2 + HNO_2 = C_6H_5{\cdot}N_3 + 2H_2O.$$

Its production in this way gives the first indication of its properties, for unlike aniline and phenylhydrazine, which are bases, phenylazoimide is a neutral substance, resembling its haloid analogues, chlorobenzene, bromobenzene, and iodobenzene.

Although phenylazoimide was first brought to light by Griess in 1866, the subject of triazo-chemistry lay dormant until the discovery of hydrazoic acid by Curtius in 1890. Since that time the province has been a flourishing one, and the number of triazo-compounds which have been prepared and studied by Curtius and his pupils constitute an imposing section of nitrogen chemistry. It is principally with the acid azides, substances in which the triazo-group plays the same part as chlorine in benzoyl chloride, that the investigations of Curtius have been concerned, and it is noteworthy that some of the earliest syntheses of polypeptides were effected by him simultaneously with Emil Fischer through the agency of these compounds; in the subsequent development of this field, however, the azide process gave way to the linking up by chlorides, as adopted and elaborated by Fischer. The method by which Curtius prepared his acid azides is really an adaptation of the change by which hydrazoic acid itself is obtainable from hydrazine, namely, by the action of nitrous acid upon the hydrazides of the respective acids, and it may be stated in general terms that any derivative of azoimide may be produced in this way provided the corresponding derivative of hydrazine is available; but this is the case only in certain classes of compounds, and the preparation of aromatic azoimides is best accomplished by a process due to Noelting, which consists in adding sodium azide to the diazonium sulphate:

$$X\cdot N_2\cdot HSO_4 + NaN_3 = X\cdot N_3 + N_2 + NaHSO_4.$$

The remarkable instability of the diazonium azide, upon which this reaction depends, recalls that of the diazonium iodide, following which the iodine atom may be introduced into a benzenoid compound, and incidentally offers one more analogy between the triazo-group and a halogen. Moreover, resemblance to the haloid elements is suggested not only by this method of introducing an azoimide complex into the aromatic nucleus, but also by the circumstances attending its removal. It is well known that a chlorine atom in the benzene ring is so firmly attached that it cannot be removed by hot caustic alkalis, but that if nitro-groups also are present in the ortho- and para-positions, the halogen may be removed quite easily, so that picryl chloride is converted into picric acid by the action of water. Very similar relationships prevail among the substituted azoimides of benzene and naphthalene, from which hydrazoic acid may be eliminated according to well-defined principles.

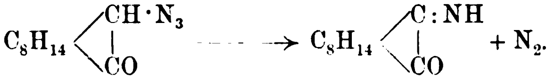
It is now six years since we were led, at the Royal College of Science, to a study of the triazo-group by the accidental discovery of triazocamphor, a substance which displays properties in some respects novel, and in this connexion it gives me the greatest pleasure to acknowledge that the development of the subject has been in a large measure due to the courage and ingenuity displayed in its early stages by my former colleague, Dr. H. E. Fierz. The majority of the new materials differ from those of previous workers in belonging, not to the aromatic series, but to the other great class of organic substances, the aliphatic derivatives. It is with a study of the general behaviour of the triazo-group in the diverse environment afforded by these two classes that we have been concerned, and with this object have prepared azoimides of such simple types as acetaldehyde, acetone, acetic acid, ethyl acetate, acetamide, ethyl alcohol, ethyl ether, substituted malonic acids, the unsaturated hydrocarbons ethylene and propylene, and, lastly, ethylamine. To these have been added certain bistriazo-compounds, namely, those of ethane, acetic ester, malonic ester and acetoacetic ester. In this latter class the explosive character of the triazo-group has been very prominent; for instance, whilst a drop of ethyl triazoacetate when thrown on a hot plate merely inflames without detonation, 1:2-bistriazoethane explodes with considerable violence; and this property is particularly noticeable when both triazo-groups are attached to the same atom of carbon, 1:1-bistriazoethane continuing in existence only a few minutes after isolation, when it disappeared with a deafening explosion. Another characteristic feature of these materials becomes evident when their vapour is inhaled, the experience of pleasure in an ethereal odour being quickly followed by a throbbing sensation at the base of the forehead often accompanied by palpitation of the heart. Whilst, however, the derivatives of benzene and naphthalene usually acquire an odour of anise by the introduction of the triazo-complex, the perfume connected with such substances as the azoimides of ethyl acetate, ethyl alcohol, acetone, ether and acetic acid, is quite devoid of the anise character, and is indeed much fainter than that of the parent compound in each case.

The method by which these aliphatic azoimides have been prepared is extremely simple. It depends on interaction of the corresponding halogen derivative with sodium azide; for example,

$$CH_3\cdot CO\cdot CH_2Cl + NaN_3 = CH_3\cdot CO\cdot CH_2\cdot N_3 + NaCl,$$

and appears to be almost general. The reaction has been lately applied to the nitrosochlorides of pinene, dipentene, the limonenes and terpineols, and it has been further found that the azoimide complex may be exchanged for the nitroxy-group in nitrosates, such as those of amylene and dipentene.

The various degrees of stability displayed by the azoimide nucleus is the feature which chiefly concerns chemists. Three principal types of decomposition may be recognized. The environment least conducive to stability betrays itself in the liberation of two nitrogen atoms in the elemental form, leaving the third atom attached to the carbon which originally carried all three. This is exhibited by triazoantipyrine, which loses nitrogen spontaneously at common temperatures, and passes into a bright red azo-compound. Triazoacetone undergoes this change more slowly, but may be stimulated into activity by the catalytic effect of alkali, a property which it shares with triazocamphor,



$$C_8H_{14}\left\langle\begin{matrix}CH\cdot N_3\\ |\\ CO\end{matrix}\right. \longrightarrow C_8H_{14}\left\langle\begin{matrix}C:NH\\ |\\ CO\end{matrix}\right. + N_2.$$

Sometimes excess of alkali must be used and the temperature raised, as in the case of triazoacetic acid, salts of which, when thrown into hot caustic potash, immediately begin to liberate two-thirds of their nitrogen as gas. The acid azides offer further illustrations of this decomposition, and here the phenomenon is accompanied by a remarkable atomic transformation, first brought to light by Curtius, later studied by Schroeter and others, the denuded nitrogen atom actually changing places with the carbon which formerly carried the triazo-group, with the result that *iso*cyanates are produced:

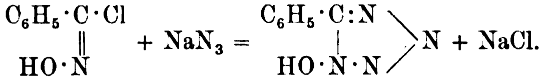
$$X\cdot CO\cdot N_3 \longrightarrow X\cdot N:C:O + N_2.$$

Sometimes this alteration is explosive, as in the case of triazoacetic azide, $N_3\cdot CH_2\cdot CO\cdot N_3$.

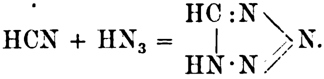
The next class of transformation undergone by the triazo-complex is more moderate. It was discovered by Dimroth, and may be described as an unfolding of the three-atom nitrogen ring without loss of the element, all three atoms of which remain in the form of a chain such as occurs in diazoaminobenzene; triazen derivatives of this type are, in fact, easily obtainable by the action of alkyl magnesium halides upon organic azoimides,

$$C_6H_5\cdot MgBr + C_6H_5\cdot N_3 = C_6H_5\cdot N{:}N\cdot N(MgBr)\cdot C_6H_5.$$

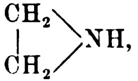
Although some external agency is usually necessary for this type of transformation we have found cases in which it takes place spontaneously. For example, allylazoimide, a colourless, mobile liquid when freshly prepared, rapidly changes into a white crystalline solid, consisting of a diazoamino-compound isomeric with the original material, whilst an attempt to prepare benzhydroximic azide led forthwith to its isomeric transformation product, hydroxyphenyltetrazole,



More recently, the Italian chemists, Palazzo and Oliveri-Mandalà, discovered that fulminic acid and methylcarbylamine, both highly unsaturated compounds, also are capable of producing this effect on the azoimide nucleus, converting hydrazoic acid into 1-hydroxytetrazole and 1-methyltetrazole respectively, whilst tetrazole itself is obtainable by the interaction of hydrazoic and prussic acids,



The third class of alteration undergone by the triazo-group involves its complete elimination from the molecule in combination with hydrogen, its place being taken by the hydroxy-group. The simplest illustration of this case followed attempts to prepare triazomethylamine, $N_3\cdot CH_2\cdot NH_2$, which we found could not be realised because derivatives of that substance display the remarkable property of liberating hydrazoic acid when treated with cold water. The same thing happens when triazotised carbon is associated with a halogen, triazoethylene dibromide, for example, yielding hydrazoic and hydrobromic acids after a few minutes contact with ice-cold water; for this reason it was not possible to realise triazoacetylene, the copper derivative of which might be expected to excel that of acetylene itself in explosibility. More generally, however, this type of decomposition requires the action of alcoholic potash for its completion.

The behaviour of the triazo-group in respect of these three classes of transformation is controlled by its molecular environment, but all organic azoimides have this in common, that they undergo change of the first type—namely, loss of two-thirds the azidic nitrogen, when treated with 80 per cent. sulphuric acid, or with various reducing agents. Moreover, they are all extremely sensitive to the action of light. As from time to time the marked analogy between the triazo-complex and the halogens has been emphasized, it should be mentioned that one complete contrast has lately been found. The halogen derivatives of ethylamine are known to part so readily with halogen hydride, that chloroethylamine and bromoethylamine cannot be liberated from their salts without immediately passing into dimethyleneimine,  $(CH_2)_2NH$, isomeric with the unknown vinylamine or aminoethylene. Salts of β-triazoethylamine, on the other hand, behave like salts of ethylamine itself, and so stable is the resulting triazo-base that it may be distilled from solid potash under reduced pressure without loss of hydrazoic acid.

Many disappointments have occurred in studying the triazo-group, and the most conspicuous one which I have to admit is the failure to produce hexatomic nitrogen; in view of the resemblance between the complex in question and the halogens, and the association of two atoms of the latter in their free molecules, it was to be expected that a new modification of nitrogen might arise by linking together two triazo-radicles. Experiments with this object have failed, and if differently arranged efforts in the same direction are also barren of result, the reluctance of the two nuclei to become united may be regarded as having some bearing on the question of the constitution and transformations of the triazo-group. To aid in the elucidation of this is, indeed, the principal purpose of the foregoing observations, in view of our present ignorance regarding the disposition in space of the atoms or groups which are associated with nitrogen in its organic derivatives. Such disposition or configuration is fairly well understood with respect to carbon, the original views of Kekulé, as developed by van 't Hoff, having stood the test of forty years' unceasing experimentation, but the efforts of chemists to fathom the nature of combined nitrogen, and the arrangements in space of its varying valency directions, have not been so successful. Nitrogen remains a Sphinx among the elements, propounding riddles to the inquiring chemist, although happily, unlike her prototype, she does not annihilate those who fail to solve them.

[M. O. F.]

GENERAL MONTHLY MEETING,

Monday, May 8, 1911.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Carrol W. Ansdell, Esq.
Lady Bertha D. Bell,
Sebastian Ziani de Ferranti, Esq., Pres.Inst.C.E.
Harry Kahn, Esq.
George Manuel, Esq.
C. E. Moulton, Esq.
Arthur Perks, Esq.

were elected Members of the Royal Institution.

The Chairman announced that His Grace The President had nominated the following Gentlemen as Vice-Presidents for the ensuing year :—

The Right Hon. Sir Henry Burton Buckley, P.C. M.A.
The Right Hon. Earl Cathcart, D.L. J.P.
Donald William Charles Hood, Esq., C.V.O. M.D. F.R.C.P.
Sir Francis Laking, Bart., G.C.V.O. K.C.B. M.D. LL.D. M.R.C.P.
Henry Francis Makins, Esq., F.R.G.S.
Alexander Siemens, Esq., M.Inst.C.E.
Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S. (Treasurer).
Sir William Crookes, O.M. LL.D. D.Sc. F.R.S. (Honorary Secretary).

The Special Thanks of the Members were returned to Sir John Wolfe Barry, K.C.B. LL.D. F.R.S., for his Present of a Portrait of Professor Faraday.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Secretary of State for India—Geological Survey of India: Records, Vol. XL. Part 4. 8vo. 1910.
Memoirs, Vol. XXXIX. Part 1. 8vo. 1911
Palæontologia Indica, Series XV. Vol. IV. Fasc. 3. 4to. 1910.
British Museum Trustees—Catalogue of Greek Coins: Phœnicia. 8vo. 1910.
Catalogue of Romances, Vol. III. 8vo. 1910.
Supplementary Catalogue of Bengali Books, 1886–1910. 4to. 1910.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XX. 1º Semestre, Fasc. 5. 8vo. 1911.

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Association of Accountants—Journal, Vol. IV. No. 13, April, 1911. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LXXI. No. 5. 8vo. 1911.

Bankers, Institute of—Journal, Vol. XXXII. No. 5. 8vo. 1911.

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British Architects, Royal Institute of—Journal, Third Series, Vol. XVIII. Nos. 12–13. 4to. 1911.

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Buenos Aires—Bulletin of Municipal Statistics for Jan. 1911. 4to.

Canada, Geological Survey—Department of Mines: Bulletin, Nos. 4–5. 8vo. 1910.

Carnegie Institution, Mount Wilson Solar Observatory—Contributions, Nos. 49–50. 8vo. 1911.

Annual Report, 1910. 8vo. 1911.

Chemical Industry, Society of—Journal, Vol. XXX. Nos. 7–8. 8vo. 1911.

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Journal for April, 1911. 8vo.

Civil Engineers, Institution of—Proceedings, Vol. CLXXXIII. 8vo. 1911.

Colonial Institute, Royal—United Empire for April, 1911. 8vo.

East India Association—Journal, N.S. Vol. II. No. 2. 8vo. 1911.

Editors—Agricultural Economist for April, 1911. 4to.

American Journal of Science for April-May, 1911. 8vo.

Athenæum for April, 1911. 4to.

Author for April-May, 1911. 8vo.

Chemical News for April, 1911. 4to.

Chemist and Druggist for April, 1911. 8vo.

Concrete for April, 1911. 8vo.

Dyer and Calico Printer for April, 1911. 4to.

Electrical Engineer for April, 1911. 4to.

Electrical Engineering for April, 1911. 4to.

Electrical Review for April, 1911. 4to.

Electrical Times for April, 1911. 4to.

Electricity for April, 1911. 8vo.

Engineer for April, 1911. fol.

Engineer-in-Charge for April, 1911. 8vo.

Engineering for April, 1911. fol.

Horological Journal for April, 1911. 8vo.

Illuminating Engineer for April-May, 1911. 8vo.

Journal of Physical Chemistry for March, 1911. 8vo.

Journal of the British Dental Association for March-April, 1911. 8vo.

Law Journal for April, 1911. 8vo.

London University Gazette for April, 1911. 4to.

Model Engineer for April, 1911. 8vo.

Mois Scientifique for March-April, 1911. 8vo.

Motor Car Journal for April, 1911. 4to.

Musical Times for April, 1911. 8vo.

Nature for April, 1911. 4to.

New Church Magazine for May, 1911. 8vo.

Nuovo Cimento for Jan. 1911. 8vo.

Page's Weekly for April, 1911. 8vo.

Physical Review for April, 1911. 8vo.

Science Abstracts for March-April, 1911. 8vo.

Editors—continued.
Science of Man for Jan. 1911. 8vo.
Surveying for April, 1911. 4to.
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Florence, Biblioteca Nazionale—Bulletin for April, 1911. 8vo.
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Geneva, Société de Physique—Comptes Rendus, No. XXVII. 8vo. 1910.
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Inwards, Richard, Esq., F.R.A.S. (the Author)—William Ford Stanley, his Life and Work. 8vo. 1911.
Johns Hopkins University—American Journal of Philology, Vol. XXXII. No. 1. 8vo. 1911.
Lloyd Library, Cincinnati—Bibliographical Contributions, Nos. 1-2. 8vo. 1911.
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Meteorological Society, Royal—Journal, Vol. XXXVII. No. 158. 8vo. 1911.
Record, Vol. XXX. No. 119. 8vo. 1911.
Microscopical Society, Royal—Journal, 1911, Part 2. 8vo.
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National Church League—Church Gazette for April-May, 1911. 8vo.
Navy League—The Navy for May, 1911. 8vo.
New York Academy of Sciences—Annals, Vol. XX. Parts 1-2. 8vo. 1910.
New York, Society for Experimental Biology—Proceedings, Vol. VIII. No. 3. 8vo. 1910.
New Zealand, High Commissioner—New Zealand Official Year Book, 1910. 8vo.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for March, 1911. 4to.
Paris, Société Française de Physique—Journal de Physique for April, 1911. 8vo.
Parsons, Messrs. C. A. and Co.—The Evolution of the Parsons Steam Turbine. By A. Richardson. 4to. 1911.
Pharmaceutical Society of Great Britain—Journal for April, 1911. 8vo.
Philadelphia Academy of Natural Sciences—Proceedings, Vol. LXII. Part 3. 8vo. 1910.
Photographic Society, Royal—Journal, Vol. LI. No. 4. 8vo. 1911.
Illustrated Catalogue of Fifty-sixth Annual Exhibition. 8vo. 1911.
Physical Society of London—Proceedings, Vol. XXIII. Part 3. 8vo. 1911.
Post Office Electrical Engineers, Institution of—Journal, Vol. IV. Part 1. 8vo. 1911.
Quekett Microscopical Club—Journal, Ser. 2, Vol. XI. No. 68, April, 1911. 8vo.
Rome, Ministry of Public Works—Giornale del Genio Civile for Feb. 1911. 8vo.
Röntgen Society—Journal, Vol. VII. No. 27, April, 1911. 8vo.
Royal Engineers' Institute—Journal, Vol. XIII. No. 5. 8vo. 1911.
Royal Irish Academy—Proceedings, Vol. XXIX. A, No. 1; B, No. 4; C, No. 3. 8vo. 1911.
Royal Society of Arts—Journal for April, 1911. 8vo.
Royal Society of Edinburgh—Transactions, Vol. XLVII. Part 3. 4to. 1911.
Royal Society of London—Proceedings, A, Vol. LXXXV. No. 576. 8vo. 1911.
Philosophical Transactions, A, Vol. CCXI. Nos. 473-474; B, Vol. CCII. Nos. 282-283. 4to. 1911.
St. Petersburg, Imperial Academy of Sciences—Bulletin, 1911, Nos. 6-7. 8vo.
Sanitary Institute, Royal—Journal, Vol. XXXII. No. 4. 8vo. 1911.
Selborne Society—Selborne Magazine for May, 1911. 8vo.
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Statistical Society, Royal--Journal, Vol. LXXIV. No. 5. 8vo. 1911.

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Vienna Imperial Geological Institute--Verhandlungen, 1910, Heft 17-18; 1911, Heft 1-2. 8vo.

Warsaw, Society of Sciences—Comptes Rendus, Vol. IV. Nos. 1-2. 8vo. 1911.

Western Society of Engineers—Journal, Vol. XVI. Nos. 2-3. 8vo. 1911.

Zoological Society of London—Report for the Year 1910. 8vo. 1911.

WEEKLY EVENING MEETING,

Friday, May 12, 1911.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

PROFESSOR WILLIAM STIRLING, M.D. LL.D. D.Sc.

Biology and the Cinematograph.

[WITH ILLUSTRATIONS.]

WE are but children of a larger growth—direct descendants of "Helen's Babies" and their predecessors. Some of you may remember that "Helen's Babies" more than three decades ago wished to see the wheels go round—they were not content with a glance at the dial of the three hundred dollar timekeeper of their long-suffering uncle. Humanity of older growth is still trying to look beyond the dial. It also wishes to see the wheels go round.

Movement is one of the most characteristic signs of active life—although it is not peculiar to or distinctive of animal life—but as animals possess in the highest degree the power of generating mechanical motion, naturally their motor organs have been far more carefully investigated than the movements themselves, and the reason is not far to seek. Progress in the intimate study of animal movements is being made, as it becomes daily more apparent how infinitely fertile are the movements of organisms as a field for the study of adapted actions. I include amongst these actions all those movements which have been investigated with such conspicuous success by Jennings, of Philadelphia, under the title "Behaviour of Animals."

Borelli, "De Motu Animalium."

On the last day of the year 1678, and as the new year was coming in, there died in the Convent of San Pantaleone, in Rome, Giovanni Alfonso Borelli, the founder of the study of animal movement and the teacher of Marcellus Malpighi. Borelli, in his classical work, "De Motu Animalium," published after his death, deals not only with the external visible movements of animals, but also with movements of internal organs, such as the heart, the gizzard of birds, and the swimming bladder of a fish. Marey points out that he even gave the first correct explanation of the flight of a bird. Such analyses of animal movements as Borelli did make were made by means of the unaided eye.

Graphic Method.

One of the most illustrious names associated with the Royal Institution of Great Britain is that of Thomas Young, who recorded the movements of a vibrating rod on a moving cylinder about the beginning of last century.

In physiology, however, the foundation of the "Graphic Method" came about in another and quite remarkable way, when Carl Ludwig, in the small University town of Marburg, on December 12, 1846, recorded on a moving surface the beats of the heart as expressed in the variations of the pressure in the arteries coincident with every pulse-beat, and the larger undulations corresponding to every breath—to every rise and fall of the chest-wall. Leaving aside the extraordinary development of the graphic method in its application to the arts and in science, let us pass from mechanical methods of recording events to chemical methods, as exemplified by chemically sensitized surfaces exposed in a camera—methods which have rendered the pursuit of cinematography not only useful and attractive, but practical, and practical in the sense of being lucrative. It is only in its infancy as a means for investigating, recording, and solving some of the most obscure phenomena of animal mechanics, which, on account of their rapidity or complexity, have not been solved by other means.

Luminous Stimuli.

Whatever may be the exact nature of the events that take place in our visual apparatus when it is stimulated by a luminous stimulus, this we know, that it is excited by slight stimuli, and even by stimuli of exceedingly short duration. A lightning flash, although it lasts only about one-millionth of a second, lasts long enough, not only to stimulate the retina, but to be seen; and so fleeting is it that a disk or other object moving with great rapidity, when illuminated by an electric spark, appears as if it were stationary.. A stimulus of even shorter duration, such as a beam of light reflected from a rotating mirror, will stimulate the visual apparatus though it may last only for one eight-millionth of a second. The visual response is immediate, and there is no latent period that is determinable. An inconceivably brief flash therefore not only affects the retina, but it sets up changes which last for a measurable time. Charpentier has shown that the after-vibrations of the retina occur at about thirty-six per second. If, however, a succession of luminous impressions of the same kind be applied to the retina with sufficient rapidity, they produce the same effect as a continuous illumination. Applying this to the retina itself, then, as Helmholtz puts it, the repetitions of the impressions must be of such rapidity of sequence that the after-effect of the previous impression must not have noticeably diminished before the next impression falls on the retina. The persistence of vision applies to coloured as well as to uncoloured objects. As the impression lasts about one-tenth of a second, its disappearance is sometimes spoken of as death of an impression. One of the easiest ways of showing this is by means of a rotating disk with alternate white and black sectors, or one or more white or coloured spots on a disk, made to revolve with sufficient rapidity.

Rotating disks of this kind were first described by Muschenbrock in 1700. A heavy metal disk, about 6 inches in diameter, mounted on a vertical axis, moves about six revolutions per second. In order to get fusion there must be four to six sectors, black and white, of the same breadth, arranged alternately on the disk.

Persistence of Vision.

One of the earliest recorded experiments in physiological optics, that of persistence of vision, is said to be mentioned by Ptolemy in his work on Optics in A.D. 140. "If a sector of a disk be coloured in patches at various distances from the circumference, and then rapidly revolved, the sector will present the appearance of a series of coloured rings." The phenomenon of "after-images" was known to Alhazen in the twelfth century, and to Leonardo da Vinci in the fifteenth century. Hallam, in his "Introduction to the Literature of Europe," claims "the right of Leonardo to stand as the first name of the fifteenth century." His unpublished MSS. "contain discoveries and anticipations of discoveries within the compass of a few pages, so as to strike us with something like the awe of preternatural knowledge."

After-images were also known to that strange "curiosity of literature," mathematician, astrologer, eccentric medical man of the sixteenth century, Jerome Cardan, who—according to Bayle and Henry Morley—when he passed through London was consulted about the health of Edward VI., to cast his horoscope and foretell his fate. The King died a few months afterwards in spite of the astrologer's forecast of a longish life.

He was more successful when he visited St. Andrews in 1550 to attend Archbishop Hamilton, who is said to have been cured of some affection of the chest by this strange forerunner of that still stranger and most singular personality Theophrastus Bombast von Hohenheim, better known as Paracelsus. The invention of the zoetrope is even attributed to Cardanus.

Comparison of Muscle and Retina.

We know that when a muscle is stimulated with an adequate stimulus it contracts. If, however, a sufficient number of stimuli per second are applied to the muscle—directly or indirectly—there is a fusion of the contractions, and the condition known as Physiological Tetanus is set up. There is a striking analogy between a muscular contraction and excitation of the retina. The muscular contraction outlasts the duration of the stimulus exciting it: the curve of contraction rises slowly, reaches its height, and then declines—the duration of a simple twitch of a frog's muscle being about one-tenth of a second.

The retinal curve of excitation follows a somewhat similar course, while the retinal excitation outlasts the luminous stimulus by about one-eighth of a second.

Flicker—Fusion of Effects.

This "after-effect" or "after-vibration" is more or less true of all our sense-organs. The direct immediate effect outlasts the duration of the stimulus. This is the basis of corporal punishment administered through the sensitive cutaneous nerves.

When a given retinal area is stimulated by intermittent stimuli at a certain rate, the disagreeable sensation of "Flicker" is set up, which is due to a rhythmical unpleasant rise and fall of the luminous sensations comparable to an "Incomplete Tetanus." If the rate of stimulation is increased, there is fusion of the individual stimuli by the visual apparatus—or retino-cerebral mechanism—and the result is a continuous sensation of light, such as is seen when a rocket is discharged or catherine wheels revolve, or when a lighted stick is rapidly whirled round.

Exner has shown that if the light stimulus increases in geometrical progression, the time during which it must act to produce its maximum effect decreases nearly in arithmetical progression, and this time is about one-quarter of a second. For fusion of the effects of luminous stimuli, however, they must follow each other at a more rapid rate than four per second. The results of stimulation of the retina have been formulated in the form of a law, that bears the name of "Talbot's Law," or "Talbot-Plateau Law." It is found that when once flicker is established any further increase in the rate of rotation of the disk produces no change in the character of the sensation.

The fact, then, that our retino-cerebral apparatus can fuse, or summate the effects of intermittent stimuli, falling on the retina so as to give rise to the sensation of a continuous luminous impression, is the basis of cinematography. Fusion of stimuli occurs with extraordinary readiness in visual stimulation—in this respect presenting a marked contrast to our auditory sensations.

Stimuli of adequate frequency cause an uninterrupted visual sensation to be produced. The fusion of sufficiently rapid intermittent stimuli is easily demonstrated on the colour wheel. To get rid of flicker and to obtain complete fusion, it is found that the speed of rotation depends on the intensity of the illumination as well as on the brightness value of the stimulus. The more intense the luminous sensation, the greater must be the number of revolutions to produce a complete fusion. There are two kinds of "flicker" before complete fusion is obtained: "coarse" flicker, which has a certain glittering character, and a "fine" flicker, passing into complete fusion of sensations.

Zoetrope, Cinematograph, and their Predecessors.

On this principle is constructed the well known "wheel of life," or "zoetrope." If the successive phases of a movement be drawn on a glass disk, and this disk be rotated in relation to an opaque disk with a slit in it, then on looking through the slit one sees the object as if it were in motion. This is the stroboscopic method, of which I will show you some striking yet simple examples.

The thaumatrope of Dr. Paris (1827)—now adapted as a children's toy—was an early precursor of the zoetrope, and consisted of a rectangular plate or card which is held between two strings and made to twirl. A cage is sketched on one side and a bird on the other. On rotating the plate on its vertical axis, the bird appears to be in the cage.

The stroboscope of Stampfer, and the corresponding apparatus of Plateau, which he called a "phänakistoscope," are well known. It appears that Plateau was led to invent his stroboscopic disk by some observations of Faraday on revolving wheels and disks and their appearances, published in 1831 in the first volume of the "Journal of the Royal Institution." Plateau sent an example of his apparatus through M. Quetelet to Faraday in 1832.

Naturally, with the development and perfecting of instantaneous photography first upon glass, and then on flexible sensitized films, this method lent itself admirably to the preparation of instantaneous photographs. If a series of pictures be taken one after the other, representing the successive phases of a motor event, it is obvious that if these pictures be re-presented to the eye with sufficient rapidity they will be fused, and the observer will see on the screen a representation of the original movement.

Helmholtz, in the second edition of his "Handbuch der physiologischen Optik" (1896), speaking of instantaneous photography, mentions the work of Muybridge and Anschütz, gives a picture of a horse jumping a hurdle, and adds "Diese Bilder sind auch für das Studium der thierischen Bewegungen wichtig." We have travelled a long way since then.

It is not with cinematography as a method for the representation of pageantry, or faked pictures, or spectacular drama in dumb show, or for the detection of crime, that I wish to deal to-night, but rather with this art as useful for showing reproductions of physiological and biological experiments to large audiences either in school, college, institution, or university, and also as a means of solving some of the features in movements that, because of their rapidity, cannot be analysed by other means.

M. Janssen, Professor Marey, and E. Muybridge.

Here I should like to pay my hearty tribute of admiration to the genius of my old master, the late Professor Marey, of the Collège de France, Paris, who was practically the pioneer of this method in its application to science. A visit to the "Institut Marey" in the Parc des Princes, and a survey of the numerous beautiful films made by him of botanical, zoological, and purely physiological phenomena, show one what marvellous work he did. He constructed apparatus whereby one hundred images per second could be obtained.

The late M. Janssen, the eminent astronomical observer in 1874, was the first observer to use an automatic arrangement for obtaining a series of photographs representing successive phases of an object in motion. He inaugurated chrono-photography by photographing on a circular glass plate the transit of Venus, the first chrono-photograph ever made. It was the parent and pioneer of that numerous progeny of various forms of apparatus designed for imprinting on a celluloid film "pictures in motion"—"living pictures," if you will—and, indeed, all kinds of movements, even those only visible by means of the microscope. The apparatus he called an "astronomical revolver." The plate revolved at regular intervals of seventy seconds through a small angle, and at each turn a fresh impression was made on the plate. He obtained a dark silhouette of the planet on the sun's surface.

He also suggested the photographic method as one likely to be of value for the study of biological and mechanical problems, such as walking, flight of birds, and other animal movements. At that time the difficulty was the sluggishness of the photographic plates used; now this and other difficulties have been overcome. This method gives an analytical reproduction of any movement by representing its elementary phases, a method exactly the reverse of that of the phenakistoscope or zoetrope, which reproduces a movement by means of a series of views representing the successive component phase of that movement.

Some of you may remember the work of Muybridge exhibited by him several years ago by what he called his "Zoo-praxiscope."

Muybridge did excellent work, more especially in the analysis of the movement of horses and other animals. On one side of a track was placed a screen to reflect the sunlight to the battery of twenty-four cameras placed on the opposite side of the track. The screen was marked at equal distances to indicate the rate of progression of the animal, and across the track at suitable intervals were stretched a series of wires, each of which was connected with an electro-magnet which kept the shutter of its corresponding camera closed. The horse in passing along the track broke the wires one after the other, the corresponding shutters were instantaneously opened, and thus a series of photographs were obtained. Sometimes three batteries of cameras, placed in different positions, were used to obtain movements of the body and limbs from different points of view. Once the pictures were obtained they were drawn on glass disks, which were placed in the focus of a projecting lantern, and made by an ingenious arrangement to rotate after the manner of a phenakistoscope, and they were thus projected on a screen and the movement reconstituted. Muybridge called his apparatus a "Zoo-praxiscope."

About 1881 Marey invented his "Photographic Gun." In the breech there were placed small sensitized glass plates, and a perforated disk, which rotated twelve times per second, but was arrested intermittently to allow of an exposure of $\frac{1}{750}$ of a second, i.e. the disk came to rest twelve times in a second just opposite the beam of light passing into the instrument. It was a very heavy apparatus, and was used for the study of flight in birds. The pictures obtained by Marey he combined in a zoetrope.

Onimus and Martin, as long ago as 1865, used a photographic plate to record the beats of the heart. The lens of the camera was left open, and no shutter was used. The result was a double outline representing the extreme phase of contraction and dilation of the heart; but there was no clear impression of the intermediate stages —i.e. the "positions of visibility" were recorded at what are called the "dead points," i.e. when the direction of the movement is changed.

Marey made extensive use of "fixed plates" for the recording of such movements as jumping, walking, vaulting, fencing, carried out against a dark background, the person making the movement being dressed in white garments.

The Cinematograph.

In the ordinary cinematograph a continuous narrow band of flexible sensitized film moves behind the lens of the camera, and as it moves it is checked for an instant and is stationary each time the shutter is opened and the exposures made, and then the film moves rapidly on in the intervals of darkness when the shutter is closed, so as to expose a fresh surface of the film to light. The dissociation of the successive images is obtained by moving the sensitized surface. Usually about sixteen impressions per second are taken for ordinary purposes. Even at this slow speed about 1000 impressions per minute fall on the retina. For an animal running rapidly, or an acrobat or cat turning somersaults in the air, forty to fifty impressions per second suffice.

The cinematograph is the modern evolution of chrono-photography. For this purpose two pieces of apparatus are required. First, a "Producing Apparatus," or "Camera," to take impressions of a moving object. A film of celluloid covered with sensitized emulsion is moved behind a rapid short focus lens at such a uniform speed as to produce a series of pictures when successive parts of the film are exposed.

A crank is turned about twice per second, and the works are so geared that about sixteen fresh surfaces are exposed per second. Brücke found that with black and white sectors on a disk the greatest brightness corresponded to 17·5 stimuli per second, a result confirmed by Burch. Higher rates, of course, are easily obtained, but for ordinary work not less than sixteen pictures per second, the height of each picture being 18·5 mm., or ¾ inch. Therefore, if sixteen pictures per second—i.e. one foot of film—are passed before the eye by means of the cinematograph "Projecting Apparatus" in one second, the operator, if working without any interruption, runs off sixty feet of film per minute. When the cost of the negative and positive films is taken into account, an uninterrupted cinematograph exhibition for ten minutes costs for film alone about £30, or £3 per minute.

Lendenfeld was one of the first, in 1903, to obtain two thousand photographs per second, not by moving the sensitized surface, but by displacing the object itself in relation to the sensitized surface.

At the British Association Meeting held in Sheffield in September last I had the honour of showing a number of films prepared by my friend Dr. Comandon of Paris by means of what is called "dark ground illumination," e.g., the movements of Paramecium, Trypanosomes in blood, and the Microbes in the intestine of a mouse. I do not propose to show you these films on this occasion.

Dark Ground Illumination.

Wenham by means of a parabolic condenser, more than half a century ago, used this method to illuminate microscopic objects. The light is not transmitted through the object on the stage of the microscope, but is made to fall on the microscopic object and reflected from it, so that the object appears bright on a black ground, a method familiar to bacteriologists as well as to amateur microscopists.

Wenham's dark ground illumination has led to that extraordinary development of making the invisible visible, known as "Ultra-Microscopy," and to instantaneous photography of Trypanosomes and other transparent organisms in blood and other fluid, e.g. Spirochetes. Brownian movements on a dark ground I saw some years ago in the laboratory of my friend Professor Dastre in his laboratory at the Sorbonne in France.

Analysis of a Cinematogram.

If one looks at a cinematograph film (e.g. an animal in motion) by way of analysing the successive phases of a movement, one is struck by what seems the impossible positions or attitudes of such an animal in its onward progress—e.g. a galloping horse with its legs pulled up beneath its body, a position which no human eye ever saw or artist depicted. The artist represents what he sees, and the spectator demands that what is depicted shall correspond to what he himself can see in a moving object. The sensitized film of the camera fixes phases of movement that the eye is not sensitive enough to distinguish. But fuse these apparently impossible poses by means of a cinematograph, and one realises what relatively feeble analytical powers the eye possesses as compared with a moving sensitized film of celluloid in the cinematograph camera.

BRIEF NOTES ON THE FILMS.

M. Gaumont.

The cinematograph films which I have the honour of showing you to-night were made expressly by "The Gaumont Co., Ltd.," to illustrate the value of cinematography in biology, physiology, and allied sciences. I desire to express publicly my best thanks not only to "The Gaumont Co., Ltd.," but also to M. Gaumont himself, who has taken a kindly scientific interest in the production of these films. Without his assistance I could not have undertaken this lecture to-night. Nor do I forget what I owe to my old friend Professor François-Franck, of the Collège de France, the successor of the late Professor Marey.

Artificial Amœba. Amœboid Movements and Surface Tension.

Many attempts have been made to explain the amœboid movements of protoplasm manifested by an amœba or by colourless blood corpuscles, as being due to purely physical conditions, more especially to variations of surface tension, a view associated with the names of Quincke, Bütschli, and Rhumbler. In 1878 Gad made the following experiment on surface tension. He placed a drop of rancid oil on a dilute solution of carbonate of soda. The fatty acid acts on the oil and a soap is formed; this lowers the surface tension at various points of the drop of oil, and as a result the oil drop changes its form and sends out and retracts processes having a superficial resemblance to the pseudopodia of amœba. This may be called an "Artificial Amœba."

Rhumbler made the following ingenious experiment, to indicate how, in his opinion, an Amœba can take into its body and there coil up a filamentous alga, as figured by Leidy in his magnificent work on the Rhizopoda. A drop of chloroform placed in water takes in and coils up within itself a thin hair-like filament of shellac.

Globule of Mercury Pulsating like a Heart.

When a metallic needle is brought in contact with mercury placed in a watch-glass and the mercury is covered with a dilute solution of bichromate of potash and sulphuric acid, the mercury begins to pulsate or beat rhythmically in the most regular manner—a phenomenon described in this form by Ostwald and hence called "Ostwald's Physical Heart." This phenomenon is also due to alteration of surface tension. The beats are seen with great distinctness in the cinematograph film.

Ciliary Motion and what it can do.

The movements of cilia were shown by means of a Stroboscope, so that the audience could follow the movements of the individual cilia—both when moving slowly and when vibrating quickly, as it were under the influence of heat.

To illustrate what ciliary action can do in the way of transport, the head of a dead brainless frog with the lower jaw removed was shown. On the front of the palate was placed a large piece of the animal's liver and on this a small flag. Liver and flag were seen to be rapidly carried downwards right into the gullet of the frog. The experiment illustrates the large amount of work that can be done by the combined co-ordinated action of thousands of these microscopic vibratile hairs called cilia, and also their importance in the removal of secretions and particles from the passage which they line.

Movements of Starfish and Sea Urchins.

The movements of the tube feet of various Echinoderms and the movements of the animals themselves have been carefully studied by Preyer, Romanes, Uexküll, Loeb, Jennings, and others.

Some of the starfish, such as Astropecten, rapidly bury themselves in the sand. A film showing this process was exhibited.

Starfish in Confined Space.

The movements of a starfish placed on its back and gradually "righting" itself were shown, but a more interesting phenomenon is to watch the movements of the tube feet and arms of a starfish placed on its back in a confined space, until it finally succeeds in righting itself and arranging itself in the vessel on its ventral surface.

Jennings has carefully studied these movements, as well as the movements of the tube feet of a starfish catching a fish or a crab. Slides to illustrate these remarkable modes of capturing prey were shown.

Sea Urchin—Tube Feet and Pedicellaria.

The movements of the tube feet of a sea urchin in water were also shown, and in the same animal the movements of its pedicellaria as well as those of "Aristotle's lantern" could be distinctly seen.

M. Bull and Flight of Insects.

The cinematograph has been applied with conspicuous success to the study of the flight of insects, by M. Lucien Bull, of the Institut Marey, Paris. M. Bull assisted the late Professor Marey with his experiments on this subject.

Two Thousand Photographs per Second.

When it is remembered that the movements of the wing of the common fly occur at the rate of 330 vibrations per second, the bee 190, the wasp 110, and the dragon fly 28, it will be seen that there are many technical difficulties to be overcome. M. Bull, by the means of his ingenious "Electro-stereo-chronophotograph," has been able to take photographs of a moving object at the rate of two thousand impressions per second on a sensitized film. As is well known, the electric spark—with a duration of one-millionth of a second—affects our singularly sensitive visual apparatus most markedly. Photographs have been often taken by means of a single spark. M. Bull, however, has constructed an apparatus whereby a series of two thousand such impressions can be obtained per second by means of the electric spark.

In a special camera is placed a revolving wheel, on the circumference of which is arranged the sensitized film. The electric spark is placed behind a large condensing lens which concentrates the rays directly into the lens of the camera. The wheel, actuated by an electrical motor, is rotated at a speed of about 150 kilometers per hour, i.e. it makes one revolution in one-fortieth of a second. In circuit is placed the electric illuminant—the sparks passing between two magnesium terminals. The wheel, however, is so arranged as to give fifty interruptions at each revolution, so that in this way two thousand sparks per second are obtained—the duration of each spark being practically infinitesimal—while the intervals between the sparks are practically a thousand times the duration of the sparks themselves. The whole system may be regarded therefore as comparable to a shutter opening and closing two thousand times per second.

The next problem was how to expose the film at the proper moment. A similar problem was solved in an ingenious way by Helmholtz long ago when he invented his Myograph with a small cylinder, on which he recorded the rate of transmission of a nerve-impulse along the motor nerve of a frog—really a comparatively slow velocity—about ninety feet per second.

By means of an ingenious shutter the exact exposure was made at the right moment, and this was accomplished indirectly by the insect itself, which was placed in a short length of blackened glass tube closed at one end by means of a light movable mica swing door, which is placed in the electric circuit. The door is kept slightly ajar by means of a small piece of tissue paper. When full speed of the wheel is attained, and when the insect—already placed in the tube—moves forward in the tube towards the light and opens the swing door, the paper falls and the electric circuit is made. By an ingenious arrangement the shutter is placed in this circuit so that the insect automatically opens and closes the shutter, and as it passes across the field of the stereoscopic camera—a distance of a little over a foot—impressions of the phases of its transit can be taken at the rate of two thousand impressions per second.

M. Bull has also photographed the passage of a bullet through a soap bubble. The air gun used to propel the pellet is placed in circuit with the shutter used for the exposure of the moving pellet as it traverses the soap film.

Through the kindness of M. Bull I am able to show you a beautiful film of the flight of a dragon-fly and also of a blue-bottle taken by means of his method.

Mechanical and Chemical Stimulation of a Reflex Frog.

A film of a "reflex frog"—i.e. an animal in which the brain is destroyed, but still retaining its spinal cord—was shown, to illustrate a reflex unilateral motor act in response to a mechanical stimulus of the skin of one leg. To contrast with this striking unilateral response, the extensive responses that occur in a similar frog where the stimulus is chemical in its nature—viz. the application to the skin of a small piece of paper dipped in dilute vinegar—was shown. By adaptive "purposive" movements, the animal succeeded in removing the offending object—a good example of a protective reflex act or motor response. By means of a metronome beating sixty times per minute and placed alongside the suspended frog, the audience could determine the time the frog took to dislodge the offending object.

Spread Reflex of the Turkey Cock.

The "spread reflex" of the turkey cock described by L. Hill was shown:—

"If a turkey cock is placed sitting on a narrow stool and the wings are sharply drawn downwards and forwards, the tail is spread. This can be repeated as often as one likes, the animal behaving in this respect like a mechanical doll. The same thing happens when the turkey is standing, but in this case the disturbance of balance which occurs on pulling down the wings somewhat lessens the effectiveness of the spread of the tail. On lifting up and spreading the tail, there occurs no reflex movement of the wings. The reflex only works one way." (March 1910.)

Equilibration and "The Acrobatic Frog."

The mechanism for the maintenance of the equilibrium of the body is one of the most complex in static physiology. Many factors are concerned in this process, including the impulses sent from the semi-circular canals. The late Prof. Goltz, many years ago, removed the cerebral lobes from a frog, all in front of a line joining the foremost margin of the exposed tympanic membranes. Such a frog, if placed on a horizontal surface, sits, but does not move; if placed on its back, it immediately "rights" itself. If, however, the board on which it is sitting be slightly tilted, the frog moves upward on the board; and if the board be rotated, the frog still continues to move onward; and when the board is vertical, the frog rests and sits or perches quiescent on the edge of the board. As soon as the board is again tilted, the frog moves correspondingly, and one can make it crawl up and round the board as often as one chooses by simply turning the board on its long axis. I have called this "The Acrobatic Frog." An intact frog does not behave in this way. When the board is tilted, it jumps, while one with its spinal cord only, falls as soon as the board is sufficiently tilted; nor does such a frog right itself to the sitting position when it is placed on its back.

Breathing and Artificial Inflation of the Lungs.

The lungs as they appear in a dead rabbit after the chest is opened were shown, and then the rhythmical filling and partial emptying of the elastic lungs when artificial respiration or inflation of the lungs is performed through a cannula placed in the wind-pipe, an experiment well calculated to show the collapse of the lungs when the chest is opened, as well as the elastic properties of the lungs themselves.

X-Rays and Movements of the Intestinal Tube.

The movements of swallowing food mixed with subnitrate of bismuth—which is opaque to X-rays—the passage of the food from the gullet into the stomach, and from the stomach into the intestine, were made visible by means of a film prepared by M. Carvalho. The film was obtained from a frog which had been fed with bismuth mixture. The contractions of the intestinal tube propelling onwards its contents could be followed with the utmost distinctness.

The Heart Beating. Arrest of the Heat.

A series of films was projected to show the beating of the heart in situ in a dead frog. Alongside it was shown the lever of a time-marker indicating seconds. The arrest of the beat of the frog's heart by electrical stimulation of an appropriate area of the heart itself, or the branch of the vagus nerve passing to the heart, was shown, as well as a frog's heart recording its beats on a blackened revolving drum.

Excised Heart of Tortoise Beating. Effect of Heat.

The excised heart of a dead tortoise, suspended and still living and beating, exhibited the changes of form of the empty heart during the contraction and relaxation of its several chambers; while another similar excised heart was shown beating on a glass plate. The effect of heat on the heart-beat was clearly demonstrated.

Effect of Chloroform on the Excised Heart of a Rabbit.

Lastly, the excised heart of a dead rabbit, placed in connexion with a perfusion apparatus supplied by Locke's fluid, so arranged as to record its movements on a long strip of blackened paper moving at a definite rate in front of the recording lever attached to the heart, was shown. The same heart was then perfused with fluid containing chloroform, and the audience could see how gradually but steadily the heart ceased to beat, as if it were dead. But, no, on washing out the chloroform with fresh Locke's fluid, the audience could see how the apparently dead heart gradually resumed and recorded its beats until its full vigour was established. The heart was prepared and the apparatus arranged by Dr. Locke.

In contrast to the action of chloroform, the same heart was shown subjected to the action of adrenalin, which threw the heart-muscle into an extraordinary state of excitement, visible alike in the heart itself and in the record on the smoked surface of the recording apparatus.

[W. S.]

WEEKLY EVENING MEETING,

Friday, May 19, 1911.

THE RIGHT HON. EARL CATHCART, D.L. J.P., Vice-President, in the Chair.

PROFESSOR R. W. WOOD, LL.D., Professor of Experimental Physics, Johns Hopkins University.

Recent Experiments with Invisible Light.

BY far the greater proportion of the discoveries which have been made in Natural Science up to the present time depend upon observations made with the eye, either with or without the aid of optical instruments. The eye is, however, sensitive to only a very small part of the total radiation which reaches it, and it seems not unlikely that, if its range could be extended, many new phenomena would immediately come to light. By the employment of photography and of instruments which detect and measure the intensity of the infra-red or heat rays, much new information has been gathered, especially in the science of spectroscopy; but usually these methods have been applied only in cases where the invisible radiations were known to be present. It seemed quite probable that if photographic methods were applied to various physical phenomena which excluded the action of any but invisible rays, new facts would probably be discovered. I can illustrate what I mean by taking two striking cases which were found at the very outset of the investigation, and which will be more fully discussed presently.

If the finger be dipped into powdered zinc oxide and rubbed over a sheet of white paper, eye observation is absolutely unable to detect the presence of the streaks made by the white powder, unless it has been very thickly applied. If, however, we photograph the paper with ultra-violet light, we obtain a picture in which the streaks are as black as if made with powdered charcoal. This suggests that if we apply the process to the photography of the moon and planets, we have some reason to suspect that substances which cannot be detected visually may come out in the photographs, a surmise which has been justified in one case at least. This and other similar cases will be taken up in detail presently.

As an illustration of how the method may be applied to the investigation of various physical phenomena, we may take another interesting case, in which a new radiant emission from the electric spark has been discovered. It was suspected that the very short waves discovered by Schumann, which are powerfully absorbed by air, might possibly render the air fluorescent, the emitted light being invisible, however, on account of its short-wave-length. A heavy spark discharge was accordingly placed behind a small disc of metal, which cut off all the direct light, and the surrounding region photographed with a quartz lens, which is transparent to the ultra-violet rays. It was found that the air in the neighbourhood of the spark actually did give off actinic invisible rays, the photograph giving the impression of a luminous fog surrounding the metal disc.

I will now show you an experiment which illustrates that two objects which cannot be distinguished under ordinary illumination may appear quite different when the light which illuminates them is restricted to certain regions of the spectrum. I have here two pieces of scarlet silk which cannot be distinguished the one from the other in the light of the incandescent electric lamps which illuminate this room. I now extinguish the lamps and place the two pieces of silk under this Cooper-Hewitt mercury arc lamp, and as you see one of them still appears scarlet as before, while the other appears very dark blue, almost black, in fact. The peculiarity of the mercury lamp lies in the fact that it gives out little or no red light, consequently red objects in general appear almost black. The peculiarity of this particular piece of silk, by virtue of which it appears quite as red as in ordinary lights, lies in the fact that the red dye with which it is coloured is fluorescent under the action of the green rays from the lamp: the red light is manufactured, so to speak, from the green light by the colouring matter of the silk. If I place the arc lamp and the piece of silk behind this large sheet of red glass, you will observe that the fabric is actually brighter than the lamp itself, probably eight or ten times as bright. We can form an image of the lamp on the silk with a lens, and the image will be many times brighter than the lamp, which might be taken as a refutation of the old and well-known theorem in optics that no optical system can yield an image brighter than the source (!) Here is another piece of white silk upon which I have made some red spots with this same dye. By the ordinary illumination of the room it is seen to be white, with large pink polka dots, something quite suitable for a young lady's summer gown! I now place it behind the red screen under the mercury arc and it at once becomes quite diabolical in appearance, bluish-black with flaming spots of scarlet, entirely unsuitable for the aforementioned purpose. The dye which was used for colouring these fluorescent fabrics was rhodamin. The conditions of illumination and observation are, of course, rather special in these cases, and I have introduced them merely to illustrate how the eye may be deceived under certain conditions.

Practically all sources of light in ordinary use give out more or less ultra-violet light which plays no part in vision, but which can be rendered apparent in various ways. I have on the table a new arrangement by which these rays can be separated from the visible ones. The apparatus is practically identical with the device quite recently used by Professor Rubens and myself for isolating the longest heat waves that have been discovered up to the present time. It can be used as well for the isolation of the ultra-violet, since its action depends upon the high refractive index which quartz has for these two types of radiation. The source is, in this case, an electric spark contained in this box, and the ultra-violet rays are brought to a focus upon a small circular aperture in a cardboard screen. The focal length of the lens is so much greater for visible light that these rays do not come to a focus at all, but are spread over a circular area of a diameter nearly half that of the lens.

A penny has been fastened to the centre of the lens with wax, and this shields the aperture from the cone of visible rays coming from the central portions of the lens. If I hold a sheet of white paper above the aperture you observe that it remains dark, that is, no visible rays pass through to the paper; if, however, I substitute for the paper this mass of uranium nitrate crystals, the presence of the ultra-violet rays is made manifest, the crystals shining with a brilliant green light.

Certain vapours shine with a brilliant light when exposed to these invisible rays. One of the most striking is the vapour of metallic mercury, which I can show you by boiling the metal in this flask of fused quartz placed above the aperture. The metal is boiling now, and you can all see the brilliant cone of green light which marks the path of the ultra-violet rays through the metallic vapour. If I hold a thin sheet of glass between the aperture and the flask you will observe that the vapour instantly becomes dark, for the glass stops completely the rays in question.

The vapour of mercury exhibits an absorption band in the ultra-violet region which resembles the band at wave-length 5893 shown by dense sodium vapour. So powerful is this absorption that I have detected it in the vapour of mercury at room temperature. It occurred to me that this light instead of being absorbed might possibly be re-emitted by the vapour laterally in all directions. To test this point I sealed up a drop of mercury in an exhausted flask of quartz, and focused the light of the mercury arc (burning in a silica tube) at the centre of the bulb, which was not heated. The bulb was then photographed with a quartz lens, and the picture clearly showed the cone of focused rays precisely as if the bulb were filled with smoke. This is another very good example of how new discoveries may be made by ultra-violet photography.

If the object to be photographed gives off visible rays in addition to the invisible ones, it is necessary to remove these by a suitable screen or ray filter. We will begin by considering some remarkable effects which are obtained when sunlit landscapes are photographed

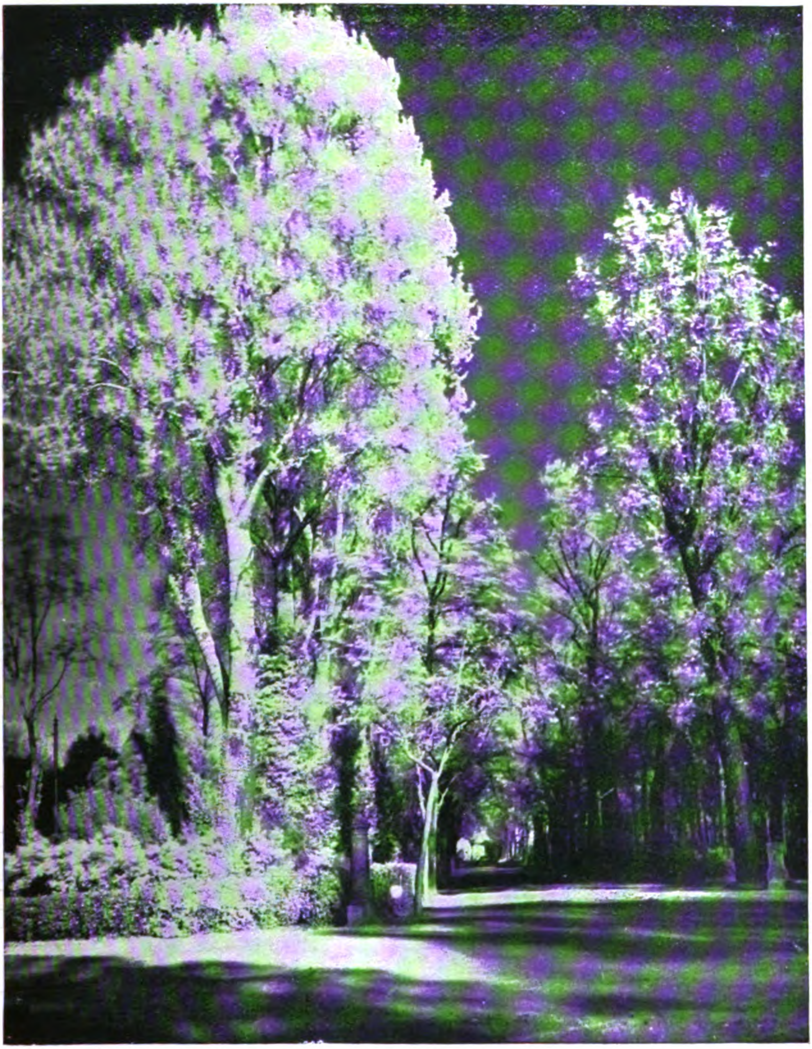


FIG. 1. PARK IN FLORENCE.

Photographed by infra-red rays.

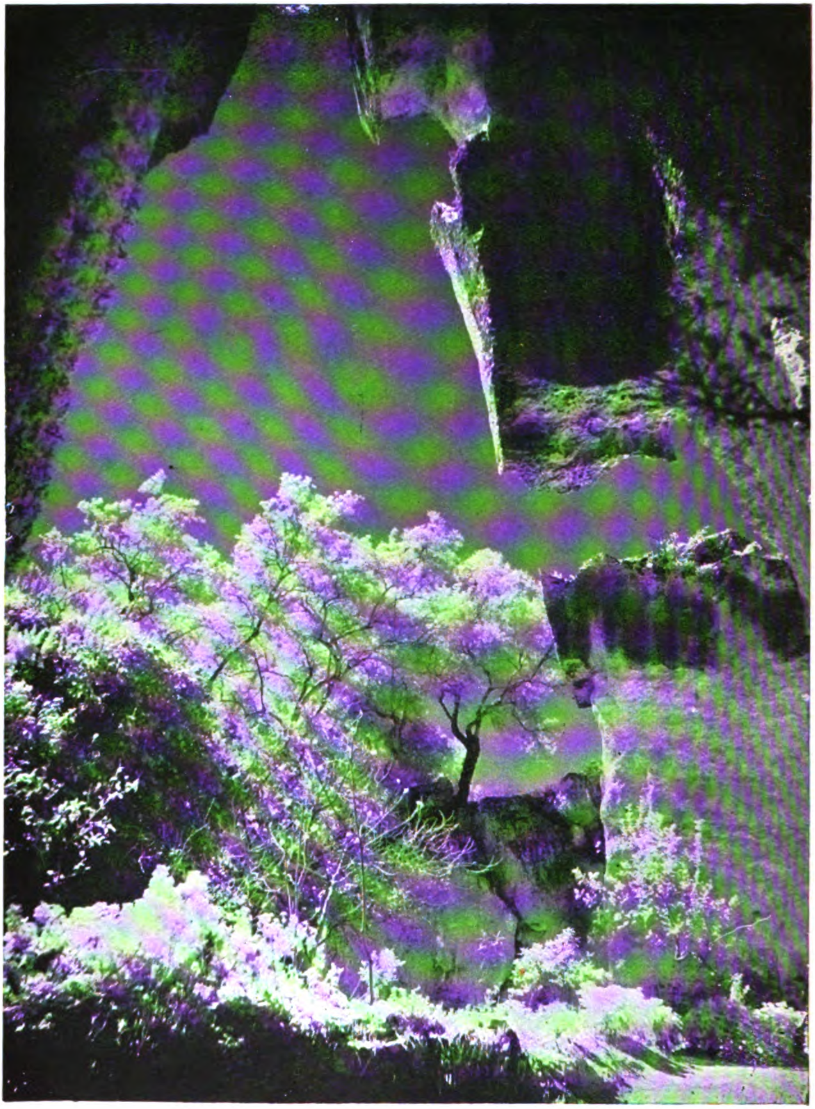


FIG. 2. QUARRY IN SYRACUSE.

Photographed by infra-red rays.

by means of the obscure rays at the extreme red end of the spectrum. A screen can be prepared which transmits these rays, and is at the same time opaque to all other radiations, by combining a sheet of the densest blue cobalt glass with a solution of bichromate of potash or some suitable orange dye.

Such a screen transmits a region of the spectrum comprised between wave-lengths 6900 and 7500. Though this region is visible to the eye if all other rays are cut off, it is so feeble in its action that it plays no part in ordinary vision, being overpowered by the other radiations. We may thence, for convenience, call photographs made through such a screen infra-red pictures, though the infra-red region is usually considered as beginning at the point where all action upon the human retina ceases.

The photographs which I am now going to show you were taken through such a screen, with the spectrum plates made by Wratten and Wainwright. The time of exposure was about three minutes in full sunlight, with the lens stop set at *f*/8. The views were, for the most part, made in Sicily and Italy, and have a very curious appearance, for while the sky comes out in all of them almost as black as midnight, the foliage of the trees and the grass come out snow white. This peculiar effect results from the failure of the atmosphere to scatter these long rays. The green leaves, however, reflect them very powerfully, or more correctly, transmit them, since we are dealing with pigment or transmission colour. If we look at a landscape through the screen, carefully protecting the eye from all extraneous light with a black cloth, we shall find that the trees shine with a beautiful rich red light against a black sky. This condition obtains only on very clear days, for the presence of the least haze in the air enables it to scatter the long rays, and you will notice that in those pictures which show the sky down to the horizon, there is a progressive increase in its luminosity as we pass from the zenith downwards, as a result of the greater thickness of the mass of air sending the scattered rays to the camera.

Another point to be noticed is the intense blackness of the shadows in the infra-red pictures, due to the fact that most of the light comes directly from the sun and little or none from the sky, which reminds one forcibly of the conditions which obtain on the moon, where there is no atmosphere at all to form a luminous sky.

When we come to the subject of photographs made with ultra-violet light, we shall find that we have the conditions reversed, for practically all of these very short waves are scattered by the atmosphere, and we have no shadows even in full sunlight.

We will now run through the series of infra-red pictures as rapidly as possible, for I have a considerable number of them. The one which is on the screen is one of the finest in the collection (Fig. 1). It was made in the park at Florence, and shows the long drive, overshadowed by trees, the one in the foreground being particularly fine in appearance. The next one (Fig. 2) was made at the bottom of one of the old quarries or latomiæ at Syracuse, the view looking out through a cave-like formation at a group of almond trees, with which the quarry is overgrown.

Here is a fine row of cypresses growing by an old gate, taken on a somewhat hazy day, with the sky appearing a little lighter than usual. Some of the pictures show the advantage gained in bringing out the detail of distant objects seen through the atmospheric haze, and it does not seem impossible that photographs of the brighter planets made through an infra-red screen might prove interesting if the planets are surrounded by a light scattering atmosphere, for we must bear in mind that the surface of the earth, as seen from a neighbouring planet, would be seen through a luminous haze, equal in brilliance to the blue sky on a clear day—that is, it would present much the same appearance as is presented by the moon when seen at noon-day.

We will now look into the question of how things would appear if our eyes were sensitive only to ultra-violet light. In applying the same method which we have used for the infra-red, we require a screen which is opaque to all visible light, but which transmits the ultra-violet.

Glass is opaque to these rays, cutting them off almost completely, and for this reason we cannot employ glass lenses. Quartz, on the other hand, is exceedingly transparent to these invisible rays, but it is a little difficult to find a medium which is transparent to them and at the same time quite opaque to visible light. Indeed, there is only one substance known which completely fulfils such a condition, namely, metallic silver. If we deposit chemically a thin film of metallic silver on the surface of a quartz lens, a certain amount of ultra-violet radiation between 3000 and 3200 is able to struggle through and form an image on the plate.

I have used silver films through which the filament of a tungsten lamp is invisible. The best thickness is that at which the tungsten lamp is just barely discernible. If the objects to be photographed are illuminated with the light of an electric spark, or some other source rich in ultra-violet rays, much thinner films of silver can be employed, but in the case of sunlight, which has passed through the earth's atmosphere, the ultra-violet in the region for which silver has its lowest reflecting power and greatest transparency has been so tremendously weakened by atmospheric absorption, that it is necessary to employ thick films and long exposures, otherwise the action upon the photographic plate results chiefly from the violet and ultra-violet rays, which are capable of traversing glass.

As an illustration of the behaviour of silver films of different thicknesses, used as ray filters, we may take some pictures which were made for the purpose of studying the reflecting power of various metals, suitable for telescope mirrors, for ultra-violet photography.

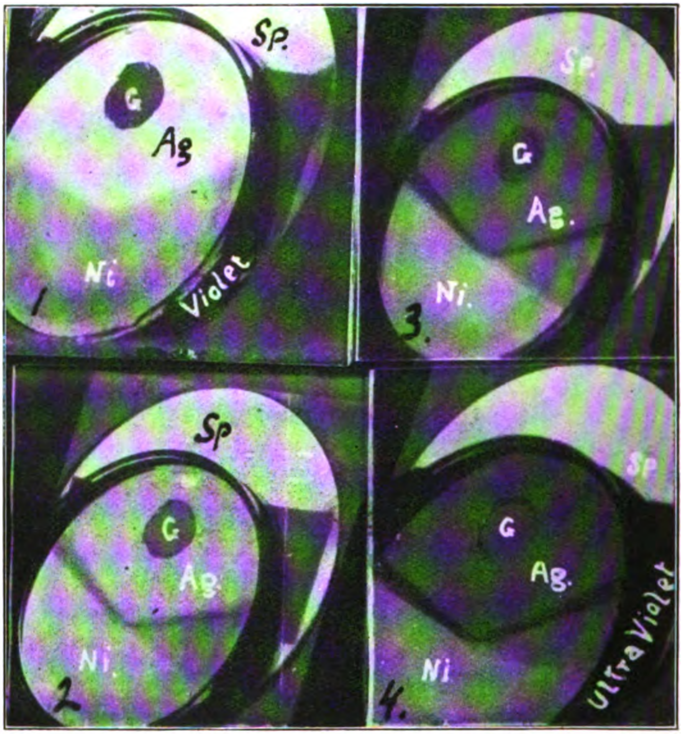
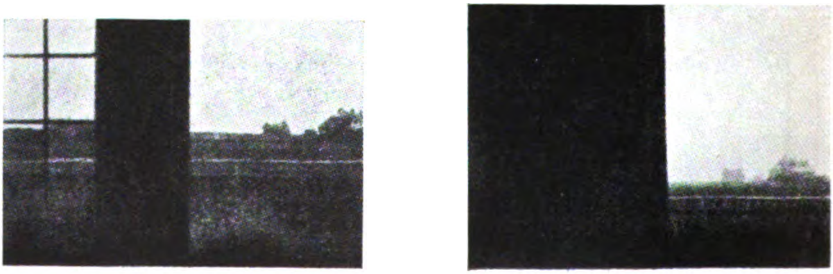


Fig. 3.



Visible Light.

Ultra-Violet Light.

Fig. 4.

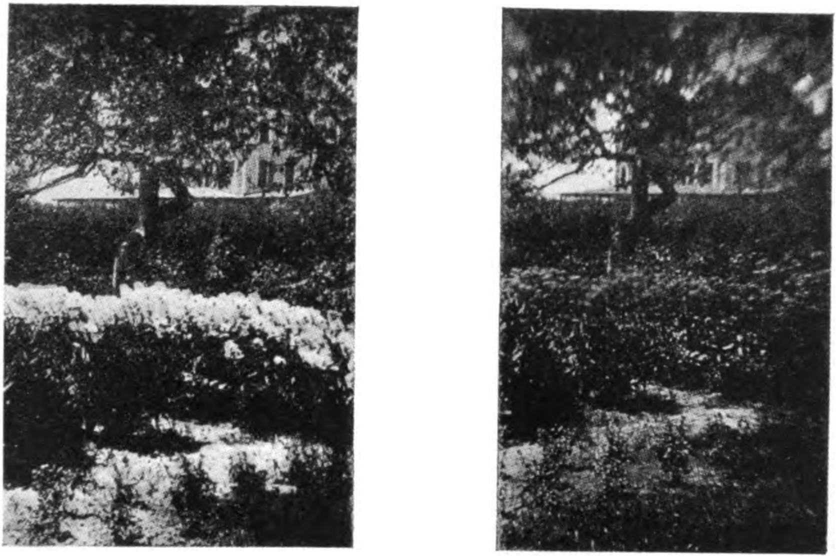
As silver reflects only about 4 per cent. of the ultra-violet in the spectrum range for which it is transparent, a silvered glass reflecting telescope for this purpose is obviously out of the question. Speculum metal is fairly suitable, but speculum mirrors of large size are troublesome, and difficult to procure. I accordingly worked out a method of depositing nickel on glass. The glass is first silvered, and then electro-plated with nickel, by a process which I have described recently in the Astrophysical Journal (Dec. 1911). The double sulphate of nickel and ammonia is used with one or two dry cells. The solution must be very dilute (10 grams or less to the litre), otherwise the nickel strips the silver from the glass. We have here four pictures of a silvered glass dish, partially plated with nickel (Fig. 3). The silvered portion is marked Ag, the nickel Ni, while at G we have a spot of clear glass from which the metal has been removed. The dish stands against a flat plate of polished speculum metal Sp, and the metal surfaces reflect the light of the sky to the camera. The first picture was made by blue and violet light without any ray filter, and as you see the glass surface G is quite black, while the silver reflects much more powerfully than the nickel. The following three pictures were made with a quartz lens, coated with silver films of increasing thickness. The silver and nickel reflect to about the same degree in the second picture, in the third the silver is much darker than the nickel, while in the fourth the silver is seen to reflect no more than the spot of clear glass G. This last was made through a film, through which a tungsten lamp was invisible. If these ultra-violet rays were visible to us, metallic silver would appear to have about the same reflecting power and appearance as anthracite coal.

We will next take up the action of our atmosphere on these ultra-violet rays. I have taken two photographs of a man standing in the road in full sunshine, in the one case by ordinary light and in the other by ultra-violet radiation. In the latter the shadow is completely absent. Ultra-violet behaves in exactly the opposite way to the infra-red. The infra-red rays are enabled to drive through the atmosphere without being scattered laterally by the molecules of the air or the dust particles. The short or ultra-violet rays, on the other hand, are completely scattered, so that the greater part of the ultra-violet light which reaches the surface of the earth comes from the sky and not directly from the sun. If our eyes were sensitive only to ultra-violet we should find the world appearing not greatly different from the aspect which obtains at the time of light fog. We should, indeed, see the sun, but it would be very dull, and there would be no shadows, just as there are none on a foggy day. We should walk the earth like Peter Schlemeil, the shadowless man of the German fable.

The next picture (Fig. 4) illustrates the opacity of ordinary window-glass to ultra-violet radiation. It will be noticed that there is no trace of the landscape seen through the glass window, although it is clearly rendered in the companion picture taken with visible light. Another difference to be noted in these pictures is that the flowers in the garden, which are white in the picture taken with visible light, disappear entirely in the picture taken by means of the ultra-violet radiation. The white garden flowers become almost black as is shown in Fig. 5, which shows white phlox photographed by visible and ultra-violet light. It occurred to me that this ability of the white flowers to absorb the ultra-violet rays might play some economic part in the growth of the plant. I therefore experimented with some flowers which had been grown under glass, and had thus been deprived of ultra-violet, but I was unable to find any marked difference between those which had been grown in the open and others which had been deprived of their full quota of this radiation. It is possible that if the experiments were carried on through the course of a number of generations we should find a difference. I have found, however, that all white flowers are not equally dark when photographed with ultra-violet light. White geraniums, for example, come out much lighter than common white phlox, which is practically black when photographed through the silvered quartz lens.

In order to demonstrate the difference in the appearance of one of the common pigments when viewed respectively with visible light and with ultra-violet radiation, some letters were painted in Chinese white on a page of a magazine. In the photograph (Fig. 6) taken with visible light the Chinese white appears as white as the paper itself, if not indeed whiter; but, photographed with the ultra-violet radiation, it comes out absolutely black. One may say that what is Chinese white in visible light becomes Japan black in ultra-violet. Under this radiation also black printer's ink becomes lighter than in visible light. This failure in the reflecting capacity of Chinese white is a source of some annoyance in reproducing drawings executed in part in this medium, as has been pointed out by Mr. A. J. Newton. In working with my Chinese white I made a mistake in one letter in the word "appears," and carefully wiped it out, leaving no trace of the correction discernible in visible light; but when the photograph was made with the ultra-violet, the erasure, otherwise invisible, showed as a black smudge. The ultra-violet camera is evidently very much more sensitive than the eye to the presence of traces of Chinese white on the printed page, for so far as I could see every particle of the pigment had been removed. Whether this has any bearing upon the detection of forgeries has yet to be discovered.

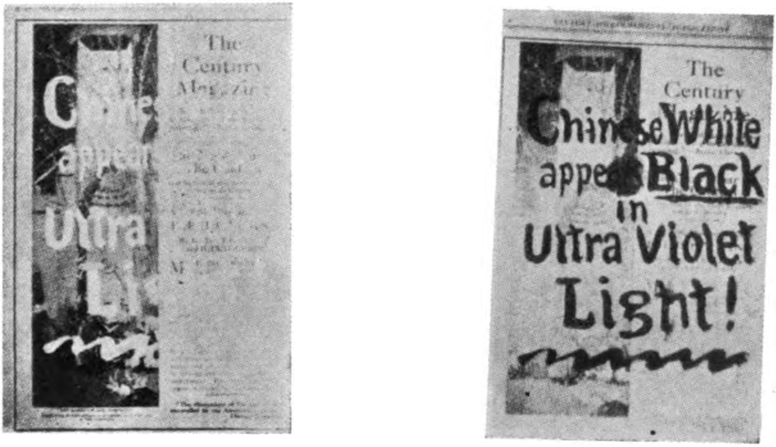
Another class of work in which this comparative study is likely to be of service is the photography of celestial bodies. For the full moon the exposure through the silver screen was two minutes with ultra-violet light belonging to the region 3000 to 3200. This length of exposure necessitated an equatorial telescope with some means of driving it to compensate for the moon's movement. The support for my telescope was the framework of an old bicycle minus the wheels. This carried a 4-inch refractor and a quartz-silver telescope, and by the



VISIBLE LIGHT.

ULTRA-VIOLET LIGHT.

FIG. 5.



VISIBLE LIGHT.

ULTRA-VIOLET LIGHT.

FIG. 6.

operation of a little screw it was possible to follow the moon accurately for half an hour. It will be seen at once (Fig. 7) that there is very little difference between the ordinary image of the moon and the one which is shown us by the ultra-violet radiation. Nevertheless, in the neighbourhood of Aristarchus, which is the brightest crater on the lunar surface, the photograph taken with the ultra-violet rays shows a dark patch which is absent on the one taken with visible light. I made an enlargement of the region in which this crater appears, and it is evident that there is in its neighbourhood a large deposit of some material which can only be brought out by means of the ultra-violet. These photographs of the moon make it appear extremely probable that by carrying on experiments of this nature on a larger scale we might get a good deal of new information as to the materials of which the moon is composed. It is possible to examine the igneous rocks of the earth under the different radiations, and then compare them with the pictures of celestial objects obtained at the same wave-lengths. I have found that some rocks, which when illuminated by ultra-violet rays appear darker than others, are lighter than the others in visible light.

Note added October 1911.

[I have had constructed a 16-inch mirror of 26-feet focus which I have coated with nickel, for extending the study of the ultra-violet photography of the moon and planets. This is now being used in combination with a plate of the new ultra-violet glass, 12 cm. square and 1 mm. thick, heavily silvered. The plate was made by Zeiss, and I find that it is quite as transparent as quartz for the rays transmitted by the silver filter. This reflector was mounted on the 23-inch equatorial of Princeton University, and some very fair pictures have been obtained, though the moon's motion in declination could not be followed with sufficient accuracy to secure the best results. Fig. 7 shows two views of the region around Aristarchus (indicated by an arrow), one made with yellow, the other with ultra-violet light. The dark deposit to the right of the bright crater comes out very clearly in the latter. The markings to the right of this region are quite different in the two pictures. Immediately below the pictures of the moon are three photographs made of two samples of volcanic "tuff" arranged one upon the other, with the crater Aristarchus marked with white chalk (as a check upon the exposure). The left-hand picture was made with yellow light, and the central specimen is lighter than the one surrounding it. The right-hand one was made with ultra-violet, and shows the central specimen distinctly darker. The middle picture was taken with violet light, which shows the two specimens of very nearly the same luminosity. I made an analysis of the fragment of tuff which photographed dark in ultra-violet light, and found that it contained iron and traces of sulphur. Photographs of rocks stained with iron oxide did not show the required peculiarity, and I accordingly attributed the result to the sulphur. A light deposit of sulphur was formed on the surface of a piece of light-grey rock by directing a fine jet of sulphur vapour against it. The deposit was so slight that absolutely no trace of it could be detected by the eye. The specimen was then photographed with yellow, violet, and ultra-violet light, and it was found that the deposit was quite invisible in the first picture, faintly visible in the second, and quite black in the third—precisely the peculiarity shown by the deposit surrounding the crater Aristarchus. Fig. 8, *a*, *b*, *c*, show the gradual appearance of the deposit, which is an oval spot in the centre of the specimen. I feel inclined, therefore, to attribute this spot to an extensive deposit of sulphur, resulting from vapour ejected from the crater. The shape and vast extent of the deposit has always suggested to me that it resulted from material driven out in a volcanic blast.]

Returning now from the moon to the physical laboratory, we will consider a further phenomenon which has been discovered and studied by means of photography in the ultra-violet region. The vapour of mercury has an absorption-band in this region at wave-length 2536, which I have made the subject of a somewhat extended investigation. At low pressures the line is very narrow, resembling one of the D lines of sodium, and I have detected its presence in mercury vapour at room temperature, by employing a tube 3 metres long closed with quartz plates. It occurred to me that this vapour might prove to be the substance which I have long sought for the study of what I have named resonance radiation, i.e. a re-emission of light by absorbing molecules, of precisely the same wave-length as that of the light absorbed. Sodium vapour was found to exhibit the phenomenon, but the experimental difficulties were so great that very little was accomplished. A small box was made of brass and square plates of quartz. The inside was varnished and blackened with soot, a drop of mercury introduced and the box exhausted. The camera with its quartz objective was now trained on the box, and a beam of light from a mercury lamp (quartz) focused at the centre of the box. Though the eye could see no trace of the cone of rays, the photograph brought it out as distinctly as if the box was full of smoke. An exposure of only one second was necessary, and with a ten-second exposure the spectrum of the light scattered by the vapour was secured. It was found to consist of a single line only (the 2536 line), though the light entering the box was the total radiation of the mercury arc, the spectrum of which contained hundreds of lines. The pressure of the mercury vapour was about 0·001 mm., in other words, $\frac{1}{760000}$ of the pressure of the air in the room. It seems most extraordinary that a vapour at such a very low pressure and at the temperature of the room should glow so brilliantly with invisible light. A little further experiment-

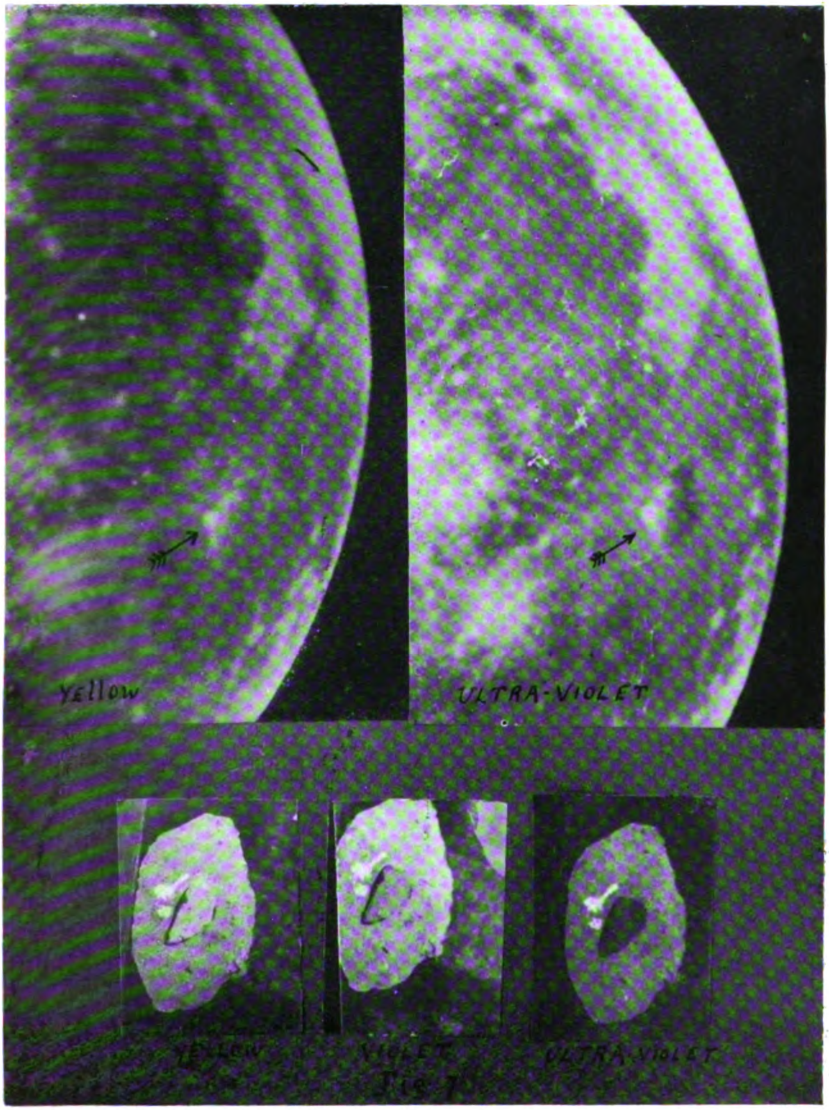
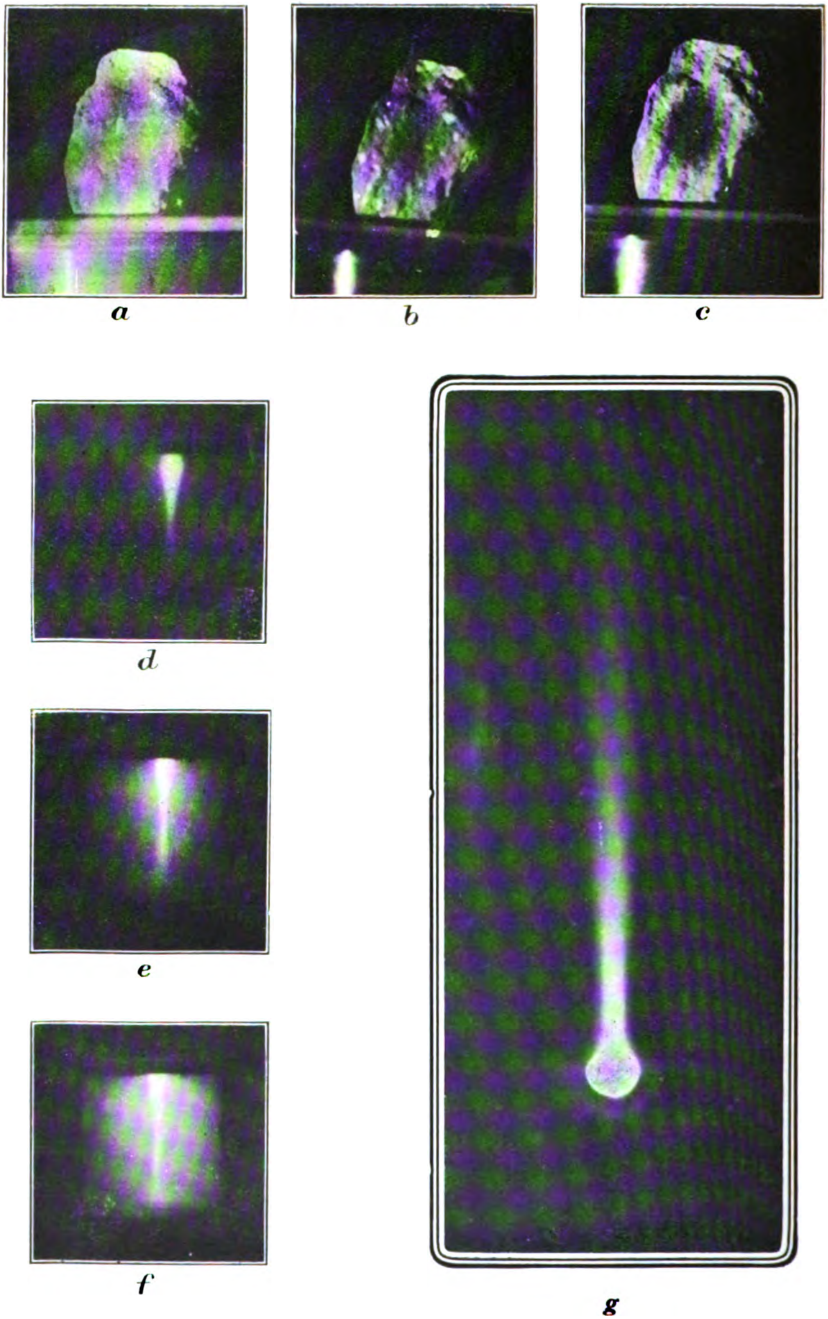


FIG. 7.



a b c d e f g

FIG. 8.

ing resulted in a further discovery. It was found that if the box was filled with air at atmospheric pressure, the cone of rays glowed feebly in the mercury vapour with which the air was saturated. As the pressure was reduced the glow increased in brilliancy, reaching its maximum at a pressure of about 5 mm. As the exhaustion was pushed further the mercury vapour outside of the cone became luminous, and at the highest vacuum attainable the glow filled the entire box. This is secondary resonance radiation excited by the primary radiation of the mercury vapour, which is excited by the cone of focused rays. The brilliancy of the cone remained about the same, so that we cannot attribute the bursting out of this secondary fluorescence to a mere increase in the brilliancy of the directly excited vapour.

Experiments are now in progress to determine why the presence of a few millimetres of air destroys all trace of the secondary radiation. Photographs of the glowing vapour in air at pressures of 5 mm., 1 mm., and 0 are reproduced in Fig. 8, *d, e, f.*

If we put the drop of mercury in a small flask with very thick walls, exhaust the air, and seal the neck of the flask with the oxy-hydrogen flame, we are in a position to study this interesting type of radiation in mercury vapour at high pressures. I found that as the temperature of the flask was raised the radiation came from a region nearer and nearer the front surface which was illuminated by the rays from the lamp, and that when the pressure was about ten atmospheres the ray from the lamp, which had a wave-length of 2536, was selectively reflected from the surface of the vapour, precisely as if the inner surface of the bulb were plated with silver. The other rays passed through the bulb with their usual facility. I am at the present time engaged in the study of just how the change from the resonance radiation (which is scattered in all directions) to the regular reflection takes place, a matter of great interest in connection with the theory of absorption and reflection. As a matter of fact, I expect it to turn out that the mercury light does not absorb the light at all, for experiments indicate that the lateral emission of the ultra-violet light is about as bright as when white paper is used to scatter the light.

Another interesting line of investigation which I have recently carried out illustrates how new discoveries may be made by the aid of ultra-violet photography. It occurred to me that the air surrounding an electric spark might possibly be rendered fluorescent by the absorption of the very short ultra-violet waves discovered by Schumann, but that the fluorescence might be made up wholly of ultra-violet light, and consequently invisible. I therefore photographed the region surrounding a powerful spark discharge with a quartz lens, shielded from the direct light of the spark by a circular disc. The photograph, when developed, showed a highly luminous aureole surrounding the spark and extending out in all directions to a distance of nearly 2 cm. It was now necessary to prove that this was not light scattered by the dust particles in the air. To do this we have only to photograph the spectrum of the aureole. If it is similar to the spectrum of the spark, we are safe in attributing it to scattered light. If it differs we know that it must be fluorescence, or the genesis of waves of different wave-length from any present in the light of the spark. A photograph of the region surrounding the spark was made with a quartz spectrograph, and it was at once found that the spectrum was wholly different from that of the spark—in fact it was almost identical with that of the oxy-hydrogen flame. For the further study of the phenomenon, a piece of apparatus was devised by which the light of the spark could be more effectually shut off. A small hole was bored through a plate of aluminium fastened to the end of a short vertical brass tube. This plate formed one electrode. the spark passing between an aluminium rod lying along the axis of the tube and the under side of the plate at the point perforated by the hole.

In a perfectly dark room, if the eye was held a little below the plane of the plate, no luminosity could be seen in the air above the hole, if it was reasonably free from dust, yet a photograph taken with a quartz lens showed a bright beam, or squirt, of light issuing from the hole. A photograph of the phenomenon is here shown, and you will notice the strong resemblance which it bears to a comet. (Fig. 8, *g*).

Many weeks have been spent in an attempt to determine the exact origin of this radiation, and the question has proved to be the most baffling one which I have ever attempted to solve. The work is still in progress, and many remarkable observations have been made, each one leaving us more in the dark than before. As an illustration I may mention a circumstance discovered by Dr. Hemsalech and myself last winter in Paris. We found that if a jet of air was blown through the squirt of light the luminosity was destroyed in the region traversed by the moving current of air, but was of undiminished intensity both above and below this region. This makes it seem as if the emanation which comes from the spark, and which causes the luminosity of the air, must act for a brief time upon the air in order to cause the luminosity. It also shows that the emanation, whatever may be its nature, is not swept aside by the air current. We have also found that other gases become luminous when subjected to the spark emanations, the spectrum in each case being different and peculiar to the gas used, electrolytic hydrogen, for example, giving a strong luminosity.

It is thus apparent that by employing this "photographic eye" of quartz many new phenomena may be brought to light which have previously hidden themselves behind the limitations of the human eye. A study of the absorption by the candle-flame of ultra-violet has also been made. In this case the light emitted by the candle falls out of the problem, for its flame emits little or no ultra-violet.

I can show you a photograph of the shadow cast by a flame of this description, and you will observe that the shadow is blackest at the point where the flame is brightest, that is, at the point where the minute carbon particles, which, by their incandescence, cause the luminosity, are being set free from the hydrocarbon vapour.

There are other questions which can doubtless be investigated to advantage by means of ultra-violet photography. It is well known, for example, that the amount of ultra-violet light emitted by a body increases with the temperature. By photographing groups of stars through the quartz silver filter, and comparing the photometric intensities of the images obtained in this way with the intensities as shown on a plate made by means of yellow light, valuable data might be obtained. This method is merely an extension of one already in use at the Harvard Observatory.

[R. W. W.]

WEEKLY EVENING MEETING,

Friday, May 26, 1911.

The Right Hon. Sir Henry Buckley, P.C. M.A., Vice-President, in the Chair.

Professor Gilbert Murray, M.A. LL.D. D.Litt. F.B.A.

The Greek Chorus as an Art Form.

"Chorus" = Dance: an ancient Dance was the expression by the whole body of emotions that could not be fully expressed by words: especially therefore of religious emotions, of hopeless regret and the like.

Tragedy originally a Dance at the grave of a dead Hero. Then out of the inarticulate Dance comes element of articulate speech; explanation, appeal. Actors.

A Greek tragedy is carried on, as it were, on two planes: below, the individual actors representing definite persons expressing themselves in speech; above, the ultimate emotion of the situation, the emotion which no individual actor can properly express and which perhaps cannot ever be fully expressed by definite words, is left to the impersonal Dance. E.g., suppose the death of Nelson as a theme. There is an essential emotion about the situation which neither Nelson himself nor any one of his associates can properly express: a modern poet would probably fall back on the Greek model and have some Chorus of Spirits or Ages, like Shelley or Thomas Hardy.

From these two planes or two worlds the poets get certain effects:

1. The two can practically coincide, as where we have supernatural characters: e.g., the *Prometheus*; or where the Chorus forms the chief character, *The Suppliant Women.*

2. There can be a deliberate clash between the two worlds, so that —by a sort of hysterical effect—the calm of the upper world is riven by the suffering of the lower. *Cf.* Castor at end of Euripides' *Electra*; especially the Chorus in the *Medea* when the children are murdered behind the barred door.

3. More usually, the upper and serener world comes in to heal the wounds of the lower, to flood the play with beauty when the mere suffering has become too great. Hippolytus 732*ff*, Trojan Women 799*ff*.

[G. M.]

WEEKLY EVENING MEETING,

Friday, June 2, 1911.

His Grace The Duke of Northumberland, K.G. P.C.
D.C.L. F.R.S., President, in the Chair.

Commendatore G. Marconi, LL.D. D.Sc. *M.R.I.*

Radiotelegraphy.

The practical application of electric waves to the purposes of wireless telegraphic transmission over long distances has continued to extend to a remarkable degree during the last few years, and many of the difficulties, which at the outset appeared almost insurmountable, have been gradually overcome—chiefly through the improved knowledge which we have obtained in regard to the subject generally and to the principles involved.

The experiments which I have been fortunate enough to be able to carry out, on a much larger scale than can be done in ordinary laboratories, have made possible the investigation of phenomena often novel and certainly unexpected.

Although we have—or believe we have—all the data necessary for the satisfactory production and reception of electric waves, we are yet far from possessing any very exact knowledge concerning the conditions governing the transmission of these waves through space—especially over what may be termed long distances. Although it is now perfectly easy to design, construct and operate stations capable of satisfactory commercial working over distances up to 2,500 miles, no really clear explanation has yet been given of many absolutely authenticated facts concerning these waves. Some of these hitherto apparent anomalies I shall mention briefly in passing.

Why is it, that when using short waves the distances covered at night are usually enormously greater than those traversed in the day time, whilst when using much longer waves the range of transmission by day and night is about equal and sometimes even greater by day?

What explanation has been given of the fact that the night distances obtainable in a north-southerly direction are so much greater than those which can be effected in an east-westerly one?

Why is it that mountains and land generally should greatly obstruct the propagation of short waves when sunlight is present, and not during the hours of darkness?

The general principles on which practical radiotelegraphy is based are now so well known, that I need only refer to them in the briefest possible manner.

Wireless telegraphy, which was made possible by the fields of research thrown open by the work of Faraday, Maxwell and Hertz, is operated by electric waves, which are created by alternating currents of very high frequency, induced in suitably placed elevated wires or capacity areas. These waves are received or picked up at a distant station on other elevated conductors tuned to the period of the waves, and the latter are revealed to our senses by means of appropriate detectors.

My original system, as used in 1896, consisted of the arrangement shown diagrammatically in Fig. 1, where an elevated or vertical wire was employed. This wire sometimes terminated in a capacity or was connected to earth through a spark gap.

By using an induction coil or other source of sufficiently high tension electricity sparks were made to jump across the gap; this gave rise to oscillations of high frequency in the elevated conductor and earth, with the result that energy in the form of electric waves was radiated through space.

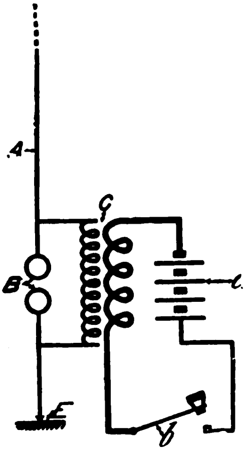


FIG. 1.

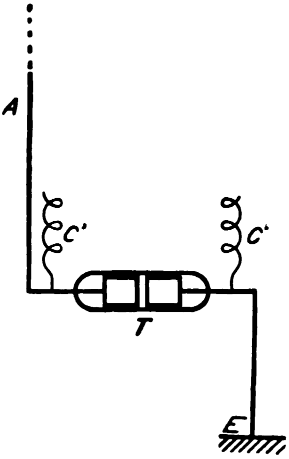


FIG. 2.

At the receiving station (Fig. 2), these waves induced oscillatory currents in a conductor containing a detector, in the form of a coherer, which was usually placed between the elevated conductor and earth.

Although this arrangement was extraordinarily efficient in regard to the radiation of electrical energy, it had numerous drawbacks.

The electrical capacity of the system was very small, with the result that the small amount of energy in the aerial was thrown into space in an exceedingly short period of time. In other words the energy, instead of giving rise to a train of waves, was all dissipated after only a few oscillations, and, consequently, anything approaching good tuning between the transmitter and receiver was found to be unobtainable in practice.

Many mechanical analogies could be quoted which show that in order to obtain syntony the operating energy must be supplied in the form of a sufficient number of small oscillations or impulses properly timed. Acoustics furnish us with numerous examples of this fact—such as the resonance produced by the well-known tuning fork experiment.

Other illustrations of this principle may be given, e.g. if we have to set a heavy pendulum in motion by means of small thrusts or impulses, the latter must be timed to the period of the pendulum, as otherwise its oscillations would not acquire any appreciable amplitude.

In 1900 I first adopted the arrangement which is now in general use, and which consists (as shown in Fig. 3) of the inductive association of the elevated radiating wire with a condenser circuit which may be used to store up a considerable amount of electrical energy and impart it at a slow rate to the radiating wire.

As is now well known, the oscillations in a condenser circuit can be made to persist for what is electrically a long period of time, and it can be arranged moreover that by means of suitable aerials or antennæ these oscillations are radiated into space in the form of a series of waves, which through their cumulative effect are eminently suitable for enabling good tuning and syntony to be obtained between the transmitter and receiver.

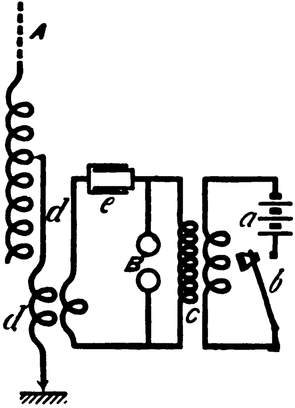


FIG. 3.

The circuits, consisting of the condenser circuit and the elevated aerial or radiating circuit, were more or less closely coupled to each other. By adjusting the inductance in the elevated conductor, and by the employment of the right value of capacity or inductance required in the condenser circuit, the two circuits were brought into electrical resonance, a condition which I first pointed out as being essential in order to obtain efficient radiation and good tuning.

The receiver (as shown in Fig. 4) also consists of an elevated conductor or aerial, connected to earth or capacity through an oscillating transformer. The latter also contains the condenser and detector, the circuits being made to have approximately the same electrical time period as that of the transmitter circuits.

At the long distance station situated at Clifden in Ireland, the arrangement which has given the best results is based substantially upon my syntonic system of 1900, to which have been added numerous improvements.

An important innovation from a practical point of view was the adoption at Clifden and Glace Bay of air condensers, composed of insulated metallic plates suspended in air at ordinary pressure. In this manner we greatly reduce the loss of energy which would take place in consequence of dielectric hysteresis were a glass or solid dielectric employed. A very considerable economy in working also results from the absence of dielectric breakages, for, should the potential be so raised as to even produce a discharge from plate to plate across the condenser, this does not permanently affect the value of the dielectric, as air is self-healing and one of the few commodities which can be replaced at a minimum of cost.

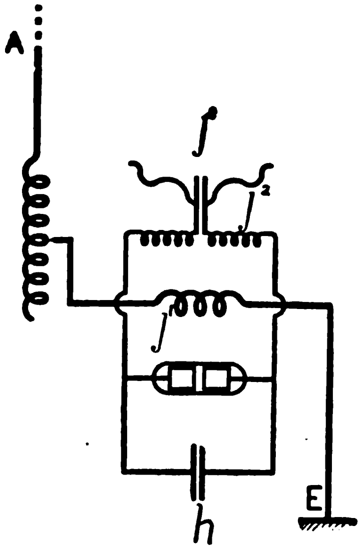


FIG. 4.

Various arrangements have been tried and tested for obtaining continuous or very prolonged trains of waves, but it has been my experience that, when utilising the best receivers at present available, it is neither economical nor efficient to attempt to make the waves too continuous. Much better results are obtained when groups of waves (Fig. 5) are emitted at regular intervals in such manner that their cumulative effect produces a clear musical note in the receiver, which is tuned not only to the periodicity of the electric waves transmitted, but also to their group frequency.

In this manner the receiver may be doubly tuned, with the result that a far greater selectivity can be obtained than by the employment of wave-tuning alone.

In fact, it is quite easy to pick up simultaneously different messages transmitted on the same wave length, but syntonised to different group frequencies.

As far as wave tuning goes, very good results—almost as good as are obtainable by means of continuous oscillations—can be achieved with groups of waves, the decrement of which is in each group ·03 or ·04, which means that about 30 or 40 useful oscillations are radiated before their amplitude has become too small to perceptibly affect the receiver.

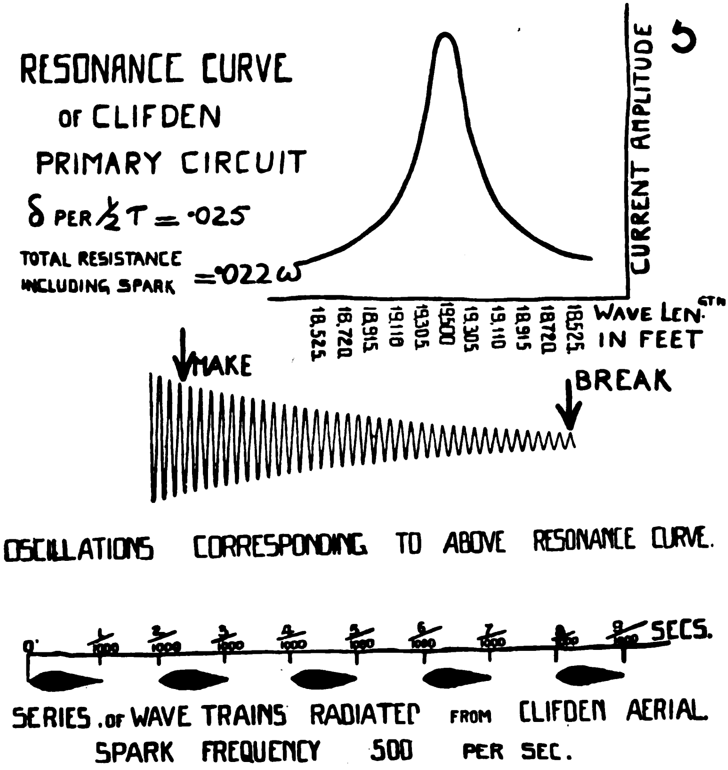


Fig. 5.

The condenser circuit at Clifden has a decrement of from ·015 to ·03 for fairly long waves.

This persistency of the oscillations has been obtained by the employment of the system shown in Fig. 6, which I first described in a patent taken out in September 1907. This method eliminates almost completely the spark gap and its consequent resistance, which, as is well known, is the principal cause of the damping or decay of the waves in the usual transmitting circuit.

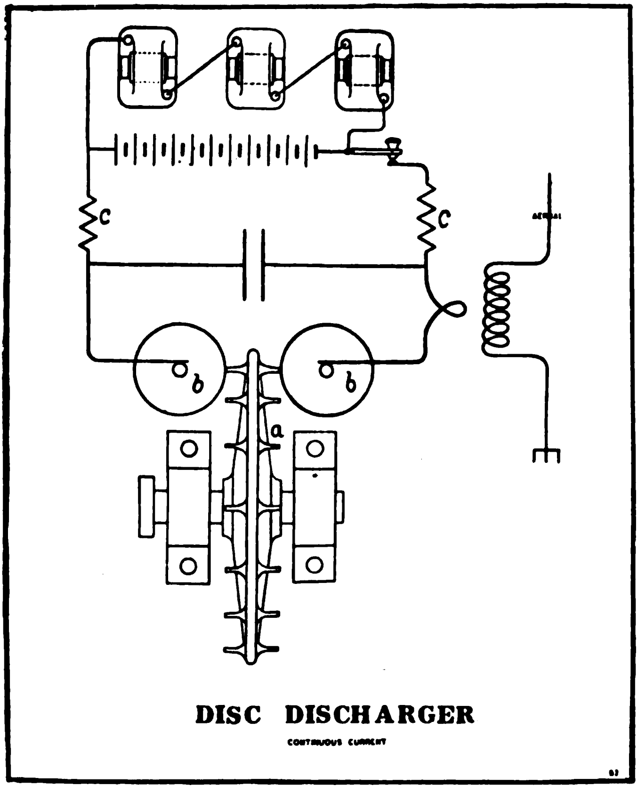


Fig. 6.

The apparatus shown in Fig. 6 consists of a metal disc *a*, having copper studs firmly fixed at regular intervals in its periphery and placed transversely to its plane. This disc is caused to rotate very rapidly between two other discs, *b*, by means of a rapidly revolving electric motor or steam turbine. These side discs are also made to slowly turn round in a plane at right angles to that of the middle disc. The connections are as illustrated in the figure. The studs are of such length as to just touch the side discs in passing, and thereby bridge the gap between the latter.

With the frequency employed at Clifden, namely 45,000, when a potential of 15,000 volts is used on the condenser, the spark gap is practically closed during the time in which one complete oscillation only is taking place, when the peripherical speed of the disc is about 600 feet a second. The result is that the primary circuit can continue oscillating without material loss by resistance in the spark gap. Of course the number of oscillations which can take place is governed by the breadth or thickness of the side discs, the primary circuit being abruptly opened as soon as the studs attached to the middle disc leave the side discs.

This sudden opening of the primary circuit tends to immediately quench any oscillations which may still persist in the condenser circuit; and this fact carries with it a further and not inconsiderable advantage; for, if the coupling of the condenser circuit to the aerial is of a suitable value, the energy of the primary will have practically all passed to the aerial circuit during the period of time in which the primary condenser circuit is closed by the stud filling the gap between the side discs; but, after this, the opening of the gap at the discs prevents the energy returning to the condenser circuit from the aerial, as would happen were the ordinary spark gap employed. In this manner the usual reaction which would take place between the aerial and the condenser circuit can be obviated, with the result that with this type of discharger and with a suitable degree of coupling the energy is radiated from the aerial in the form of a pure wave, the loss from the spark gap resistance being reduced to a minimum.

I am able to show a resonance curve taken at Clifden, which was obtained from the oscillations in the primary alone (Fig. 5).

An interesting feature of the Clifden plant, especially from a practical and engineering point of view, is the regular employment of high tension direct current for charging the condenser. Continuous current at a potential which is capable of being raised to 20,000 volts is obtained by means of special direct-current generators; these machines charge a storage battery consisting of 6000 cells all connected in series, and it may be pointed out that this battery is the largest of its kind in existence. The capacity of each cell is 40 ampere hours. When employing the cells alone the working voltage is from 11,000 to 12,000 volts, and when both the direct-current generators and the battery are used together the potential may be raised to 15,000 volts through utilizing the gassing voltage of the storage cells.

For a considerable portion of the day the storage battery alone is employed, with a result that for 16 hours out of the 24 no running machinery need be used for operating the station, with the single exception of the small motor revolving the disc.

The potential to which the condenser is charged reaches 18,000 volts when that of the battery or generators is 12,000. This potential is obtained in consequence of the rise of potential at the condenser plates, brought about by the rush of current through the choking or inductance coils at each charge. These coils are placed between the battery or generator and the condenser *c*, Fig. 6.

No practical difficulty has been encountered either at Clifden or Glace Bay in regard to the insulation and maintenance of these high-tension storage batteries. Satisfactory insulation has been obtained by dividing the battery into small sets of cells placed on separate stands. These stands are suspended on insulators attached to girders fixed in the ceiling of the battery room. A system of switches, which can all be operated electrically and simultaneously, divides the battery into sections, the potential of each section being low enough to enable the cells to be handled without inconvenience or risk.

The arrangement of aerial adopted at Clifden and Glace Bay is shown in Fig. 7. This system, which is based on the result of tests which I first described before the Royal Society in June 1906,* not only makes it possible to efficiently radiate and receive waves of any desired length, but it also tends to confine the main portion of the radiation to any desired direction. The limitation of transmission to one direction is not very sharply defined, but nevertheless the results obtained are exceedingly useful for practical working.

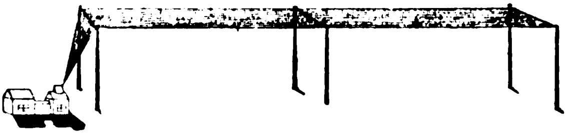


Fig. 7.

In a similar manner, by means of these horizontal wires, it is possible to define the bearing or direction of a sending station, and also limit the receptivity of the receiver to waves arriving from a given direction.

The commercial working of radiotelegraphy and the widespread application of the system on shore and afloat in nearly all parts of the world, has greatly facilitated the marshalling of facts and the observation of effects. Many of these, as I have already stated, still await a satisfactory explanation.

A curious result which I first noticed over nine years ago in long distance tests carried out on the S.S. "Philadelphia," and which still remains an important feature in long distance space telegraphy, is the detrimental effect produced by daylight on the propagation of electric waves over great distances.

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* "On methods whereby the Radiation of Electric Waves may be mainly confined, etc." Proc. Roy. Soc., A. vol. lxxvii., p. 413.

The generally accepted hypothesis of the cause of this absorption of electric waves in sunlight is founded on the belief that the absorption is due to the ionisation of the gaseous molecules of the air affected by the ultra-violet light, and, as the ultra-violet rays which emanate from the sun are largely absorbed in the upper atmosphere of the earth, it is probable that that portion of the earth's atmosphere which is facing the sun will contain more ions or electrons than that which is in darkness, and therefore, as Sir J. J. Thomson has shown,* this illuminated or ionised air will absorb some of the energy of the electric waves.

The wave length of the oscillations employed has much to do with this interesting phenomenon, long waves being subject to the effect of daylight to a very much lesser degree than are short waves.

Although certain physicists thought some years ago that the daylight effect should be more marked on long waves than on short, the reverse has been my experience; indeed, in some transatlantic experiments, in which waves about 8000 metres long were used, the energy received by day at the distant receiving station was usually greater than that obtained at night.

Recent observation, however, reveals the interesting fact that the effects vary greatly with the direction in which transmission is taking place; the results obtained when transmitting in a northerly and southerly direction being often altogether different from those observed in the easterly and westerly one.

Research in regard to the changes in the strength of the received radiations, which are employed for telegraphy across the Atlantic, has been recently greatly facilitated by the use of sensitive galvanometers, by means of which the strength of the received signals can be measured with a fair degree of accuracy.

In regard to moderate power stations such as are employed on ships, and which, in compliance with the International Convention, use wave lengths of 300 and 600 metres, the distance over which communication can be effected during day time is generally about the same, whatever the bearing of the ships to each other or to the land stations—whilst at night interesting and apparently curious results are obtained. Ships, over 1000 miles away, off the south of Spain or round the coast of Italy, can almost always communicate during the hours of darkness with the Post Office stations situated on the coasts of England and Ireland, whilst the same ships, when at a similar distance on the Atlantic to the westward of these islands and on the usual track between England and America, can hardly ever communicate with these shore stations unless by means of specially powerful instruments.

It is also to be noticed that in order to reach ships in the

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* "Philosophical Magazine," Ser. 6, vol. iv., p. 253.

Mediterranean, the electric waves have to pass over a large portion of Europe and, in many cases, over the Alps. Such long stretches of land, especially when including very high mountains, constitute, as is well known, an insurmountable barrier to the propagation of short waves during day time. Although no such obstacles lie between the English and Irish stations and ships in the North Atlantic *en route* for North America, a night transmission of 1000 miles is there of exceptionally rare occurrence. The same effects generally are noticeable when ships are communicating with stations situated on the Atlantic coast of America.

Although high-power stations are now used for communicating across the Atlantic Ocean, and messages can be sent by day as well as by night, there still exist periods of fairly regular daily occurrence during which the strength of the received signals is at a minimum. Thus in the morning and the evening, when, in consequence of the difference in longitude, daylight or darkness extends only part of the way across the ocean, the received signals are at their weakest. It would almost appear as if electric waves, in passing from dark space to illuminated space and *vice versa*, were reflected and refracted in such manner as to be diverted from the normal path.

Later results, however, seem to indicate that it is unlikely that this difficulty would be experienced in telegraphing over equal distances north and south on about the same meridian, as, in this case, the passage from daylight to darkness would occur more rapidly over the whole distance between the two stations.

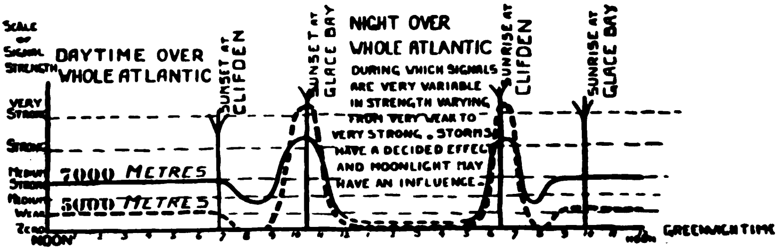

Fig. 8.

I have here some diagrams which have been carefully prepared by Mr. H. J. Round. These show the average daily variation of the signals received at Clifden from Glace Bay.

The curves traced on diagram Fig. 8 show the usual variation in the strength of these transatlantic signals on two wave lengths—one of 7000 metres and the other of 5000 metres.

The strength of the received waves remains as a rule steady during day-time.

Shortly after sunset at Clifden they become gradually weaker, and about two hours later they are at their weakest. They then begin to strengthen again, and reach a very high maximum at about the time of sunset at Glace Bay.

They then gradually return to about normal strength, but through the night they are very variable. Shortly before sunrise at Clifden the signals commence to strengthen steadily, and reach another high maximum shortly after sunrise at Clifden. The received energy then steadily decreases again until it reaches a very marked minimum, a short time before sunrise at Glace Bay. After that the signals gradually come back to normal day strength.

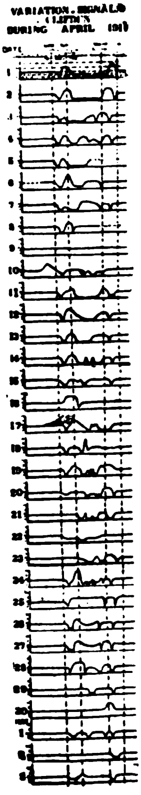

Fig. 9.

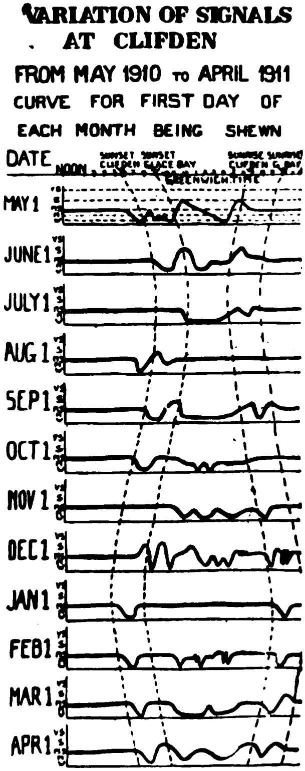

Fig. 10.

It can be noticed that, although the shorter wave gives on the average weaker signals, its maximum and minimum variations of strength very sensibly exceed that of the longer waves.

Fig. 9 shows the variations at Clifden during periods of twenty-four hours, commencing at 12 noon throughout the month of April, 1911, the vertical dotted lines representing sunset and sunrise at Glace Bay and Clifden.

Fig. 10 shows the curve for the first day of each month for one year, from May 1910 to April 1911.

I carried out a series of tests over longer distances than had ever been previously attempted, in September and October of last year, between the stations at Clifden and Glace Bay, and a receiving station placed on the Italian S.S. "Principessa Mafalda," in the course of a voyage from Italy to the Argentine (Fig. 11).

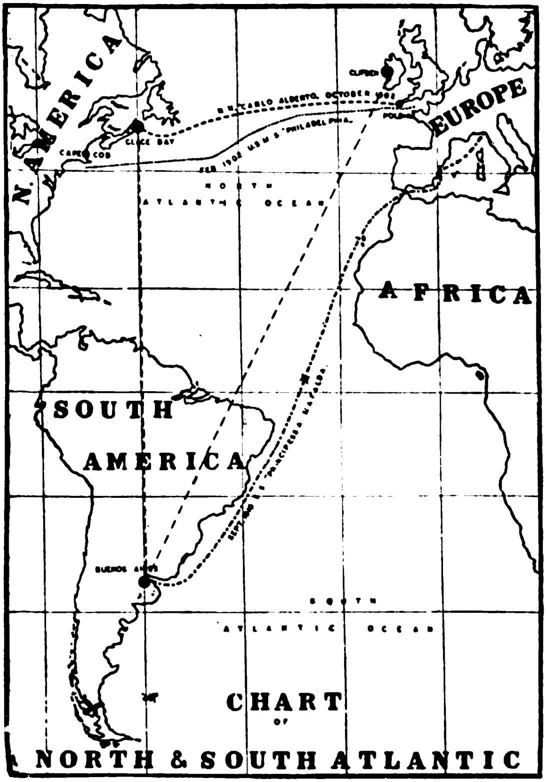

Fig. 11.

During these tests the receiving wire was supported by means of a kite, as was done in my early transatlantic tests of 1901, the height of the kite varying from about 1000 to 3000 feet. Signals and messages were obtained without difficulty, by day as well as by night, up to a distance of 4000 statute miles from Clifden.

Beyond that distance reception could only be carried out during night time. At Buenos Ayres, over 6000 miles from Clifden, the night signals from both Clifden and Glace Bay were generally good, but their strength suffered some variations.

It is rather remarkable that the radiations from Clifden should have been detected at Buenos Ayres so clearly at night time and not at all during the day, whilst in Canada the signals coming from Clifden (2400 miles distant) are no stronger during the night than they are by day.

Further tests have been carried out recently for the Italian Government between a station situated at Massaua in East Africa and Coltano in Italy. Considerable interest attached to these experiments, in view of the fact that the line connecting the two stations passes over exceedingly dry country and across vast stretches of desert, including parts of Abyssinia, the Soudan and the Libyan desert. The distance between the two stations is about 2600 miles.

The wave length of the sending station in Africa was too small to allow of transmission being effected during day time, but the results obtained during the hours of darkness were exceedingly good, the received signals being quite steady and readable.

The improvements introduced at Clifden and Glace Bay have had the result of greatly minimising the interference to which wireless transmission over long distances was particularly exposed in the early days.

The signals arriving at Clifden from Canada are as a rule easily read through any ordinary electrical atmospheric disturbance. This strengthening of the received signals has moreover made possible the use of recording instruments, which not only give a fixed record of the received messages, but are also capable of being operated at a much higher rate of speed than could ever be obtained by means of an operator reading by sound or sight. The record of the signals is obtained by means of photography, in the following manner. A sensitive Einthoven string galvanometer is connected to the magnetic detector or valve receiver, and the deflections of its filament, caused by the incoming signals, are projected and photographically fixed on a sensitive strip, which is moved along at a suitable speed (Fig. 12). On some of these records, which I am able to show, it is interesting to note the characteristic marks and signs produced amongst the signals by natural electric waves or other electrical disturbances of the atmosphere, which, on account of their doubtful origin, have been called "X's."

Although the mathematical theory of electric wave propagation through space was worked out by Clerk Maxwell more than 50 years ago, and notwithstanding all the experimental evidence obtained in laboratories concerning the nature of these waves, yet so far we understand but incompletely the true fundamental principles concerning the manner of propagation of the waves on which wireless telegraph transmission is based. For example, in the early days of wireless telegraphy it was generally believed that the curvature of the earth would constitute an insurmountable obstacle to the transmission of electric waves between widely separated points. For a considerable time not sufficient account was taken of the probable effect of the earth connection, especially in regard to the transmission of oscillations over long distances.

FIG. 12.

Physicists seemed to consider for a long time that wireless telegraphy was solely dependent on the effects of free Hertzian radiation through space, and it was years before the probable effect of the conductivity of the earth was considered and discussed.

Lord Rayleigh, in referring to transatlantic radiotelegraphy, stated in a paper, read before the Royal Society in May 1903, that the results which I had obtained in signalling across the Atlantic suggested "a more decided bending or diffraction of the waves round the protuberant earth than had been expected," and further said that it imparted a great interest to the theoretical problem.* Professor Fleming, in his book on electric wave telegraphy, gives diagrams showing what may be taken to be a diagrammatic representation of the detachment of semi-loops of electric strain from a simple vertical wire (Fig. 13).

As will be seen, these waves do not propagate in the same manner as does free radiation from a classical Hertzian oscillator, but instead glide along the surface of the earth.

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* Proc. Roy. Soc., vol. lxxii., p. 40.

Professor Zenneck* has carefully examined the effect of earthed receiving and transmitting aerials, and has endeavoured to show mathematically that when the lines of electrical force, constituting a wave front, pass along a surface of low specific inductive capacity—such as the earth—they become inclined forward, their lower ends being retarded by the resistance of the conductor, to which they are attached. It therefore would seem that wireless telegraphy as at present practised is, to some extent at least, dependent on the conductivity of the earth, and that the difference in operation across long distances of sea compared to over land is sufficiently explained by the fact that sea water is a much better conductor than is land.

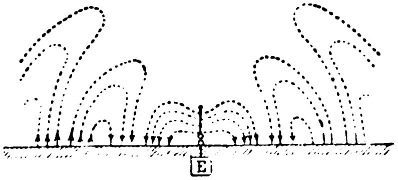

FIG. 13.

The importance or utility of the earth connection has been sometimes questioned, but in my opinion no practical system of wireless telegraphy exists where the instruments are not in some manner connected to earth. By connection to earth I do not necessarily mean an ordinary metallic connection as used for wire telegraphs. The earth wire may have a condenser in series with it, or it may be connected to what is really equivalent, a capacity area placed close to the surface of the ground. It is now perfectly well known that a condenser, if large enough, does not prevent the passage of high-frequency oscillations, and therefore in this case, when a so-called balancing capacity is used, the antenna is for all practical purposes connected to earth.

I am also of opinion that there is absolutely no foundation in the statement which has recently been repeated to the effect that an earth connection is detrimental to good tuning, provided of course that the earth is good.

Certainly, in consequence of its resistance, what electricians call a bad earth will damp out the oscillations, and in that way make tuning difficult; but no such effect is noticed when employing an efficient earth connection.

In conclusion, I believe that I am not any too bold when I say that wireless telegraphy is tending to revolutionise our means of communication from place to place on the earth's surface. For example, commercial messages containing a total of 812,200 words were sent and received between Clifden and Glace Bay from May 1, 1910, to the end of April 1911; wireless telegraphy has already furnished means of communication between ships and the shore where communication was before practically impossible. The fact that a system of Imperial Wireless Telegraphy is to be discussed by the Imperial Conference, now holding its meetings in London, shows the supremely important position which radiotelegraphy over long distances has assumed in the short space of one decade. Its importance from a commercial, naval and military point of view has increased very greatly during the last few years, as a consequence of the innumerable stations which have been erected, or are now in course of construction, on various coasts, in inland regions, and on board ships in all parts of the world. Notwithstanding this multiplicity of stations and their almost constant operation, I can say from practical experience that mutual interference between properly equipped and efficiently tuned instruments has so far been almost entirely absent. Some interference does without doubt take place between ships, in consequence of the fact that the two wave lengths adopted in accordance with the rules laid down by the International Convention, are not sufficient for the proper handling of the very large amount of messages transmitted from the ever increasing number of ships fitted with wireless telegraphy. A considerable advantage would be obtained by the utilisation of a third and longer wave to be employed exclusively for communication over long distances.

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* "Annalen der Physik," xxiii., p. 846, "Physikalische Zeitschrift," 1908, pp. 50, 553.

In regard to the high-power transatlantic stations, the facility with which interference has been prevented has to some extent exceeded my expectations. At the receiving station situated at a distance of only eight miles from the powerful sender at Clifden, during a recent demonstration arranged for the Admiralty, messages could be received from Glace Bay without any interference from Clifden when this latter station was transmitting at full power on a wave length differing only 25 per cent. from the wave radiated from Glace Bay, the ratio between the maximum recorded range of Clifden and 8 miles being in the proportion of 750 to 1.

Arrangements are being made to permanently send and receive simultaneously at these stations, which, when completed, will constitute in effect the duplexing of radiotelegraphic communication between Ireland and Canada.

The result which I have last referred to also goes to show that it would be practicable to operate at one time, on slightly different wave lengths, a great number of long-distance stations situated in England and Ireland without danger of mutual interference.

The extended use of wireless telegraphy is principally dependent on the ease with which a number of stations can be efficiently worked in the vicinity of each other.

Considering that the wave lengths at present in use range from

200 to 23,000 feet, and moreover that wave group tuning and directive systems are now available, it is not difficult to foresee that this comparatively new method of communication is destined to fill a position of the greatest importance in facilitating communication throughout the world.

Apart from long-distance work, the practical value of wireless telegraphy may perhaps be divided into two parts: (1) when used for transmission over sea; (2) when used over land.

Many countries, including Italy, Canada and Spain, have already supplemented their ordinary telegraph systems by wireless telegraphy installations, but some time must pass before this method of communication will be very largely used for inland purposes in Europe generally, owing to the efficient network of landlines already existing which render further means of communication unnecessary; and therefore it is probable that, at any rate for the present, the main use of radiotelegraphy will be confined to extra-European countries, in some of which climatic conditions and other causes absolutely prohibit the efficient maintenance of landline telegraphy. A proof of this has been afforded by the success which has attended the working of the stations recently erected in Brazil on the Upper Amazon.

By the majority of people the most marvellous side of wireless telegraphy is perhaps considered to be its use at sea. Up to the time of its introduction, ships at any appreciable distance from land had no means of getting in touch with the shore throughout the whole duration of their voyage. But those who now make long sea journeys are no longer cut off from the rest of the world; business men can continue to correspond at reasonable rates with their offices in America or Europe; ordinary social messages can be exchanged between passengers and their friends on shore; a daily newspaper is published on board most of the principal liners, giving the chief news of the day. Wireless telegraphy has on more than one occasion proved an invaluable aid to the course of justice—a well known instance of which is the arrest, which took place recently through its agency, of a notorious criminal when about to land in Canada.

The chief benefit, however, of radiotelegraphy lies in the facility which it affords to ships in distress of communicating their plight to neighbouring vessels or coast stations; that it is now considered indispensable for this reason is shown by the fact that several governments have passed a law making a wireless telegraph installation a compulsory part of the equipment of all passenger boats entering their ports.

[G. M.]

WEEKLY EVENING MEETING,

Friday, June 9, 1911.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Professor Svante Arrhenius, D.Sc. Hon.F.R.S.
Hon.F.C.S. *Hon.M.R.I.*,
Director, Nobel Institute of Physical Chemistry.

Applications of Physical Chemistry to the Doctrine of Immunity. Antigenes and Antibodies.

I.—Introduction.

The doctrine of antigenes and antibodies is of very recent date. The first systematic investigations in this field, which has developed so rapidly and become of enormous importance, are due to Behring and Kitasato (1890). It is quite natural that attempts should be made to systematize and explain the vast material collected, in order that there should be a general survey of the field of work. The most prominent fact is that it is generally possible, by injection of different substances of organic origin into the veins of an animal, to obtain from the blood of that animal a serum which neutralizes the action of the injected substance. This is called "antigene"; the active neutralizing body in the blood-serum is called "antibody."

It seems rather natural to compare this neutralization between antigene and antibody with the very familiar neutralization process occurring on mixing an acid with a base. This idea was at first developed by Ehrlich, who was prevented by the great difficulties he encountered from carrying out his conceptions. These difficulties are largely dependent upon the slowness of the reaction of these substances, with the incompleteness of their reactions, and with the instability of the reagents. A great number of investigations regarding these phenomena have been made by Madsen in Copenhagen, and by myself. I will now try to describe the more general features of the results of these and similar studies.

Many of the antibodies are obtained by the injection of red blood-corpuscles into the veins of animals. Some of these antibodies possess the property of causing hemolysis—i.e. red blood-corpuscles of the same kind as the injected ones, under the influence of these antibodies, give up their red colouring matter, the hemoglobin, to the surrounding fluid, which is thereby coloured red· This is easily observed if the blood-corpuscles are allowed to subside for a certain time, generally 12 to 24 hours, in an ice-safe. The strength of the colour may be measured colorimetrically, so that quantitative results are easily obtained. Such antibodies are called hemolysins, and play an important rôle.

Other antibodies obtained in this manner are called agglutinins, because they agglutinate the red blood-corpuscles to aggregates which subside or stick to the walls of the vessel containing the emulsion of blood-corpuscles from animals of the same species as the injected corpuscles. In this case, as in experiments on hemolysis, the blood-corpuscles are separated through centrifugation from the blood-serum, and then suspended in a solution having their own osmotic pressure, in general 0·9 per cent. solution of sodium chloride; otherwise the blood-corpuscles may be attacked by the surrounding fluid itself. For instance, pure water hemolyses red blood-corpuscles very rapidly.

If bacteria are injected, the blood-serum contains substances which may attack the bacteria, so that they slowly disappear—those antibodies are called bacteriolysins—or the antibodies may agglutinate the bacteria, then they are called bacterio-agglutinins. These antibodies, just as the hemolysins, are very specific, so that they do not react with other bacteria except those which were injected. They are therefore used to discriminate bacteria from each other, which is of great use in determining the cause of illnesses. The high degree of specificity indicates a chemical action.

If a fluid antigen, as common egg-white or blood-serum, is injected, the serum of the injected animal contains an antibody called precipitine, because on mixing with egg-white or a serum of the same kind (i.e. taken from an animal of the same kind) as that injected a precipitate is formed. Even this reaction is in a high degree specific.

II.—SPONTANEOUS OR CATALYTIC DESTRUCTION OF ANTIGENES OR ANTIBODIES.

A great number of these substances are rapidly destroyed at high temperatures (over 50° C.). In order to find the rate of destruction, one determines the relative quantities which are necessary to add to the reacting substance, e.g. a given quantity of a standard emulsion of red blood-corpuscles or bacteria, or a given quantity of an albuminous substance, in order that the reaction—hemolysis, agglutination, precipitation—shall be of the same order of magnitude. Madsen and his pupils have performed a great number of such experiments. It was found in all cases that the laws of physical chemistry govern this phenomenon. As an instance, I give the figures for the

SPONTANEOUS DESTRUCTION OF DISSOLVED TETANOLYSIN AT 49·8° C.

t (minutes)	c, observed	c, calculated
0	100·0	100·0
20	80·0	80·6
40	61·1	64·8
60	52·1	52·3
80	46·3	42·1
120	26·8	26·7
180	14·6	14·3

Tetanolysin is a very strong poison secreted by tetanus-bacilli, whereby they cause lock-jaw (tetanus). The close agreement between the calculated and the observed figures of its strength c indicates that the simple law, which says that 50 per cent. of the poison is destroyed in about 64 minutes, 75 per cent. in double the time, 128 minutes, and 87·5 per cent. in the threefold time, 192 minutes, is very nearly exact. That is the law of monomolecular reactions, when every reacting molecule—here every molecule of the poison—is decomposed independently of every other molecule in the solution, except perhaps of the water itself. It is necessary to keep the temperature very accurately constant, because it has an extremely great influence on the rate of decomposition. Thus, for instance, this rate is 16·7 times greater at 53·5° than at 49·8°, i.e. the tetanolysin loses its half strength in a little less than 4 minutes at 53·5°. An increase of as little as 0·1° C. increases the rate of decomposition by 8 per cent. An increase of 1° accelerates the decomposition 2·14 times.

In general, the stability of these bacterial poisons is largely influenced by temperature.

As Madsen has remarked, this property may be useful for the animal body, which generally reacts against these poisons by an increased temperature, i.e. fever.

An addition of alkali or acid mostly accelerates the process in a high degree. Many of these preparations contain a little alkali in their original state. Then an addition of acid increases their stability. In such cases even diluted solutions may be more stable than more concentrated ones.

But, in general, these preparations, and even the antigenes, are most stable in the form of dry powder, and are decomposed the more rapidly the higher the dilution. This is the case, for instance, with rennet, as is seen from the following figures of Madsen and Walbum, valid for 46° C. :—

Concentration p.c.	7	5	3	2	1	0·5	0·25	0·125	0·063
Velocity of decomposition	0·0037	0·0049	0·0154	0·0212	0·028	0·032	0·039	0·060	0·073

Dry rennet is very much more stable: it is not decomposed in a sensible degree at temperatures below 100° C.; at 158° C. the velocity of decomposition is only 0·071. The higher stability in higher concentrations probably depends on the formation of compounds with albuminous substances contained in these preparations, which are more stable than the antigen or antibody itself, and which are decomposed at higher dilutions.

Some albuminous substances diminish the stability. Thus Madsen and Walbum found that an addition of Witte's peptone increases the rate of decay of tetanolysin.

As has already been said regarding tetanolysin, the rate of decomposition increases very rapidly with temperature. This increase is subject to the same law as is found for common chemical reactions, namely, that an exponential formula is valid, i.e. the velocity of reaction increases in a certain proportion for every degree. Thus Madsen and Famulener found for a hemolysin from goat-serum :—

Temperature, °C. . . .	51	51·5	52	52·5	53
Velocity of decomposition, obs. .	0·0139	0·025	0·038	0·060	0·095
„ „ calc..	0·0145	0·024	0·038	0·060	0·095

This law is verified for a great number of substances in their spontaneous decomposition as well as in their decomposition through the presence of foreign substances, such as acids or bases, or in the case of tetanolysin, of Witte's peptone. When foreign substances act catalytically the influence of temperature generally is not so great as in spontaneous decomposition, but nearly of the same order of magnitude as in common catalytic processes—the velocity of reaction increases in the proportion of about 1 : 2 or 1 : 3 if the temperature rises 10° C.

III.—VELOCITY OF REACTION OF ANTIGENES ON OTHER SUBSTANCES.

Many of these reactions are of such a nature that only a certain phase of the process may be accurately determined. Thus, for instance, during the coagulation of milk by the aid of rennet, the moment when the milk contained in a test-tube does not change its surface at a slight inclination of the tube may be rather accurately determined. After that the milk gets more and more viscous, but still no suitable method of determining the times corresponding to these higher degrees of viscosity has been found. Something similar is also true regarding the agglutination. A certain degree of agglutination may be determined with comparative accuracy, but not others. In similar cases the time necessary to bring about just that degree of coagulation or agglutination has been studied, when different quantities (q) of antigene have been used. It has been invariably found that the greater the quantity (q) the less the time (t), so that in most cases the product ($q.t$) was constant. Madsen investigated for instance the digestion of gelatin by means of pepsin by adding to a mixture of a solution of 2 c.c. of a 7 per cent. solution of gelatin and 1 c.cm. of 0·4 per cent.

solution of HCl, 1 c.c. of a solution containing different quantities (q) of pepsin. The mixture was heated for 2 hours to 36·6° C., and then put in an ice-safe, where it soon solidified, if it was not digested. By trying a great number of mixtures containing different quantities (q) he found the least quantity (q_2) necessary to cause just that degree of viscosity, as described above, in two hours. In the same way he determined the quantity (q_4) necessary to give the same degree of fluidity in four hours, and so forth. He found the following values :—

Time, t	2	4	8	14	20	24
Quantity of pepsin, q .	0·47	0·26	0·13	0·07	0·045	0·038
Product, $q.t$ . . .	0·94	1·04	1·04	0·98	0·90	0·91

The product ($q.t$) is not wholly constant, but very nearly so. The law for common monomolecular reactions, where the rate of chemical change is proportional to the quantity of the acting substance, is fulfilled. In a later experiment Madsen maintained the quantity (q) constant, and determined the time necessary for the given degree of digestion at a given temperature. This time is evidently inversely proportional to the velocity of reaction. He found the same regularity in this case as for the change of other chemical reactions with temperature (cf. above).

Similar regularities were found for the digestion of gelatin by means of trypsine or an antigene, produced by pyocyaneus-bacilli, in the coagulation of milk by means of rennet, in the coagulation of blood-plasma, etc. It is sometimes found that the velocity of reaction has a maximum at a certain temperature, which is explained by the fact that the spontaneous decomposition of the antigene increases very rapidly with rising temperature and much more rapidly than the reaction investigated. At high temperatures, therefore, the time during which the antigene, e.g. rennet, really has opportunity to react, is practically zero, and the observed reaction extremely small. From this circumstance the occurrence of a maximum is easily understood. In the coagulation of milk by means of rennet, Fuld observed such a maximum at 43° C. In reactions of living cells on other substances such maxima are generally found at about 40° C., at which temperature the cells slowly lose their vital activity by the coagulation of their protoplasmatic content. Such maxima are often found in this department of science; they depend on the concurrence of two simultaneous processes which act in opposite directions. In many of these processes, for instance the coagulation of milk or of blood-plasma, the presence of certain salts, in most cases salts of calcium (salts of barium or strontium act in a similar manner), play an important rôle. Reichel and Spiro found for the coagulation of milk that the necessary time is inversely proportional to the concentration of calcium-ions in the solution.

IV.—THE PEPTIC DIGESTION, SCHÜTZ'S RULE.

The most interesting reaction of this kind is the peptic digestion of albuminous substances. E. Schütz found that the quantity of pepton formed during a given time is proportional to the square root of the adopted quantity of pepsin. Later investigations by Sjöquist, Henri, and Bayliss proved that with a constant quantity of pepsin the digested quantity (i.e. the quantity of pepton) is proportional to the square root of the time of digestion; or, generally, that the digested quantity is proportional to the square root of the product of quantity of pepsin used and time of digestion. This rule is called Schütz's rule, after its discoverer.

It was believed for a long time that this rule was peculiar to the action of organic ferments; it is also found in the action of trypsin on albuminous substances, and of lipases from the pancreatic juice, or from castor beans, on fats, as well as for the peptic or tryptic digestion of coagulated albuminous substances, and for the action of proteolytic ferments, e.g. from malt. It had not been found for common chemical processes. Then I proved, theoretically as well as experimentally, that the same rule holds good for the saponification of esters with great excess of ammonia. The peculiarity depends upon the formation of ammonium salts in the process itself. According to the law of mass-action, the acting number of hydroxyl-ions is lowered in a high degree through the presence of the ammonium-ions of the ammonium-salt formed, so that after a short time the number of acting hydroxyl-ions is very nearly inversely proportional to the quantity of salt present. In this case Schütz's rule holds good as indicated by a mathematical analysis of the process, and experiments verified this conclusion. But the calculation indicates that this rule should not be applicable further than until about 50 per cent. of the ammonia is consumed, and it is easy to deduce the general law for the whole process. The same was found to be the case for the action of the examined ferments of organic origin. Thereby it was proved that these phenomena depend upon an interaction between the products of decomposition and one of the reacting substances of such a nature that the really active part of that substance is inversely proportional to the quantity of reaction-products formed. The antigenes or ferments have also in this case their analogies in general chemistry.

The mathematical analysis of these cases shows that the decomposed quantity is only dependent on the magnitude of the product $q.t$, where q is the quantity of ferment used and t is its time of action. These processes are therefore monomolecular.

Cases have been observed in which the reaction-products do not exert this influence upon the reacting substances. The reacting substances were in this case of a relatively low molecular weight, but in their properties were similar to albuminous substances or to fats, namely, glycyl-glycin and triacetin. In the digestion of glycyl-glycin with crepsin, an extract from the stomach, Euler found that the general law for undisturbed monomolecular reactions is valid. The same was found by A. E. Taylor for the saponification of triacetin by means of extract from castor beans. This corresponds to the saponification of an ester by means of sodium hydrate in great excess. This circumstance seems to indicate that the reaction-products, in the cases for which Schütz's rule is valid, act on the albuminous substances or fats and not on the ferments.

V.—PROCESSES IN HETEROGENEOUS SYSTEMS.

In processes in heterogeneous systems the same laws are found to be applicable. But a certain part of the acting substance, for instance, a hemolysin or a bacterio-agglutinin, is necessary to induce any action at all, still more if a certain easily measurable action—in these cases a hemolysis of e.g. 30 per cent. of the blood-corpuscles, or the very accurately observable degree of agglutination—would be obtained in, say, 1 hour at 37° C. In such cases the reaction does not, generally, proceed very much further with time. Therefore the rule, that the product *q.t* of reacting substance and time of reaction regulates the phenomenon, does not hold good for such low concentrations as those just spoken of, but only when the reacting substance (i.e. the hemolysin or bacterio-agglutinin) is present in a considerably greater quantity. In such cases a correction is made for the quantity just necessary to produce a sensible effect. This rule has been verified by Madsen and myself for the hemolytic action of ammonia; by Madsen and Henderson Smith for the hemolytic action of tetanolysin; and by Madsen for the agglutinating effect of Coli-agglutinin, which agglutinates Bacillus Coli. The following figures for hemolysis by means of ammonia may serve as an instance. The figures give the time in minutes necessary for obtaining a certain degree of hemolysis, i.e. percentage of the emulsionated blood-corpuscles hemolysed at 37° C. The process was carried to an end by rapidly cooling the test-tubes to 0° and centrifuging the red blood-corpuscles at this low temperature.

Degree of hemolysis in per cent.		3	10	20	30	40
Attenuation of the ammonia $1/q$:	1	13 (13)	26 (26)	35 (35)	44 (44)	53 (53)
Ditto	0·44	6 (5·7)	10 (11·5)	15 (15·4)	18 (19·4)	23 (23·3)
Ditto	0·23	..	5·5 (6·0)	9 (8·0)	12 (10·1)	14 (12·2)
Ditto	0·133	..	..	4 (4·7)	6·2 (5·9)	8 (7·1)

The figures calculated according to the *q.t* rule are written in brackets at the side of the observed ones. The agreement is quite satisfactory having regard to the magnitude of the errors of observation. Even acids act as hemolysins.

As is well known, water itself acts as a weak base and acid, and therefore acts as a hemolysin. Gros observed that red blood-corpuscles, emulsionated in an isotonic cane-sugar solution, are completely hemolysed in 18·8 minutes at 59° C., in 45·5 minutes at 56° C. This corresponds to an increase in the velocity of reaction in the proportion of 1 to 19 in an interval of 10° C. Madsen determined the increase of the velocity of reaction in the hemolysis by means of acids, bases, and hemolysins of bacterial origin, such as streptolysin and vibriolysin. He observed an increase in the proportion of about 1 to 4 in an interval of 10° C. For some snake-poisons, e.g. those from *Naja tripudians* or *Ancistrodon* (water-moccasin) he observed maxima of action at a certain temperature. The cause of these maxima is still unexplained.

Strong acids react much more rapidly than weak acids; but if small doses are used, so that a considerable time (e.g. 12 hours) was necessary for the reaction, the strong acids (e.g. HCl) acted only about 1·25 times stronger than very weak ones (e.g. acetic acid) used in equivalent quantities. This indicates that a real chemical union takes place, which causes the hemolysis. A similar remark may be made regarding the bases. The action of acids is very much increased if the blood-corpuscles have been treated for some time (20 minutes) with a weak emulsion of lecithin. Oleic acid, and other fatty acids of high molecular weight, act more strongly than other acids.

In these reactions the hemolytic or agglutinating substance is rather rapidly taken up by the red blood-corpuscles or bacteria; but it takes some considerable time—the so-called time of incubation—till hemolysis begins. This indicates that it is a rather slow chemical process which causes the hemolysis or agglutination, after the hemolysing or agglutinating substances are united to substances in the interior of the treated cells.

VI.—PARTITION OF HEMOLYSING OR AGGLUTINATING SUBSTANCES BETWEEN CELLS AND THE SURROUNDING MEDIUM.

If we allow an emulsion of red blood-corpuscles or bacteria to stand for some time with a hemolytic or agglutinating substance dissolved in an isotonic solution of e.g. sodium chloride, the substance is absorbed rather rapidly by the emulsionated cells. We may thereafter centrifuge away the cells and examine the remaining fluid as to its contents of acting substance. By these means it is possible to determine the rate of partition of the said substance between the cells and the surrounding solution. If we wish to avoid hemolysis, it is necessary to work at a low temperature (near 0°). A shaking up of the cells now and then is also desirable.

In this manner, Madsen and Teruchi found that vibriolysin is divided between red blood-corpuscles and the fluid, so that the concentration of vibriolysin in the cells is about 230 times greater than in the fluid. This was proved by treating different quantities of cells from 0·05 up to 0·6 c.c. with 9·95 resp. 9·4 c.c. of salt-solution, charged with a certain quantity of the poison. In the first case (0·05 c.c.), the cells took away 56 per cent. of the poison; when the cells were eight times as many, 90 per cent. The two determinations give ratios of partition of 254:1 and 227:1, which may be regarded as equal, if we consider the great errors of observation.

I observed the same property in a more indirect way, namely, by means of the observed degree of hemolysis. If this is constant, the red blood-corpuscles are in the same state, independently of their number, and the surrounding solution has the same concentration of the poison. If, then, I investigate different quantities of red blood-corpuscles in the same quantity of isotonic salt-solution, and add just so much poison that a certain fraction—say 40 per cent.—of the blood-corpuscles is hemolyzed, then the same quantity of poison is in the fluid, and the blood-corpuscles have taken up a quantity of poison proportional to their number. By changing this number, it is possible to determine how great a part of the poison remains in the fluid, and how much is taken up by each c.c. of the suspended blood-corpuscles. Of course, it is necessary always to use the same temperature—I had a bath of 37° C.—and time of reaction—I used two hours—in the same series of experiments. In this manner I found that for saponin the concentration in the blood-corpuscles was 120 times greater than in the solution. For ammonia, caustic soda, and acetic acid, I found values varying between about 600 and 900 times. I have found similar high values for silver salts and mercuric chloride, which are also hemolysing substances.

This strong absorption of the blood-poisons in blood-corpuscles indicates that they are probably chemically bound to some substances in the cells. The absorption proceeds, according to Madsen's and Teruchi's experiments, in agreement with common physico-chemical laws, i.e. in proportion to the excess of the concentration in the fluid over that concentration which would be in equilibrium with the poison absorbed in the cell at the time observed.

Those antibodies, which have been examined hitherto, behave in a somewhat different manner. Eisenberg and Volk allowed certain kinds of bacilli, viz. typhoid-bacilli or cholera vibrions, to be emulsified for a certain time with solutions of different quantities of the corresponding agglutinins, dissolved in a given quantity of 0·9 per cent. solution of sodium chloride. The bacillis slowly subsided, and the supernatant fluid was decanted and its content of agglutinin determined. It was then found that the concentration in the bacilli increased more slowly than the concentration in the fluid, and proportional to the power two-thirds of this concentration. This circumstance indicates that the molecular weight of the agglutinin in the bacilli is only two-thirds of its molecular weight in the surrounding fluid. If the agglutinin has entered as a chemical compound in the bacilli, then the quantity of agglutinin in each molecule of this compound is only two-thirds of the quantity of agglutinin in each agglutinin-holding molecule in the fluid.

As is said above, after injection of red blood-corpuscles of one animal into the veins of another animal, we find in the blood-serum of the second animal a substance which hemolyses the red blood-corpuscles of the first animal. This hemolysin loses its hemolytic properties if it is heated for some time to 56° C. But it still contains a substance, called immune-body, characterized by the fact that it gives a hemolysin specific to red blood-corpuscles of the first animal if it is mixed with some innocuous serum, e.g. serum from guinea-pigs. By means of this property it is possible to determine the quantity of immune-body contained in a certain volume of a fluid. The absorption of immune-body in the red blood-corpuscles of the first animal has been investigated by Morgenroth and myself. In this respect it behaves exactly as the agglutinins.

As instances the following measurements of Eisenberg and Volk regarding cholera-vibrions, and of Morgenroth and myself regarding red blood-corpuscles from an ox may serve. The absorbed substances were in the first case agglutinin against cholera vibrions, in the second immune body contained in serum of a rabbit, treated with red blood-corpuscles from an ox. T is the total quantity of one of these substances in an arbitrary unit, B that part of it which remained in the fluid, C the absorbed quantity. The following equations are used for the calculations:—

$$B + L = T; \qquad L = K B^{2/3}.$$

Absorption of Cholera-agglutinin					Absorption of Immune Body from Rabbit-serum				
T	C	B obs.	B calc.	K	T	C	B obs.	B calc.	K=18·3
2	2	0	0·03	..	250	226	27	39	
20	20	0	1·0	..	330	275	55	57	
40	38	2	2·8	24	670	500	170	151	
67	60	7	6	16·4	1,330	850	480	376	
200	120	80?	27	..	2,700	1,710	990	942	
2,000	1,300	700	620	16·5	5,000	3,070	1,930	2,050	
11,000	6,500	4,500	5,200	23·9	10,000	5,800	4,200	4,800	
20,000	10,000	10,000	10,750	21·5	16,700	7,800	8,800	8,870	
					33,000	13,900	19,100	19,700	
			Mean	19					

The agreement between the observed and the calculated figures may be regarded as quite satisfactory. The change of the values of

B, the observed quantity, are extremely great (in the proportion of 1 to 5000 in the one case and 1 to 800 in the other), and the errors of observation are relatively important. Under such circumstances it may be regarded as proved that the equation given above represents the phenomenon with a high degree of accuracy.

VII.—INFLUENCE OF FOREIGN SUBSTANCES. SENSIBILISATORS.

In many cases it has been found that the presence of certain salts is necessary, or in some cases favourable, for the reactions under consideration. The necessity of certain salts (especially salts of Ca, Ba or Sr) for the precipitation of casein from milk by means of rennet has already been alluded to. In this case probably a chemical reaction takes place. The coagulation of blood-plasma behaves in the same manner, salts of calcium and analogous metals playing the same rôle as in the precipitation of casein from milk.

In other cases, as in the agglutination of bacilli, the salts seem to exert a similar influence as in the sedimentation of particles suspended in a fluid. The trivalent positive ions, such as Al, act more powerfully than divalent ions, such as Zn, Mg, Cd, and these latter greatly exceed the monovalent ions, such as K or Na. The H-ion has a very strong action. The bases seem to hold back the agglutination just as in the sedimentation of suspensions. These regularities are not very well marked in all cases.

Other substances, especially albuminoids, such as serum from blood or gelatin, diminish the agglutination. The agglutinating cells are probably coated with a thin skin of these albuminoids, whereby their properties become changed.

In hemolysis by means of different bases, such as KOH, NaOH, or LiOH, the presence of their neutral salts has a diminishing action. Equivalent quantities seem to exert the same influence. Madsen and Walbum found that about double the quantity of acid or of base is necessary for complete hemolysis if the red blood-corpuscles are suspended in a salt solution, as when they are suspended in an isotonic solution of cane-sugar. Ammonium-salts have a very strong retarding effect on hemolysis by means of ammonia, acetates on the hemolytic action of acetic acid. Probably the velocity of reaction here plays a rôle—under such circumstances this action is easily explained.

Immune bodies, alexins and agglutinins are, according to experiments of Bordet and Gay, absorbed in a higher degree in red blood-corpuscles if these are suspended in a salt-solution than if they are suspended in blood-serum. The anti-poisonous action of different natural sera may probably be ascribed to this peculiarity. Here evidently a chemical union of the albuminous substances in the serum plays the chief rôle.

In many cases lecithin has a very strong action. If red blood-corpuscles have been in contact for half an hour with an isotonic salt-solution containing only about 3 per cent. of an emulsion with 0·1 per cent. lecithin, they are hemolysed to the same degree by a certain quantity of an acid, as by the threefold quantity of the same acid, if they have not been treated with lecithin. Probably this action of the lecithin is connected with the double action, one coagulating, and one hemolytic, of the acids. When the coagulation has reached a certain stage it hinders the hemolysis. On the other hand, treatment with lecithin seems to hold back the coagulation. This gives the explanation of its favourable influence on the hemolytic action of acids. The hemolytic action of bases, which do not coagulate the red blood-corpuscles, is not increased by means of lecithin. Mercuric chloride which possesses also a strong coagulating action acts more strongly hemolytic on blood-corpuscles if they have been treated with lecithin than on common blood-corpuscles. Lecithin has an extremely great influence on the hemolytic action of cobra-poison. It was believed for a long time that this action was due to the formation of a very strongly hemolytic compound of cobra-poison and lecithin, called cobra-lecithid. Through quantitative experiments I showed that neither the cobra-poison nor the lecithin is consumed on the formation of the hypothetical compound, whereby its existence was made very doubtful. Later investigations seem also to indicate that this compound does not exist. The action of lecithin is therefore that of a "sensibilisator" in this case, a theory first enunciated by Bordet in 1898.

There is a case in which lecithin exerts an opposite influence, namely, the hemolysis by means of saponin. Substances which dissolve fats, for instance, alcohols and ether, exert also a weak diminishing influence on the hemolytic action of saponin, but a weak increasing influence on the hemolysis of red blood-corpuscles, treated with lecithin, by means of cobra-poison.

VIII.—NEUTRALIZATION OF TOXINS BY MEANS OF ANTITOXINS.

We now come back to the central problem in immuno-chemistry, namely, that regarding the action of antitoxins on toxins. It has, as stated above, been found by Ehrlich that if a certain addition of diphtheria antitoxin neutralizes 50 per cent. of a given diphtheria poison, then the double quantity is not able to completely neutralize the poison, and even an addition of the three- or four-fold quantity does not make the poison innocuous. This behaviour agrees wholly with the phenomena observed on neutralization of a weak acid, such as boric acid, with a weak base, such as ammonia. This is seen by a comparison of the two following tables, of which the one relates to the neutralization of tetanolysin by means of its antilysin, the other gives the quantity of free ammonia in a mixture of this base with boric acid. In this case the quantity of free ammonia was determined by its hemolytic activity; *n* is the number of equivalents of antitoxin, or boric acid, added to one equivalent of toxin or ammonia. T gives the quantity of free toxin, or ammonia, in per cent. T calc. is the corresponding quantity calculated according to the law of Guldberg and Waage. The temperature was 37° C.

Neutralization of Tetanolysin			Neutralization of Ammonia		
n	T obs.	T calc.	*n*	T obs.	T calc.
0	100	100	0	100	100
0·36	70	66	0·167	85	79
0·72	36	38	0·333	69	64
1·09	22	23	0·667	43	42
1·45	14·2	13·9	1·00	25	27
2·54	6·1	6·3	1·333	20	18
4·35	2·7	2·9	1·667	13	13
6·52	1·8	1·9	2·00	10	10

The formula of Guldberg and Waage is, in this case:—

(Conc. of free lysin) (conc. of free antilysin)
= K (conc. of combined lysin)^2.

The constant K of the equation for the tetanolysin is K = 0·115, for the ammonia with boric acid it is 1·02. (Subsequent measurements on the conductivity of borate of ammonia, carried out by Lundén at 37° C., gave K = 1·04 in good accordance with the figure deduced from Madsen's and my experiments.)

The agreement between the observed and calculated figures is wholly within the errors of observation. The less the constant K, the stronger is the compound. For strong acids with strong bases the constant K is very small, nearly equal to zero. Even by the neutralization of toxins the constant K, in some cases, has a very small value.

With the diphtheria-toxin the constant is 0·012, i.e. about ten times less than with the tetanolysin, but still the partial decomposition of its compound with its antibody is very evident, as the quoted observations of Ehrlich indicate. But for the lysins in snake-poisons the constant K has still lower values, so that the decomposition of its combination with the corresponding antivenin is nearly insensible except in the neighbourhood of the neutralization point. This is evident from the following values for the toxicity of one equivalent cobralysin mixed with *n* equivalents of its antivenin, in its action at 37° C. on blood-corpuscles from the horse.

Neutralization of Cobralysin			Neutralization of Cobralysin		
n	T obs.	T calc.	*n*	T obs.	T calc.
0	100	100	0·5	44	50
0·167	84	83	0·667	33	33
0·25	71	75	0·833	16	16
0·333	65	66	1·00	3·3	0
0·417	56	58			

In these examples the form of Guldberg and Waage's law indicates that one molecule of poison and one molecule of antitoxin give two molecules of the compound toxin-antitoxin. In other cases, especially with the so-called nerve-poisons, e.g. the ricin and the nerve-poisons contained in the snake-venoms, the formula of equilibrium indicates that three molecules of poison combine with three molecules of antitoxin to form two molecules of the compound. This has been regarded as indicating that the hemolysin of the snake-venoms is different from the corresponding nerve-poison, but this conclusion is not necessary; it might be that the compound toxin-antitoxin possesses a molecular weight 4/3 times as high when it enters in, i.e. acts upon the nervous system, as when it enters in the red blood-corpuscles. A similar remark may be made regarding the nerve-poison and agglutinating poison in ricin. There is also another possibility, namely, that the poison as well as the antitoxin, when dissolved in the blood-corpuscles, possess a molecular weight which is only two-thirds of that characteristic of their solution in the nervous substance, and that their compound has a double molecular weight when dissolved in the nerves than when dissolved in the red blood-corpuscles. Many experiments have been made to separate the blood-poison from the nerve-poison, and through the admixture of different substances it has been found possible to change the toxicity in regard to blood in a different proportion from that in regard to nervous substance. It must here be borne in mind that these admixtures may act as sensibilisators in regard to the blood-corpuscles, but not or only in a lesser degree in regard to the nervous substance, which may explain the effect without supposing that the poison is different in the two cases. This case would then be similar to that of mercuric chloride, which acts both as hemolysing and as coagulating red blood-corpuscles, and through the admixture of lecithin to the blood becomes less coagulating, and thereby more strongly hemolysing. Of course, no one would conclude from this fact that mercuric chloride contains two different poisons. The circumstance that in the preparation of antibodies to a certain poison, for instance, ricin, the strength of the antitoxin in regard to blood is generally nearly proportional to its strength in regard to nervous action, and the corresponding property of different preparations of poison tells very much in favour of the opinion that the blood-poison and the nerve-poison are identical as well as their antibodies.

This question is very nearly related to another, namely, the weakening of poisons with time. In this case we very often find that the poison retains its effect of neutralizing antitoxin undiminished, whereas its toxicity weakens. This question has been investigated very accurately for tetanolysin and diphtheria poison, because this property plays a very great part in practice. Thus, for instance, a diphtheria-toxin prepared in the Danish State Serum Institute in February, 1902, slowly lost its toxicity, so that it was only half as great in September, 1903, as when it was freshly prepared. Still its power of neutralizing antitoxin remained wholly intact, and the constant in the equilibrium formula did not change. This peculiar behaviour was explained by Ehrlich in the following manner. The molecule of the toxin is very complicated and contains one "side-chain" (this term is taken from organic chemistry), which has toxic properties—or, as it is said, contains the toxophorous group —whereas another side-chain binds the antitoxin. With time the toxophorous group decomposes, but the other, so-called haptophorous (from Greek, haptein, bind) group, remains unchanged. The product, called syntoxoid, possesses all the properties of the toxin except that it is innocuous. It has, for instance, the very valuable property of provoking the production of antitoxin when injected in the veins of an animal. The toxicity may be weakened artificially without influencing the binding property, and this circumstance has been much used in preparing antitoxins.

A much more simple explanation is to suppose that there is formed some antisensibilisator in the diphtheria-poison; this preparation contains a great many foreign organic substances which may slowly give new compounds. This antisensibilisator diminishes the action of the diphtheria poison on the nerves, but not on the animal organs—it has been supposed the spleen or the bone-marrow —which produce antitoxin. Further, it does not interfere with the chemical reaction of toxin and antitoxin.

In this manner the extremely complicated hypothesis of Ehrlich, who assumed the presence of about ten different toxic and innocuous substances (the latter called toxoïdes or toxones) in diphtheria-poison (and even in other poisons), is absolutely unnecessary as regards the syntoxoïds. Many of these "partial poisons" have been introduced to explain experimental errors, which were, especially at first, unusually great in this field; another set seek to explain the successively diminishing neutralizing power of antitoxin, which depends upon the incompleteness of its chemical reaction with toxin. As a matter of fact, the "side-chain theory" of Ehrlich gives no explanation of the observed phenomena, and has only a very moderate scientific value.

Danysz, a Polish scientist in the Pasteur Institute in Paris, observed a peculiar phenomenon—which has been called, after him, the Danysz-effect—in the neutralization of ricin or diphtheria poison by their specific antitoxins. He divided the poison in two parts, and in one experiment he mixed a given quantity of antitoxin with both parts simultaneously; in another experiment he added only the one part of the poison to the same quantity of antitoxin, and after some hours (at 37° C.) he added the second part of the poison. After the poison had had sufficient time to react with the antitoxin, the two mixtures were investigated, and it was found that the second mixture was more poisonous than the first. A similar effect was found later on with tetanolysin, staphylolysin, and rennet.

This effect seemed very peculiar; it was, therefore, used by the opponents against the chemical theory of the binding of toxin through antitoxin as an objection against this theory, notwithstanding that they were unable themselves to explain the phenomenon otherwise than by supposing the occurrence in the poisons of some new hypothetical substances, called epitoxonoïds Madsen and Walbum therefore carried out a very great number of experiments regarding the Danysz-effect in regard to tetanolysin, which I treated theoretically afterwards. I found that the Danysz-effect corresponds to the following well-known chemical process.

Suppose we replace the poison with monochloracetic acid, $CH_2ClCOOH$, and the antitoxin by an equivalent quantity of caustic soda. If I mix the whole quantity of the acid with the alkali, I get a neutral solution of sodium-monochloracetate, which remains neutral for a long time even at a high temperature (65° C.).

If I in another experiment divide the acid in two equal parts, and mix only the one with the said quantity of caustic soda, at 65° C., then a neutralization immediately takes place, and there remains a half equivalent of alkali in presence of a half equivalent of sodiummonochloracetate. These two substances give rise to a slow reaction, by which sodium-chloride and sodium-glycolate are formed—

$$NaOH + NaCH_2ClCO_2 = NaCl + NaCH_2OHCO_2$$

and at the end of this reaction the solution is neutral. If I now add the second half equivalent of the monochloracetic acid, the mixture has a strongly acid reaction, whereas if the two half equivalents of acid had been added to the equivalent of caustic soda, the solution would not have been acid at all.

This phenomenon corresponds wholly to the Danysz-effect. It is evident that no effect would have been found if the quantity of alkali present in the first instance had not exceeded the quantity of acid added—i.e. an excess of alkali is necessary, and the effect is proportional to this excess. Quite the same holds good for the Danysz-effect, as I found by calculation. The experiments had been performed so that 1 c.c. of tetanus poison (a highly diluted preparation) was added to different quantities of antitoxin, and after this mixture had been kept at 37° C., during so long a time that no further sensible chemical change took place, it was mixed with 3 c.c. of the tetanus poison. It was found that no effect is observed if the quantity of antitoxin added is less than 0·16 c.c. of the solution used. With higher quantities of antitoxin, the effect is proportional to the used quantity diminished by 0·16 c.c., just as might be expected. The quantity of antitoxin equivalent to 1 c.c. of tetanolysin was by direct experiments found to be 0·18 c.c., which corresponds very well with the 0·16 c.c. calculated from the Danysz-effect, especially if we regard the partial decomposition of the compound toxin-antitoxin.

If we had used trichloracetic acid instead of monochloracetic acid, the "Danysz-effect" would have been found to take place in an interval three times as great as the interval of neutralization, whereas with monochloracetic acid the two intervals are equal. By the action of tetanolysin on its antitoxin, the interval of the Danysz-effect was found to be more than six times as great as that of neutralization. This probably depends on the relatively complicated structure of the lysin-molecule.

There are some other peculiar phenomena which are to a certain extent similar to the Danysz-effect. If we take a quantity of a lysin which is just sufficient to cause the total hemolysis of a given quantity of red blood-corpuscles, and divide this quantity in two equal parts, and add them successively with an interval of some minutes at 37°, to the poison, the hemolysis will not be complete. This depends on the property of the contents of the blood-corpuscles to combine with a quantity of lysin, which exceeds the quantity causing total hemolysis by many, perhaps a hundred, times. The first half of the red blood-corpuscles, therefore, takes away nearly all the poison from the fluid, so that there remains practically no poison at all for the second half of the red blood-corpuscles, which, therefore, remain nearly intact, whereas the first half is totally hemolysed. The result, therefore, is that only a little more than fifty per cent. of the total quantity of blood-corpuscles is hemolysed, whereas if all these corpuscles had been mixed with the lysin simultaneously the hemolysis would have been complete.

This observation was made by Bordet; similar effects are found for the agglutination of bacilli by Joos, and they may be explained in the same manner. There is a small secondary effect, as Morgenroth has shown in these cases. The compound of the poison with the content of the cells is partially decomposed, and therefore a small part of the poison diffuses out into the liquid and attacks the cells which were intact before. But this effect is a very slow one, so that it does not disturb the first result very much.

Quite the opposite effect to that observed by Bordet may be stated. Suppose we have so much tetanolysin that it is just able to give total hemolysis with a certain quantity of red blood-corpuscles. In another vessel we have the same quantity of red blood-corpuscles. Then if we take only the one part of the blood-corpuscles and mix it with the poison, and let it have time to be absorbed, for which a quarter of an hour is sufficient, and then add the second quantity of blood-corpuscles, the hemolysis will be 50 per cent. If, on the other hand, we divide the poison in two equal portions, and add each of them to each part of the blood-corpuscles, then the hemolysis will be much less than 50 per cent., and only about 25 per cent., because the degree of hemolysis of a certain quantity of blood-corpuscles is approximately proportional to the square root of the quantity of poison added. (If this quantity falls below a certain limit, there is no action at all.) In this case, evidently, the hemolytic effect is less if we add the whole quantity of red blood-corpuscles at once to the poison than if we add the two half parts at different times, whereas in the experiment of Bordet the effect was the opposite.

From this it is clear that in hemolytic and similar experiments it is necessary to take care that the poison added does not remain for some time in concentrated solutions acting on only a part of the blood-corpuscles, but it is necessary to shake them up immediately after admixture, so that a homogeneous fluid surrounds them. This is most easily done by adding the emulsion of the blood-corpuscles by means of a syringe in a strong current, and shaking the test-tube containing the mixture immediately after the injection of the blood.

Many of the experimental errors in older experiments are due to the circumstance that this precaution has not been followed.

As has been pointed out repeatedly, the poisons are taken up very rapidly by the red blood-corpuscles, or bacilli, into their interior. There they react rather slowly with the albuminous substances, and thus cause the effects of hemolysis, bacteriolysis, or agglutination, which is probably only a special case of coagulation. Therefore, the equilibrium between toxins and antitoxins finds place chiefly in the interior of the cells, and not in the surrounding fluid. The protecting influence of sera and other albuminous substances probably depends on their entering into combinations with the toxins, and thereby hindering them from diffusing into the cells. Many of the difficulties in explaining the effects of toxins and antitoxins were due to the circumstance that it was generally supposed that they react outside of the cells.

IX.—The Compound Hemolysins.

In former times so-called "transfusion," i.e. injection of animal blood in the veins of the patient, was often used to refresh the blood of patients suffering from different illnesses. This treatment was often found pernicious, because the foreign blood "killed" the human red blood-corpuscles. This noxious action is due to the serum of the animal blood. Only blood from the same species is harmless. Daremberg proved, in 1891, that the "globulicid" action of the foreign blood is lost if it is heated to 50°–60° C.

In the same manner blood-serum is "bactericid," i.e. it kills bacteria (Fodor, 1887). This "bactericid" property of the serum also vanishes after heating to 60° C. (Buchner).

In 1894, Pfeiffer found that the "bactericid" action of an animal's serum is increased in a very high degree if the animal receives injections of a suspension of the bacteria in question. This serum thereafter contains a bacteriolysin specific to the injected bacilli (Pfeiffer used cholera-vibrions). Bordet then proved that these hemo- and bacteriolysins are compound substances, consisting of one part, alexin, which is rapidly decomposed on heating to 55° (see above, VI.), and one part, the immune-body, which resists this high temperature. This immune-body is specific against the injected cells, and is absorbed by them in a very high degree (see above, VI.). It does not alone cause lysis; but after the addition of some alexin (guinea-pig serum is mostly used as alexin), it gives a strong lysis. Red blood-corpuscles, containing the immune-body, are therefore rapidly hemolyzed if they are brought into a serum.

The serum containing the immune-body acts also by agglutinating red blood-corpuscles, against which it is specific. It has been much discussed whether this agglutinin is identical with the immune-body or not, and the same question has been raised regarding the bacteriolysins and accompanying agglutinins. The influence of foreign substances changes the two qualities in different proportions, and therefore it has been concluded that we have to deal with two different substances. As we have seen above, this conclusion is not very certain (see above, VIII.).

Bordet supposed that the immune-bodies act as sensibilizators. Ehrlich and his school held the opinion that a chemical compound of the immune-body and the alexin is formed, which has hemolytic properties. They further thought that this compound is bound to the red blood-corpuscle, which they treat as if it were a big molecule. This latter view seems very improbable; the fact is that the hemolytic molecule is formed within the blood-corpuscle, where it attacks some albuminous substances and causes hemolysis, just in the same manner as do the simple hemolysins (e.g. tetanolysin).

If Bordet is right, the hemolytic action would be proportional to the product of the added quantity of alexin and that of immune-body, or perhaps to some power of these quantities. This is really the case for the action of cobra-poison in presence of lecithin (see above, VII.), and therefore this case may be regarded as due to a sensibilizing action of the lecithin. As a matter of fact, Ehrlich regarded this case as a typical paradigm for the binding of immune-body to alexin. In this special point he was not successful; but regarding the real immune-bodies and alexins his opinion was right. If we take a certain small quantity of alexin and add it to a given quantity of red blood-corpuscles, we do not attain complete hemolysis, even with very large quantities of immune-body. And, vice versâ, if the quantity of immune-body is small, we shall not have complete hemolysis even with very great quantities of alexin. (It may here be said that the guinea-pig serum in itself contains a small part of hemolysin, so that it is necessary to correct for the action of this quantity.) This indicates that a hemolytic compound is formed from the alexin and the immune-body; and if one of these is not present in sufficient quantity, then even the quantity of hemolysin will be insufficient, for it cannot exceed the quantity equivalent to the quantity present of that of the two components which is present in the least quantity.

I have investigated a considerable number of cases, varying the quantity of alexin and of immune-body, and have found that the quantity of hemolysin formed, as compared with the quantities of immune-body and of alexin, is subject to the law of chemical equilibrium. The results of the experiments could always be calculated in this way, so that there is no doubt that the hemolysin is an additional product of the alexin and the immune-body, and that these three substances are in equilibrium with each other. This equilibrium, so far as regards the hemolytic action, takes place in the interior of the blood-corpuscles.

As an instance of how well the law of chemical equilibrium agrees with the experiments regarding the production of hemolysin from immune-body and alexin, I give the following figures representing the degree of hemolysis produced on blood-corpuscles from an ox by a mixture of heated immune-serum (a) from a goat, injected with blood-corpuscles from the ox, and the alexin (b) of normal guinea-pig serum. The quantities a and b are expressed in cubic millimetres of the two sera. The figures given in brackets are calculated from the formula:—

$$(5a - x)(20b - x) = 90x.$$

x is the quantity of hemolysin. The degree of hemolysis is proportional to the square of the quantity of hemolysin. If the calculus gives a higher degree of hemolysis than 100, it corresponds to total hemolysis and is represented in the table by the figure 100, which is the highest possible degree of hemolysis (expressed in per cent.).

With small quantities (a) of immune-body even the tenfold quantity (b) of that quantity of alexin which is sufficient to produce total hemolysis does not give enough hemolysin to yield a hemolysis of more than about 40 per cent. The same is valid for the immune-body. If the quantity of alexin is very small, 0·6, we obtain not more than about 15 per cent. of hemolysis. The added quantity (a) of immune-body may be as great as may be desired.

b	*a* = 10	*a* = 30	*a* = 100	*a* = 300	*a* = 900
60	40 (46)	..	..	..	..
40	37 (45)	..	..	..	..
25	38 (42)	..	..	..	..
15	39 (37)	..	..	..	..
10	38 (33)	71 (84)	98 (100)	100 (100)	..
6	22 (25)	59 (60)	85 (98)	98 (100)	..
4	20 (20)	45 (44)	75 (66)	82 (73)	..
2·5	..	27 (29)	51 (43)	47 (47)	..
1·5	..	15 (18)	25 (25)	22 (28)	24 (29)
1	..	..	15 (17)	15 (19)	18 (21)
0·6	..	..	11 (10)	13 (11)	13 (14)

Another experiment indicates that a similar equilibrium also takes place outside the blood-corpuscles. If we mix a certain quantity of alexin with different quantities of immune-body, and let the mixture remain for half an hour at 37° C., and thereafter let it act upon the corresponding blood-corpuscles, we find a maximum action at a certain medium concentration of the immune-body. This is explained in the following manner. If no immune-body is present the action is zero and it increases therefore, to begin with, with the quantity of immune-body used. The hemolysin formed is dissociated in a rather high degree, and the constituents alexin and immune-body diffuse into the red blood-corpuscles and are absorbed there and form the hemolysin. If the immune-body is present in great excess the alexin is nearly wholly consumed for the formation of hemolysin, therefore it diffuses very slowly and in small quantities into the red blood-corpuscles, so that only insufficient quantities of hemolysin are formed in them. Of course new molecules of hemolysin dissociate and deliver new quantities of alexin, but this decomposition goes on rather slowly. Therefore during the time of action (2 hours at 37° C.), the quantity of hemolysin formed in the blood-corpuscles is insufficient. Evidently there must be a maximum of action when the immune-body and the alexin are present in equivalent quantities, and that is really what has been found experimentally. This phenomenon, which has been called "diversion of the alexin," was found by Neisser and Wechsberg in investigations regarding bacteriolysins. The corresponding phenomenon with hemolysins was eagerly sought for but not found until I made some quantitative experiments with red blood-corpuscles from an ox, immune-serum from an injected rabbit, and guinea-pig-serum as alexin. It was not observed in other combinations investigated by me, and it is not very prominent, so that it was necessary to use quantitative measurements on the degree of hemolysis. The red blood-corpuscles were very carefully washed in 0·9 per cent. sodium-chloride solution, and thereby freed from the accompanying serum. If that had not been the case the simultaneous injection of serum would have produced a formation of a precipitin (see below, X.) which with the serum on the surface of the blood-corpuscles might have produced a precipitate, which has a great power for absorbing alexins, so that thereby the insufficiency of alexin, if a certain quantity of immune-serum was added, might be explained. As a matter of fact, Gay has made a similar objection to the experiments of Neisser and Wechsberg (see below, X.).

As stated above Madsen and Teruchi observed the absorption of streptolysin in red blood-corpuscles at low temperatures (near 0° C.) without interference of hemolysis, notwithstanding that the absorbed quantity was much more than sufficient to produce total hemolysis. If these corpuscles, with their content of streptolysin, were heated to 37° C. for a quarter of an hour, they were totally hemolysed. Of course the lysin exists also at the low temperature, but the velocity of the reaction which causes hemolysis is extremely small at that low temperature and rather great at 37° C. The compound hemolysins behave in the same manner. There is still another circumstance. The immune-body is absorbed in a high degree and very rapidly by the blood-corpuscles. On the other hand, most alexins do not seem to be absorbed in a high degree by blood-corpuscles if immune-body is not present in them to combine with them.—According to experiments by Malvoz, alexin from dog-serum forms an exception. It is absorbed in a high degree by red blood-corpuscles from rabbits, for if they have been treated with dog's serum and then placed in a solution of the right immune-body, they become hemolysed.—Now if we place sheep-corpuscles in a mixture of immune-body from goat's serum and alexin (normal goat-serum), and let them remain in this mixture for some time at a low temperature, no sensible quantity of hemolysin is formed in the blood-corpuscles because of the very low velocity of reaction at that temperature (0 to 3° C.) But a great quantity of immune-body is absorbed. This is apparent from the fact that the blood-corpuscles are not subject to hemolysis if heated to 37° C. for one hour, but are hemolyzed as soon as alexin is added. At 40° the experiment also succeeds, but only partially, if the corpuscles are separated from the mixture within ten minutes. Then a certain degree of hemolysis is observed, which is increased in a high degree after addition of alexin (Ehrlich and Morgenroth). Evidently the whole phenomenon depends only upon the very slow reaction of hemolysin formation at low temperatures and the corresponding small absorption of alexin. Ten minutes at 40° C. correspond to about 400 minutes at 0° C., according to the general change of the velocity of reaction with temperature. The conclusion of Ehrlich and Morgenroth that hemolysin, which is stable at 40°, should not be so at 0°, is extremely improbable.

X.—The Precipitins.

Those organic substances which give a precipitate with some albuminous substances are called precipitins, e.g. rennet, precipitating casein from milk, is called a precipitin. But this name is often reserved for such precipitating substances as are specific and have been prepared by the injection of the albuminous substance in question into the veins of an animal. Thus, for instance, the blood-serum of animals injected with milk contains a precipitin against milk, called lactoserum, which, like rennet, precipitates casein. It has been studied carefully by Bordet and P. T. Müller.

Lactoserum is decomposed at about 70° C. As its compound with casein is partially dissociated, heating to 70° C. for a certain time decomposes the total quantity of the serum, so that the casein is regenerated. The process as regards casein is wholly reversible. Generally speaking, the processes investigated in immuno-chemistry are reversible, but the treated substances are so easily altered, like the lactoserum, that their composition is changed by secondary processes, and it is therefore in most cases impossible to recover them quantitatively.

If we add casein in increasing quantities to a given quantity of lactoserum, the precipitate increases to begin with, but is dissolved again later on. The precipitate, which is obtained by adding increasing quantities of egg-white to a given quantity of its precipitin, according to Eisenberg behaves in the same manner. Evidently we have to deal here with cases of chemical mass-action analogous to those which we obtain when we add increasing quantities of an alkali to different salts of certain metals, such as zinc, lead, aluminium, etc., in which case we also at first obtain a precipitate, which later on becomes dissolved.

A special interest attaches to those precipitins which are obtained through the injection of the blood-serum from one animal into the veins of another. Their behaviour has been investigated quantitatively by Hamburger in Groningen. As an instance, I give the following figures of Hamburger, regarding immune serum from a rabbit injected with serum from a sheep. He mixed 0·4 c.c. of the immune serum with different quantities A (in c.c.) of sheep's serum (diluted with 49 parts of 0·9 per cent. NaCl solution). After a while he determined the quantity P of precipitate by centrifuging it down into a narrow graduated tube. P is measured in units of 0·0004 c.c. This quantity has been calculated by me according to the following formula:—

$$\frac{40\,A - P}{V} \cdot \frac{120 - P}{V} = 250$$

This formula indicates that the precipitate is a compound of the two sera. Every c.c. of the solution of sheep's serum is equivalent to 40 units of the precipitate, and the 0·4 c.c. of the immune serum is equivalent to 120 units of the precipitate. If, therefore, P units of precipitate have been formed from a mixture of A c.c. sheep's serum, i.e. 40 A equivalents, and 0·4 c.c. immune serum, i.e. 120 equivalents, then there remain (40 A – P) equivalents of the sheep's serum, and (120 – P) equivalents of the precipitin. If we divide these quantities by the volumes V, and form the product, as in the formula above, this product ought to be a constant, according to the law of Guldberg and Waage. The results are as follows :—

Quantity A		V^2	Quantity of Precipitate P	
c.c.	Equiv.	$(c.c.)^2$	Obs.	Calc.
0·02	0·8	0·162	1	0·5
0·04	1·6	0·163	2	1·3
0·1	4	0·25	3	3·5
0·15	6	0·302	6	5·3
0·2	8	0·36	7	7·2
0·6	24	1	21	21·5
1	40	1·96	35	34
1·5	60	3·61	39	48
2	80	5·76	60	57
3	120	11·56	67	66
5	200	29·2	64	65
7	280	57·8	58	58
10	400	108·2	49	46
15	600	237	10	19
18	720	338	5	3
20	800	416	2	0
1 +1	40	5·76	28	25
5 c.c.	200	41	57	51
10 H_2O	400	130	41	32

In the three last experiments 1 c.c. of water was added to the mixture, whereby the volume V was increased, and consequently the precipitate diminished. The general agreement between the calculated and the observed quantities of precipitate shows that the remaining discrepancies (e.g. for A = 1·5, 2, and 15 c.c., as well as in the last two figures) may be supposed to be due to the great experimental difficulties inherent in these measurements. The constants 40, 120, and 250 are determined from the experiments.

In this case we observe an increase of the precipitate P when the quantity of added sheep's serum A increases. But a maximum is soon found, when A = 3 c.c., and thereafter the precipitate decreases. The calculation indicates that this circumstance is only due to the increase in volume, that is, to the attenuation, whereby the concentration of the immune serum diminishes without a commensurate increase in the concentration of the sheep's serum.

But, as stated above, cases are not rare in which a real chemical action takes place between the precipitate already formed and the further addition of the antigene, whereby the precipitate becomes dissolved. Hamburger has given an example of this kind. It relates to serum from the horse, and immune serum, containing precipitin, from the calf. The horse serum was attenuated to a fiftieth, and 1 c.c. of that preparation corresponded to three times the used quantity of calf's serum, namely 1 c.c., which is considered equal to 100 ; therefore, 1 c.c. of the diluted horse serum was considered equal to 300 in the formula for calculation. The maximum quantity of precipitate corresponded to 100 units, but in the experiments only 59 units were obtained, because the dilution was rather great. The results of these experiments are contained in the following table :—

Quantity of Horse Serum	Quantity of Precipitate		Quantity of Horse Serum	Quantity of Precipitate	
	Obs.	Calc.		Obs.	Calc.
4	0	0·2	79	51	53·6
8	3	3·9	88·3	55	57·1
15	10	10·3	90	57	57·5
24	17	17·8	100	59	58·9
30	21	23·6	115·4	55	57·4
39	32	29·7	137	50	51·3
45	34	34	167	43	41·3
54	43	40·1	214	25	26·8
60	45	43·9	300	5	5·5
75	52	51·9	500	2	0

Even in this case the observations agree well with the calculation. It must, however, be remembered that we have here four constants to determine experimentally, namely, the number of equivalents (counted in units of precipitate) contained in 1 c.c. of each of the two sera, and further two constants of equilibrium for the two compounds, the precipitate and its soluble compound with the albuminous substance in the horse serum. It is thereby possible to obtain a fairly good agreement, if the errors of observation are not too great.

The immune serum from a rabbit injected with sheep's serum gives a precipitate with the serum not only of sheep, but also of related animals, as goats or cows. In these cases the quantity of precipitate was less; instead of 300 units per c.c. in the sheep's serum, the corresponding figures were, for the goat's serum 212, and for the serum of cows only 90. These figures indicate that sheep and goats are very nearly related, and in a much lesser degree related to cows. This method evidently gives a possibility of determining the relationship between different animals quantitatively. As is well known it has already been used for similar determinations, but only qualitatively.

In the early days of the development of the doctrine of immunity it appeared to be possible to prepare antibodies against antibodies themselves, which are all constituents of immune sera. Thus, for instance, Bashford prepared an antibody against antiricin, the so-called anti-antiricin. The school of Frankfort prepared anti-immune bodies and anti-alexins, and so on. Recent investigations make it probable that this new anti-action is due to a precipitin against the injected immune serum, which precipitin, when mixed with this immune serum, gives a precipitate. As is the case with many precipitates this carries down with itself the large part of the dissolved bodies which occur in small quantities in the serum. It thereby takes away the small quantities of immune-bodies, of alexins, or generally of antibodies contained in the antigenes, i.e. immune sera.

Evidently this is a kind of diversion of alexin. Gay therefore raises the objection against the explanation of Neisser and Wechsberg concerning their experiments of the diversion of alexin by means of immune bodies (against bacilli). Gay is of the opinion that the injected culture of bacilli contains many albuminous substances, which after their injection caused the production of specific precipitins in the immune serum. Gay supposes that it was these precipitins which caused a precipitate with the fluids accompanying the bacteria in Neisser's and Wechsberg's experiments, and that this precipitate "absorbed" the added alexin, so that it is by no means proved that the disappearance of the alexin was due to the presence of great quantities of immune body binding the alexin. Gay was probably led to this criticism because the explanation given by Neisser and Wechsberg is in many details inconsistent with common laws of chemistry. If the bacteria, as in my corresponding experiments with red blood-corpuscles, had been freed from the accompanying albuminous substances by washing carefully, then the diversion of alexin by means of the immune body would have been proved (see above, IX.).

Gay has made similar experiments with red blood-corpuscles (0·05 c.c.) from an ox, but he added half the quantity of ox-serum, freed from alexin by heating to 55°. With these he mixed a given quantity (0·033 c.c.) of alexin (normal serum from rabbits), and varying quantities of serum from a rabbit, immunized with red blood-corpuscles from an ox. Then he added so much isotonic salt-solution that the total volume was 2·6 c.c. He observed a very strong diversion of the alexin, so that the hemolysis was complete if the added quantity of immune serum was between 0·1 and 0·3 c.c. With 1 c.c. of immune serum the hemolysis was weak, and with 2 c.c. or more it was inperceptible.

This method of determining the presence of a precipitate by means of its aptitude to absorb alexin is much more sensitive (about 100 to 1000 times) than the observation of the precipitate itself. It has therefore been used for discriminating the species of animal from which a given blood-spot is derived, instead of the older method of Wassermann and Uhlenhuth, in which the precipitate itself was observed. The new method, which was worked out by Neisser and Sachs, plays a highly important rôle in the method of Wasserman for determining the presence of spirochætes or, better, their excretions in human blood.

As will be seen from what has been said, immuno-chemistry has, during its short time of application, been able to explain the chief empirical facts of immunity, and there is good reason to hope that it will be able to throw light on the whole of that important branch of therapy and to transform it into a rational science.

Physical chemistry itself has thereby gained a great extension of its sphere, not only as regards chemical equilibria, but also as regards the study of velocities of action.

GENERAL MONTHLY MEETING,

Monday, June 12, 1911.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Miss A. Carthew,
Lieut.-Colonel Alfred Western Hatchell Hornsby-Drake,
were elected Members of the Royal Institution.

The Honorary Secretary announced the decease of Her Grace The Duchess Dowager of Northumberland, on May 4, 1911, and the following Resolution passed by the Managers at their Meeting held this day was unanimously adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution in the decease of Her Grace The Duchess Dowager of Northumberland, the widow of Algernon, Fourth Duke of Northumberland, Knight of the Garter, Doctor of Civil Law of the University of Oxford, Fellow of the Royal Society, a distinguished Patron of Science and Learning, and formerly President of the Royal Institution.

Her Grace The Duchess Dowager of Northumberland was elected a Member of the Royal Institution in the year 1847, and was one of the three oldest Members of the Institution, her name having appeared in the list of Members for sixty-four years. Her Grace always took a deep interest in the Scientific Work of the Royal Institution, and in its welfare.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

The Special Thanks of the Members were returned to William Phipson Beale, Esq., K.C. M.P. *M.R.I.*, for his Donation of £25 to the Fund for the Promotion of Experimental Research at Low Temperatures; and to Donald William Charles Hood, Esq., C.V.O. M.D. *M.R.I.*, for his Present of a Portrait of Faraday.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Kodaikanal Observatory Bulletin, No. XXIII. 4to. 1911.
Report on Kodaikanal and Madras Observatories, 1910. 4to. 1911.
Memoirs of Department of Agriculture: Botanical Series, Vol. IV. No. 1. 8vo. 1911.
Agricultural Journal of India, Vol. VI. Part 2. 8vo. 1911.
Board of Scientific Advice, Annual Report, 1909–10. 8vo. 1911.
Geological Survey: Memoirs, Vol. XXXV. Part 4. 8vo. 1911.
Records, Vol. XLI. Part 1. 8vo. 1911.

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Agricultural Society, Royal—Journal, Vol. LXXI. 8vo. 1910.
American Geographical Society—Bulletin, Vol. XLIII. No. 5. 8vo. 1911.
American Philosophical Society—Proceedings, Vol. L. No. 198. 8vo. 1911.
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Astronomical Society, Royal—Monthly Notices, Vol. LXXI. No. 6. 8vo. 1911.
Memoirs, Vol. LX. Part 1. 4to. 1911.
Bankers, Institute of—Journal, Vol. XXXII. No. 6. 8vo. 1911.
Index, Vols. XVI.-XXXI. 8vo. 1911.
Batavia, Royal Magnetical and Meteorological Observatory—Regenwaarnemingen in Nederlandsch-Indie, 1909, Deel I -II. 8vo. 1911.
Belgium, Royal Academy of Sciences—Bulletin, 1911, Nos. 3–4. 8vo.
Berlin, Royal Prussian Academy of Sciences—Sitzsungberichte, 1911, Nos. 1-22. 8vo.
Birmingham Philosophical Society—Report and List of Members, 1910. 8vo. 1911.
Boston Public Library—Fifty-ninth Annual Report, 1910–11. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol XVIII. No. 14. 4to. 1911.
British Astronomical Association—Journal, Vol. XXI. No. 7. 8vo. 1911.
Buenos Aires—Bulletin of Municipal Statistics, Feb. 1911. 4to.
Canada, Department of Mines—Chrysotile Asbestos. By F. Cirkel. 8vo. 1910.
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Carnegie Institution, Mount Wilson Solar Observatory—Contributions, Nos. 51-52. 8vo. 1911.
Chemical Industry, Society of—Journal, Vol. XXX. Nos. 9-10. 8vo. 1911.
Chemical Society—Journal for May, 1911. 8vo.
Proceedings, Vol. XXVII. Nos. 386–388. 8vo. 1911.
Chicago Field Museum of Natural History—Report Series, Vol. IV. No. 1. 8vo. 1911.
Chicago, John Crear Library—Sixteenth Annual Report, 1910. 8vo. 1911.
Cirencester, Royal Agricultural Coll.—Science Bull., No. 2, for 1910. 8vo. 1911.
Cracovie, Imperial Academy of Sciences—Bulletin: 1911, Classe de Philologie, Nos. 1-3; Classe des Sciences, A, Nos. 3–4, B, Nos. 2-4. 8vo. 1911.
Edinburgh, Royal College of Physicians—Laboratory Reports, Vols. X.-XI. 8vo. 1911.
Editors—Aeronautical Journal for April, 1911. 8vo.
Agricultural Economist for May, 1911. 8vo.
American Journal of Science for June, 1911. 8vo.
Athenæum for May, 1911. 4to.
British Dental Journal for May, 1911. 8vo.
Canada, May, 1911. 8vo.
Chemical News for May, 1911. 4to.
Chemist and Druggist for May, 1911. 8vo.
Concrete for June, 1911. 8vo.
Dyer and Calico Printer for May, 1911. 4to.
Electrical Engineer for May, 1911. 4to.
Electrical Engineering for May, 1911. 4to.
Electrical Industries for May, 1911. 4to.
Electrical Review for May, 1911. 4to.
Electrical Times for May, 1911. 4to.
Electricity for May, 1911. 8vo.
Engineer for May, 1911. fol.
Engineer-in-Charge for May, 1911. 8vo.
Engineering for May, 1911. fol.
Horological Journal for May-June, 1911. 8vo.

Editors—continued.
  Illuminating Engineer for June, 1911. 8vo.
  Journal of Physical Chemistry for April, 1911. 8vo.
  Law Journal for May, 1911. 8vo.
  London University Gazette for May, 1911. 4to.
  Marconigraph for May, 1911. 8vo.
  Model Engineer for May, 1911. 8vo.
  Mois Scientifique for May, 1911. 8vo.
  Motor Car Journal for May, 1911. 4to.
  Musical Times for May, 1911. 8vo.
  Nature for May, 1911. 4to.
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  Nuovo Cimento for Feb.-March, 1911. 8vo.
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Flemwell, G., Esq. (the Author)—Alpine Flowers and Gardens. 8vo. 1910.
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Florence, Reale Accademia dei Georgofili—Atti, 1911, Disp. 1. 8vo.
Franklin Institute—Journal, Vol. CLXXI. No. 5 8vo. 1911.
Geographical Society, Royal—Journal, Vol. XXXVII. No. 6. 8vo. 1911.
  Year Book and Record, 1910. 8vo.
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  Verhandelingen van Teylers godgeleend Genootschap, N.S. Dl. 17. 8vo. 1911.
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Johns Hopkins University—Circulars, 1910, Nos. 5-10; 1911, Nos. 1-2. 8vo.
  Studies, Series XXVIII. Nos. 1-4. 8vo. 1910.
Khan, H. Rahman, Esq. (the Author)—Water Wireless Telegraphy. 8vo. 1911.
Koerner, Professor Wilhelm, D.C.L. Hon.F.C.S. Hon.M.R.I. (the Author)—Uber die Bestimmung des Chemischen Ortes bei den Aromatischen Substanzen. 8vo. 1910.
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Life-Boat Institution, Royal National—Annual Report, 1911. 8vo.
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Liversidge, A., Esq., M.A. LL.D. F.R.S. M.R.I. (the Author)—The Minerals of New South Wales. 8vo. 1888.
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Mechanical Engineers, Institution of—List of Members, 1911. 8vo.
Monaco, Institut Océanographique—Bulletin, No. 201. 8vo. 1911.
Montpellier Académie des Sciences—Bulletin, 1911, No. 4. 8vo.
Munich, Royal Bavarian Academy of Sciences—Abhandlungen: Math.-Phys. Klasse, Band XXV. Ab. 5; Suppl. Band II. Ab. 3-4. 4to. 1910.
  Sitzsberichte: Math.-Phys. Klasse, 1910, Ab. 10-16. 8vo.
National Church League—Church Gazette for June, 1911. 8vo.
National Physical Laboratory—Collected Researches, Vol. VII. 4to. 1911.
  Report for 1910. 8vo. 1911.
Navy League—The Navy for June, 1911. 8vo.
Numismatic Society, Royal—Numismatic Chronicle, 1905-10; 1911, Part 1. 8vo. 1911.

Onnes, Prof. Dr. H. K.—Communications from the Physical Laboratory of the University of Leiden, Nos. 117–120. 8vo. 1911.

Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for April, 1911. 4to.

Paris, Société Française de Physique—Journal de Physique for May, 1911. 8vo.

Bulletin, 1910, Fasc. 4–5. 8vo.

Pharmaceutical Society of Great Britain—Journal for May, 1911. 8vo.

Princeton University Observatory—Contributions, No. 1. 4to. 1911.

Pupovac, P., Esq. (the Author)—Meine Friedenszahl Numerus Pacis. 8vo. 1911.

Rome, Ministry of Public Works—Giornale del Genio Civile for March, 1911. 8vo.

Royal Colonial Institute—United Empire, Vol. II. No. 5. 8vo. 1911.

Royal Engineers' Institute—Journal, Vol. XIII. No. 6. 8vo. 1911.

Royal Irish Academy—Proceedings, Vol. XXIX. C, No. 4; Vol. XXXI. (Clare Island Survey) Nos. 4, 5, 22. 8vo. 1911.

Royal Society of Arts—Journal for May, 1911. 8vo.

Royal Society of Edinburgh—Proceedings, Vol. XXXI. Part 3. 8vo. 1911.

Royal Society of London—Philosophical Transactions, A, Vol. CCXI. No. 475–476. 4to. 1911.

Proceedings, A, Vol. LXXXV. No. 577; B, Vol. LXXXIII. No. 566. 8vo. 1911.

St. Paulo, Secretaria da obras Publicas—Dados Clematologicas, 1909, Nos. 12–15. 8vo. 1910.

St. Petersburg, Imperial Academy of Sciences—Bulletin, 1911, Nos. 8–9. 4to.

Sanitary Institute, Royal—Journal, Vol. XXXII. No. 5. 8vo. 1911.

Selborne Society—Selborne Magazine for June, 1911. 8vo.

Smith, B. Leigh, Esq., M.R.I.—Scottish Geographical Magazine, Vol. XXVII. No. 6. 8vo. 1911.

Smithsonian Institution—Harriman Alaska Expedition, Vols. I.–V. and VIII.–XIII. 8vo. 1902–5.

Societa degli Spettroscopisti Italiani—Memorie, Vol. XL. Disp. 4. 4to. 1911.

South Africa, Union of—Agricultural Journal for April, 1911. 8vo.

South African Association for the Advancement of Science—Journal, Vol. VII. No. 7. 8vo. 1911.

Statistical Society, Royal—Journal, Vol. LXXIV. No. 6. 8vo. 1911.

United Service Institution, Royal—Journal for May, 1911. 8vo.

United States Department of Agriculture—Experiment Station Record, Vol. XXIV. Nos. 3–4. 8vo. 1911.

Food Customs and Diet in American Homes. 8vo. 1911.

United States Department of Commerce and Labour—Bulletin of the Bureau of Standards, Vol. VII. Nos. 1–2. 8vo. 1911.

Terrestrial Magnetism Report for 1910. 8vo. 1911.

Directions for Magnetic Measurements. By D. L. Hazard. 8vo. 1911.

Results of Observations at Sitka Magnetic Observatory, 1907–8. 4to. 1911.

United States Patent Office—Official Gazette, Vol. CLXVI. 8vo. 1911.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1911, Heft 5. 4to.

Vienna, Imperial Geological Institute—Abhandlungen, Band XVI. Heft 3; Band XXII. Heft 1. fol. 1910–11.

Warsaw, Academy of Sciences—Comptes Rendus, Vol. IV. No. 3. 8vo. 1911.

Travaux, 1911, No. 3. 8vo.

Western Society of Engineers—Journal, Vol. XVI. Nos. 4–5. 8vo. 1911.

Wilde, Henry, Esq., D.Sc. D.C.L. F.R.S. M.R.I. (the Author) – On the Periodic Times of Saturn's Rings. 8vo. 1911.

Yorkshire Archæological Society—Journal, Vol. XXI. No. 2, Part 82. 8vo. 1910.

Zurich, Naturforschenden Gesellschaft—Vierteljahrsschrift, 1910, Heft 3–4. 8vo. 1911.

GENERAL MONTHLY MEETING,

Monday, July 3, 1911.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Mrs. Tufnell,
Henry G. Turner, Esq., J.P.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Astronomer Royal—Report of H.M. Astronomer at the Cape of Good Hope, 1910. 4to. 1911.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XX. 1° Semestre, Fasc. 9. 8vo. 1911.
American Academy of Arts and Sciences—Proceedings, Vol. XLVI. Nos. 18-24. 8vo. 1911.
American Geographical Society—Bulletin, Vol. XLIII. No. 6. 8vo. 1911.
Astronomical Society, Royal—Monthly Notices, Vol. LXXI. No. 7. 8vo. 1911.
Belgium, Royal Academy—Memoires, Collection in 4to, 2. Serie, Tome III. Fasc. 5. 1911.
British Architects, Royal Institute of—Journal, Third Series, Vol. XVIII. No. 15. 4to. 1911.
Carnegie Institution, Mount Wilson Solar Observatory—Contributions, Nos. 53-55. 8vo. 1911.
Chemical Industry, Society of—Journal, Vol. XXX. Nos. 11-12. 8vo. 1911.
Chemical Society—Proceedings, Vol. XXVII. No. 389. 8vo. 1911.
Journal for June, 1911. 8vo.
Colonial Institute, Royal—United Empire, Vol. II. No. 6. 8vo. 1911.
Corbridge Excavation Fund—Corstopitum: Report on Excavations in 1908. 8vo. 1909.
Editors—Agricultural Economist for June-July, 1911. 4to.
Athenæum for June, 1911. 4to.
Canada for June, 1911. 8vo.
Chemical News for June, 1911. 4to.
Chemist and Druggist for June, 1911. 8vo.
Dyer and Calico Printer for June, 1911. 4to.
Electrical Engineer for June, 1911. 4to.
Electrical Engineering for June, 1911. 4to.
Electrical Industries for June, 1911. 4to.
Electrical Review for June, 1911. 4to.
Electrical Times for June, 1911. 4to.
Electricity for June, 1911. 8vo.
Engineer for June, 1911. fol.
Engineer-in-Charge for June-July, 1911. 8vo.

Editors—continued.
Engineering for June, 1911. fol.
Journal of the British Dental Association for June, 1911. 8vo.
Journal of Physical Chemistry for May, 1911. 8vo.
Law Journal for June, 1911. 8vo.
London University Gazette for June, 1911. 4to.
Marconigraph for June, 1911. 8vo.
Model Engineer for June, 1911. 8vo.
Motor Car Journal for June, 1911. 4to.
Musical Times for June, 1911. 8vo.
Nature for June, 1911. 4to.
Nuovo Cimento for April, 1911. 8vo.
Page's Weekly for June, 1911. 8vo.
Science Abstracts for June, 1911. 8vo.
Zoophilist for July, 1911. 8vo.
Electrical Engineers, Institution of—Journal, Vol. XLVI. No. 207. 8vo. 1911.
Florence, Biblioteca Nazionale—Bulletin for June, 1911. 8vo.
Franklin Institute—Journal, Vol. CLXXI. No. 6. 8vo. 1911.
Geological Society—Abstracts of Proceedings, No. 912. 8vo. 1911.
Leicester Municipal Libraries—Annual Report, 1910–11. 8vo.
London County Council—Gazette for June, 1911. 4to.
Madrid, Reale Academia de Ciencias—Revista, Tom. IX. No. 8. 8vo. 1911.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. LV. Part 2. 8vo. 1911.
Marconi, Commendatore G., LL.D. D.Sc. M.R.I. (the Author)—Radiotelegraphy. 8vo. 1911.
Microscopical Society, Royal—Journal, 1911, Part 3. 8vo.
Monaco, Musée Océanographique—Bulletin, Nos. 206–210. 8vo. 1911.
Naber, Dr. H. A. (the Author)—Exacte (?) Wetenschap. 8vo. 1911.
National Church League—Church Gazette for July, 1911. 8vo.
New York, Society for Experimental Biology—Proceedings, Vol. VIII. No. 4. 8vo. 1910.
North of England Institute of Mining Engineers—Transactions, Vol. LVIII. Part 8; Vol. LXI. Part 4. 8vo. 1911.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for May, 1911. 4to.
Paris, Société Française de Physique—Journal de Physique for June, 1911. 8vo.
Pennsylvania, University of—The Sweet Potatoe. By B. H. A. Groth. 8vo. 1911.
The Museum Journal, Vol. II. No. 1. 8vo. 1911.
Pharmaceutical Society of Great Britain—Journal for June, 1911. 8vo.
Philadelphia Academy of Natural Sciences—Proceedings, Vol. LXIII. Part 1. 8vo. 1911.
Photographic Society, Royal—Journal, Vol. LI. No. 5. 8vo. 1911.
Physical Society of London—Proceedings, Vol. XXIII. Part 4. 8vo. 1911.
Royal Engineers' Institute—Journal, Vol. XIV. No. 1. 8vo. 1911.
Royal Irish Academy—Proceedings, Vol. XXIX. A, No. 2; Vol. XXXI. Part 39. I. 8vo. 1911.
Royal Society of Arts—Journal for June, 1911. 8vo.
Royal Society of London—Proceedings, A, Vol. LXXXV. No. 578; B, Vol. LXXXIII. No. 567. 8vo. 1911.
Philosophical Transactions, B, Vol. CCII. No. 284. 4to. 1911.
St. Petersburg, Imperial Academy of Sciences—Bulletin, 1911, Nos. 10–11. 8vo.
Sanitary Institute, Royal—Journal, Vol. XXXII. No. 6. 8vo. 1911.
Saxon Academy of Sciences, Royal—Abhandlungen: Phil. Hist. Klasse, Band XXVIII. Nos. 5–7. 8vo. 1911.
Berichte: Mat. Phys. Klasse, 1910, Nos. 6–7; 1911, Nos. 1–3; Phil. Hist. Klasse, 1910, Nos. 10–11. 8vo.

Selborne Society—Selborne Magazine for July, 1911. 8vo.
Societa degli Spettroscopisti Italiani—Memorie, Vol. XL. Disp. 5. 4to. 1911.
Statistical Society, Royal—Journal, Vol. LXXIV. No. 7. 8vo. 1911.
United Service Institution, Royal—Journal for June, 1911. 8vo.
United States, Department of Agriculture—Experiment Station Record, Vol. XXIV. Nos. 5-6. 8vo. 1911.
United States Patent Office—Gazette, Vol. CLXVII. 8vo. 1911.
Western Australia, Agent General—Monthly Statistical Abstract, Jan.-Feb. 1911. fol.

GENERAL MONTHLY MEETING,

Monday, November 6, 1911.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Miss Alice Clarendon,
The Right Hon. Lord Congleton,
Archibald Hall, Esq., F.C.S. F.I.C.

were elected Members of the Royal Institution.

The Honorary Secretary announced the decease of the Rt. Hon. Earl Cathcart, on September 2, of Professor Walter Victor Spring, on July 17, of Professor Louis Joseph Troost, on September 30, and of Professor G. L. Van der Mensbrugghe, on October 20, 1911, and the following Resolutions passed by the Managers at their Meeting held this day were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution in the decease of the Rt. Hon. Earl Cathcart, D.L. J.P., late Manager of the Institution.

Earl Cathcart was elected a Member in 1894, and a Manager in 1906, and again in 1911. He took great interest in the Institution, and did all in his power to promote its efficiency. He attended most regularly the Friday Evening Meetings and Lectures, and his presence will be greatly missed.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the Scientific World in the decease of Professor Walter Victor Spring, Ph.D.(Luttich), Hon.F.C.S., Professor of Chemistry in the University of Liège, and an Honorary Member of the Royal Institution since 1907.

Professor Spring had a world-wide reputation as one of the most original investigators in the domain of physical chemistry. Some of his most important work was directed to the study of high pressures upon matter and chemical combinations.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the Scientific World in the decease of Professor Louis Joseph Troost, D.Sc. Hon.F.C.S., Commandeur de la Légion d'Honneur, Membre de l'Institut, and an Honorary Member of the Royal Institution since 1907. Professor Troost was well known in the Scientific World for his original investigations in collaboration with the late Professor St. Claire Deville on the determination of vapour densities at high temperatures, and the elucidation of the laws of dissociation, and was one of the distinguished chemists who laid the foundations of many investigations in Thermal Chemistry. Professor Troost was the author of numerous independent papers and the well-known work "Traité Élémentaire de Chimie."

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the Scientific World in the decease of their Honorary Member, Professor G. L. Van der Mensbrugghe, D.Sc., Emeritus Professor and Member of the Royal Academy of Belgium, Commander of the Order of Leopold, and an Honorary Member of the Royal Institution since 1899.

Professor Mensbrugghe carried on for many years brilliant researches on liquids, in continuation of the work of Plateau, whose daughter he married. On the occasion of the celebration of the centenary of the Royal Institution he was present, and he afterwards reported a description to appear in the Belgian Academy Transactions.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Kodaikanal Observatory Bulletin, No. XXIV. 4to. 1911.
Report on Government Museum and Connemara Library, 1910–11. 4to. 1911.
Memoirs of Department of Agriculture: Botanical Series, Vol. IV. No. 2. Chemical Series, Vol I. No. 10, Vol. II. No. 1. 8vo. 1911.
Agricultural Journal of India, Vol. VI. Part 3. 8vo. 1911.
Geological Survey: Records, Vol XLI. Part 2–3. 8vo. 1911.
Agricultural Research Institute, Pusa: Bulletin Nos. 23–25. 8vo. 1911.
Report on Public Instruction in Bengal, 1909-11. 4to. 1910.

Astronomer Royal, The—Greenwich Observations, 1909. 4to. 1911.
Greenwich Photo-Heliographic Results, 1909. 4to, 1910.

British Museum Trustees—Catalogue of African Fresh-water Fishes, Vol. II. 8vo. 1911.
Handbook of the Tsetse Flies. By C. A. Austen. 8vo. 1911.
Catalogue of Lepidoptera Phalænæ, Vol. X. and Plates. 8vo. 1910–11.
Flora of Jamaica. By W. Fawcett and A. B. Rendle. Vol. I. 8vo. 1910.
Monograph of British Lichens, Part 2. 8vo. 1911.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XX. 1° Semestre, Fasc. 10-12. 2° Semestre, Fasc. 1-7. Classe de Scienze Morali. Vol. XX. Fasc. 1-4. 8vo. 1911.
Rendiconti, 1911. Vol. II. 4to.

Allegheny Observatory—Publications, Vol. II. Nos. 11-13. 4to. 1911.

American Academy of Arts and Sciences—Proceedings, Vol. XLVII. Nos. 1-7. 8vo. 1911.

American Geographical Society—Bulletin, Vol. XLIII. No. 7-10. 8vo. 1911.

American Philosophical Society—Proceedings, Vol. L. No. 199-201. 8vo. 1911.
Transactions, Vol. XXII. Part 1. 4to. 1911.

Amsterdam, Royal Academy of Sciences—Proceedings, Vol. XIII. Verslag, Vol. XIX. 8vo. 1910–11.

Verhandelingen, 1e Sectie, Dl. X. No. 2; Dl. XI. No. 1–2. 2e Sectie, Dl. XVI. No. 4–5. 8vo. 1910–11.
Jaarboek, 1910. 8vo.
Aristotelian Society—Proceedings, Vol. XI. 1910–11. 8vo. 1911.
Armstrong Professor H. E., Ph.D. LL.D. F.R.S. M.R.I.—Det Kongelige Frederiks Universitet, 1811–1911, Vol. I. 4to. 1911.
Asiatic Society, Royal—Journal for July–Oct. 1911. 8vo.
Asiatic Society, Royal (Bombay Branch)—Journal, Vol. XXIII. No. 65. 8vo. 1911.
Association of Accountants—Journal, Vol. IV. No. 15, Oct. 1911. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXI. No. 8. 8vo. 1911.
General Index to Monthly Notices, Vols. LIII.–LXX. 8vo. 1911.
Memoirs, Vol. LX. Part 2. 4to. 1911.
List of Fellows, 1911. 8vo.
Bankers, Institute of—Journal, Vol. XXXII. No. 7–8. 8vo. 1911.
Belgium, Royal Academy—Bulletin, 1911, Nos. 5–8. 8vo.
Mémoires in 8vo, 2e Serie, Tome III. Fasc. 1–2. 8vo. 1911.
Mémoires in 4to, Classe des Sciences, 2e Série, Tome III. Fasc. 6–7, 1911.
Berlin, Könegliche Technische Hochschule—Programm, 1911–12. 8vo.
Berlin, Royal Prussian Academy of Sciences—Sitzsungberichte, 1911, Nos. 23–38. 8vo.
Boston Public Library—Bulletin, Third Series, Vol. IV. No. 2–3. 8vo. 1911.
Brewing, Institute of—List of Members, 1911. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XVIII. Nos. 16–20. 4to. 1911.
The Kalendar, 1911–12. 8vo. 1911.
British Association for the Advancement of Science—Report of the Eightieth Meeting (Sheffield, 1910). 8vo. 1911.
British Astronomical Association—Journal, Vol. XXI. No. 8–9. 8vo. 1911.
Memoirs, Vol. XVII. Part 4. 8vo. 1911.
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Zoological Society of London—Transactions, Vol. XVIII., Part 5. 4to. 1911.

Proceedings, 1911, Part 2–3. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 4, 1911.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Miss Nora Goldsmid,
Habibur Rahman Khan, Esq., M.A.
William Mathias Noot, Esq., M.R.C.S. L.S.A.
Mrs. Middleton Robinson

were elected Members of the Royal Institution.

Professor Waldemar Christofer Brögger, Sc.D. Ph.D. Hon.F.R.S. (Christiania),
Geh. Rath. Professor Theodor Curtius, Ph.D. Hon.F.C.S.(Heidelberg),
Professor Philippe Auguste Guye, D.Sc.(Geneva),
Professor Clément Georges Lemoine, D.Sc.(Paris),
Geh. Regierungs-Rath Professor Heinrich Rubens, Ph.D.(Berlin)

were elected Honorary Members of the Royal Institution.

The Special Thanks of the Members were returned to D. J. Blaikley, Esq., for presenting to the Institution, from the bequest of the late Miss J. Barnard and at her express wish, a large volume of Faraday's containing Engravings, Portraits, and Letters, a companion volume to one already in the possession of the Royal Institution. Mr. Blaikley attended the Meeting and made a statement in regard to the volume.

The Honorary Secretary reported, That the Managers, at their Meeting held this day, had appointed William Bateson, Esq. M.A. F.R.S., Fullerian Professor of Physiology, for a term of three years, in conformity with the Deed of Endowment, the appointment dating from January 13, 1912.

The Honorary Secretary read the following letter he had received from His Grace the Duke of Northumberland, President:—

ALNWICK CASTLE,
2nd December, 1911.

DEAR SIR WILLIAM,

I am very sorry to find that I cannot attend the Meeting of the Managers of the Royal Institution on Monday. I had hoped to have had the pleasure of moving a Resolution expressing the feeling which I know is entertained by the Managers that all the Members of the Royal Institution owe a very great debt of gratitude to Sir James Dewar for his generosity in undertaking the improvement of the Lecture Room, and for the judgment and taste with which it has been carried out. We must all have been sensible that the appearance of the apartment was hardly worthy of the Institution, of the great names with which it is associated, and of the magnificent work which has been done in it.

I am sure that it gives unbounded satisfaction to all the Members of the Institution, and especially to those who have the privilege of Sir James's friendship, to know that the decoration of the Lecture Room signalizes a longer tenure of his office than that which has been enjoyed by any of his predecessors, and one which has so signally redounded to the credit of the Institution itself, and has been the means of promoting so many important advances in Science.

I have little doubt that whoever occupies the Chair on Monday will express these ideas much more adequately than I can, but I cannot resist saying so much on my own behalf.

Pray convey to Sir James my hearty congratulations and entire sympathy with the action the Managers propose to take, and my sincere wish that he may long continue to hold the post which he has so long and so signally adorned.

I am, dear Sir William,
Yours sincerely,
(Signed) NORTHUMBERLAND.

SIR WILLIAM CROOKES, O.M. F.R.S., etc.

and the following Resolution was passed unanimously :—

Resolved, That the Managers offer their Special Thanks to the Fullerian Professor of Chemistry for his munificent gift to the Institution in the Decoration and Furnishing of the Lecture Room, and at the same time they wish to express their high appreciation of the occasion upon which it has been made, namely, in commemoration of his having, on October 23, 1911, occupied the Chair of Chemistry as long as it was held by Faraday.

The following Lecture Arrangements were announced :—

P. CHALMERS MITCHELL, Esq., M.A. LL.D. D.Sc. F.R.S., Secretary, Zoological Society of London. Six Lectures (adapted to a Juvenile Auditory) on THE CHILDHOOD OF ANIMALS. On *Thursday*, Dec. 28, Dec. 30, 1911; Jan. 2, 4, 6, 9, 1912.

WILLIAM BATESON, Esq., M.A. F.R.S., Fullerian Professor of Physiology, R.I. Six Lectures on THE STUDY OF GENETICS. On *Tuesdays*, Jan. 16, 23, 30, Feb. 6, 13, 20.

PROFESSOR ERNEST G. COKER, M.A. D.Sc. F.R.S.E. Two Lectures on OPTICAL DETERMINATION OF STRESS, AND SOME APPLICATIONS TO ENGINEERING PROBLEMS. On *Tuesdays*, Feb. 27, March 5.

THOMAS RICE HOLMES, Esq., Litt.D Three Lectures on ANCIENT BRITAIN. On *Tuesdays*, March 12, 19, 26.

PROFESSOR A. W. BICKERTON. Two Lectures on THE NEW ASTRONOMY. On *Thursdays*, Jan. 18, 25

PROFESSOR A M. WORTHINGTON, C.B. M.A. F.R.S. *M.R.I.* Two Lectures on THE PHENOMENA OF SPLASHES. (*Experimentally Illustrated*). On *Thursdays*, Feb. 1, 8.

M. H. SPIELMANN, Esq., F.S.A. *M.R.I.* Two Lectures on THE PORTRAITURE OF SHAKESPEARE. On *Thursdays*, Feb. 15, 22.

PROFESSOR CHARLES OMAN, M.A. LL.D. F.S.A. Three Lectures on WELLINGTON'S ARMY. On *Thursdays*, Feb. 29, March 7, 14.

F. A. DIXEY, Esq., M.A. M.D. F.R.S. Two Lectures on DIMORPHISM IN BUTTERFLIES: (1) SEASONAL DIMORPHISM; (2) SEXUAL DIMORPHISM. On *Thursdays*, Feb. 21, 28.

THE REV. JOHN ROSCOE, M.A. Two Lectures on THE BANYORO: A PASTORAL PEOPLE OF UGANDA: (1) THE MILK CUSTOMS; (2) BIRTH AND DEATH CUSTOMS. On *Saturdays*, Jan. 20, 27.

SIR ALEXANDER C. MACKENZIE, Mus.Doc. D.C.L. LL.D. *M.R.I.*, Principal of the Royal Academy of Music. Three Lectures on (1) RUSSIAN MUSIC OF TO-DAY (*with the kind assistance of the Hans Wessely Quartet*); (2 and 3) FRANZ LISZT (CENTENARY). (*With Musical Illustrations.*) On *Saturdays*, Feb. 3, 10, 17.

PROFESSOR SIR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*, Professor of Natural Philosophy, R.I., etc. Six Lectures on MOLECULAR PHYSICS. On *Saturdays*, Feb. 24, March 2, 9, 16, 23, 30.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Great Trigonometrical Survey of India, Vol. XIX. 4to. 1910.
Agricultural Journal of India, Vol. VI. Part 4. 8vo. 1911.
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Register of Fellows, etc., 1911.
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Cracovie, Academy of Sciences—Bulletin, 1911: Matt.-Phys. Klass A, Nos. 5, 7; Philol. Klasse, Nos. 4-5. 8vo.
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Athenæum for Nov. 1911. 4to.
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Editors—continued.
Church Gazette for Nov. 1911. 8vo.
Concrete for Nov. 1911. 8vo.
Dyer and Calico Printer for Nov. 1911. 4to.
Electrical Engineer for Nov. 1911. 4to.
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Electrical Industries for Nov. 1911. 4to.
Electrical Review for Nov. 1911. 4to.
Electrical Times for Nov. 1911. 4to.
Electricity for Nov. 1911. 8vo.
Engineer for Nov. 1911. fol.
Engineer-in-Charge for Nov. 1911. 8vo.
Engineering for Nov. 1911. fol.
Horological Journal for Nov. 1911. 8vo.
Illuminating Engineer for Nov.-Dec. 1911. 8vo.
Journal of the British Dental Association for Nov. 1911. 8vo.
Journal of Physical Chemistry for Nov- 1911. 8vo.
Law Journal for Nov. 1911. 8vo.
Marconigraph for Nov.-Dec. 1911. 8vo.
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WEEKLY EVENING MEETING,

Friday, January 20, 1911.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

PROFESSOR SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*,
Fullerian Professor of Chemistry.

Chemical and Physical Change at Low Temperatures.

THE retarding effect of extremely low temperatures on chemical action has long been known, and it was generally supposed that at a sufficiently low temperature all such action would be completely suspended. This inference was much shaken when it was found that liquid hydrogen and solid fluorine combined with explosive violence at 20° absolute.* One of the most characteristic reactions of ozone is that it can be dissolved in carbon disulphide at a temperature of about −100° C., showing a deep blue colour, whereas on slightly raising the temperature the colour disappears owing to rapid oxidation. But perhaps the most remarkable of all low temperature reactions is the fact mentioned in the Friday Evening Discourse, 1910,† that solid oxygen at the temperature of boiling hydrogen is capable of being partially transformed into ozone by the impact of ultra-violet rays. In this case the action is clearly a molecular transition, involving the absorption of energy, taking place in the solid state in one body, and not an interaction of two bodies as in the cases of ozone and carbon disulphide, and the combination of liquid hydrogen and solid fluorine.

ACTION OF OZONE AT THE TEMPERATURE OF LIQUID AIR.

A convenient method of illustrating the effects of low temperatures on chemical action is by the use of liquid and gaseous ozone. For the purpose of experiment its condensation can be easily effected with the aid of liquid air, in the following manner. Oxygen, after cleaning by passing through a **U**-tube B (Fig. 1) immersed in liquid oxygen, is exposed to the silent electric discharge in the ozoniser C, and bubbled through the tube D, the end of which dips into liquid air in

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* Moissan and Dewar, Comptes Rendus, vol. CXXXVI. p. 643.
† Proc. Roy. Inst., vol. XIX. p. 925.

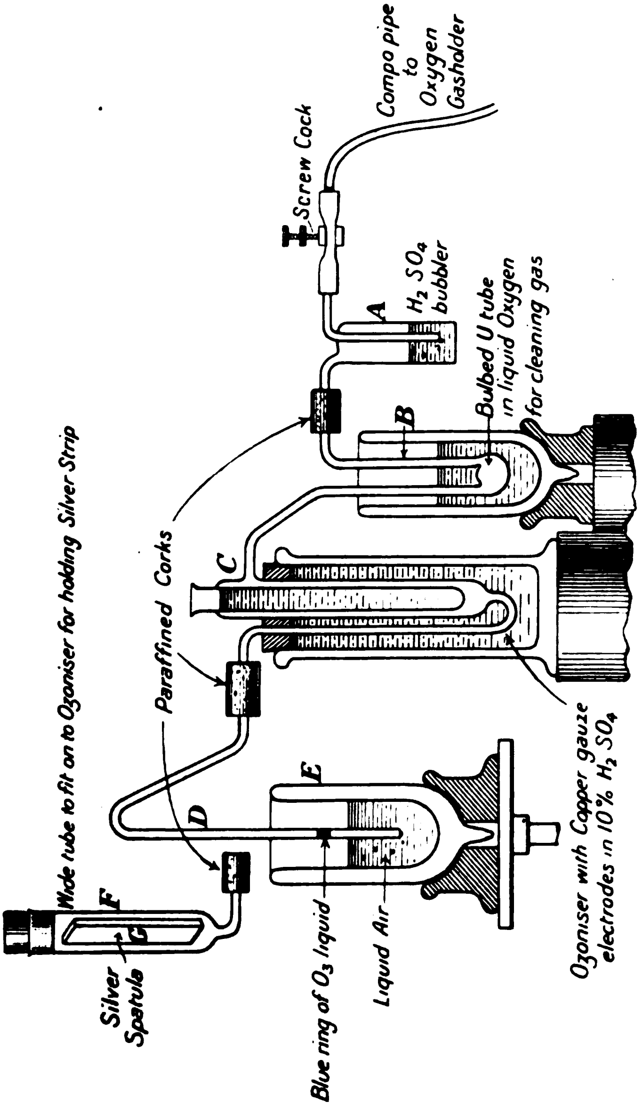
Compo pipe to Oxygen Gasholder
Screw Cock
A
H_2SO_4 bubbler
B
Bulbed U tube in liquid Oxygen for cleaning gas
C
Paraffined Corks
Ozoniser with Copper gauze electrodes in 10% H_2SO_4
E
D
Wide tube to fit on to Ozoniser for holding Silver Strip
F
G
Silver Spatula
Blue ring of O_3 liquid
Liquid Air

Fig. 1.

the vacuum vessel E. In a short time a ring of blue liquid begins to form in the tube a little above the level of the liquid air. This is condensed liquid ozone, which quickly dissolves when brought into contact with the liquid air.

The rapid oxidation of silver, as shown by the blackening of the metal which results from the action of ozone on a plate of polished silver at ordinary temperature, is well known; if, however, a strip of silver G (Fig. 1) be cooled in liquid air and placed in the tube F connected to the ozoniser C and subjected to the action of the gaseous ozone, no blackening is noticed for a considerable time, but as soon as it warms up to the temperature of the room the strip turns black. If a similar piece of silver be immersed in liquid air containing ozone in solution, and left there indefinitely, there is no apparent action and no blackening results.

From the early days of low temperature research it has been known that mere cooling to the temperature of liquid air does not destroy bacterial life, the organisms recovering their vital functions after weeks of immersion. The bactericidal effect of the ultra-violet light was discussed in the Friday Evening Discourse, 1910, on Light Reactions at Low Temperatures,* when it was shown that the effect of the ultra-violet light on bacteria, even at the temperature of liquid air, was to pulverize the organisms. It has been found by experiment that the bacteria can be protected from the action of the ultra-violet light by means of thin sheet lead. Aluminium foil, on the other hand, is sufficiently transparent for the ultra-violet rays to remain effective in killing the bacteria.

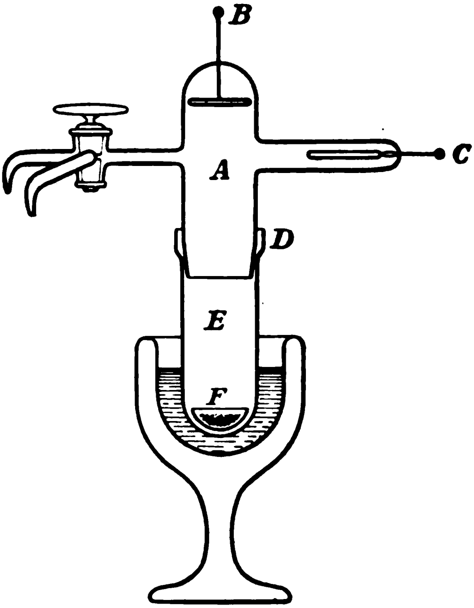

Fig. 2.

Bacteria cooled in liquid air may also be subjected to the action of the cathode discharge in the following way. A glass tube A (Fig. 2), provided with platinum electrodes B and C, is connected at D by means of a vaselined ground-glass joint to a tube E. The luminous bacteria, cultivated on a thin surface layer in a tin cup F, is placed in the bottom of the tube E, the end of which is immersed in liquid air. The united tubes are then sufficiently exhausted to allow the discharge to pass, and the cathode rays from B strike directly on the surface of the bacterial culture. Experiments have shown that hours of the cathode discharge will not kill the bacteria.

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* Proc. Roy. Inst., vol. XIX., p. 921.

SOLIDIFICATION OF GASES.

With regard to the liquefaction and solidification of gases, all have been liquefied and solidified except helium, which still resists solidification, although its temperature has been reduced to below 3° abs.

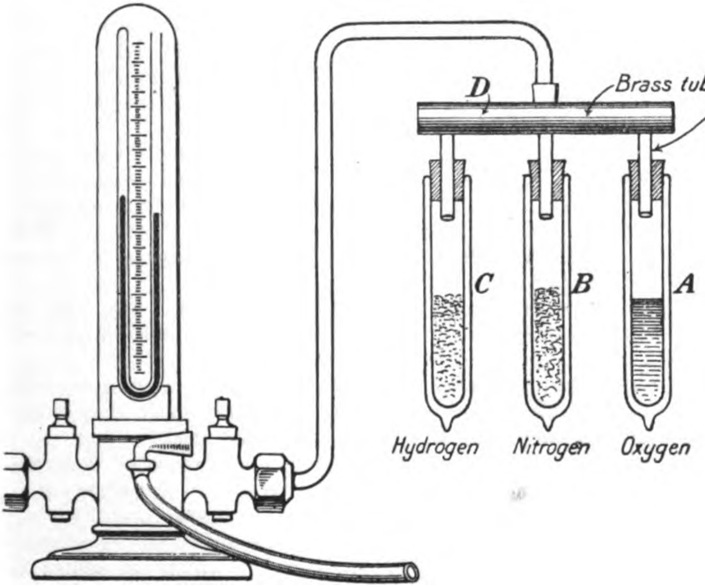

FIG. 3.

The solidification of oxygen has been finally accomplished by the aid of a charcoal vacuum and of thermal isolation, such as can be obtained with silvered vacuum vessels. The difference as regards solidification of the three liquids—nitrogen, hydrogen, and oxygen—

is plainly shown when they are made to boil by exhaustion in jacketed glass tubes (Fig. 3) connected to a powerful pump. The nitrogen and hydrogen turn into snowy solids; but the oxygen remains liquid.

It seems strange that while liquid hydrogen or nitrogen can be thus easily changed into the solid condition by evaporation under exhaustion, yet the ordinary air-pump fails to effect the transition of state in the case of liquid oxygen. This is due to the small vapour-pressure of solid oxygen at its melting point, as compared with hydrogen or nitrogen under similar conditions—together with the greater need of very perfect heat-isolation in the arrangement of the experiment. With the aid of charcoal as a gaseous condensing agent at low temperatures, combined with the employment of proper vacuum vessels, the change from the liquid into the solid can be effected. Pure liquid oxygen, contained in a properly isolated vessel, subjected to the exhaust produced by a quantity of charcoal kept at about the temperature of boiling oxygen, has its pressure lowered sufficiently to produce solidification to a transparent jelly. The pressure at which solidification takes place is determined by connecting a McLeod gauge to the vessel containing the solid oxygen.

About 75 grm. of good coconut charcoal is necessary to produce and maintain the necessary conditions of exhaustion. From 5 to 10 c.cm. of liquid oxygen are employed, previously exhausted by an air-pump. The oxygen exhibits considerable supercooling, and a pressure of less than half the melting pressure can usually be maintained on the liquid without producing solidification. These experiments indicate that the melting-point pressure is between 1·115 and 1·125 mm.

Fig. 4 shows the arrangement of the apparatus. Fig. 5 shows a modification, in which the oxygen is condensed in a bulb immersed in the solid oxygen produced by the charcoal exhaust.

In Fig. 4, A is a silvered vacuum-jacketed tube, with an uncoated slit, containing the oxygen to be exhausted. This is further isolated by a larger vacuum tube B, also silvered, and with a clear slit as above, which contains liquid air under an exhaust of less than 20 mm. A is sealed by the tube E on to a T-piece, whose limbs connect (1) through a constricted portion to bulbs containing permanganate of potash crystals and phosphoric anhydride respectively, (2) to a three-way cock C, to the arms of which are sealed (*a*) the bulb D, containing 75 grm. of coconut charcoal, (*b*) a T-piece connecting to the mercury-pump stopcock and the McLeod gauge. As soon as the charcoal vacuum is turned off the oxygen jelly melts, to solidify once more when the vacuum is again turned on. The reason of the peculiar behaviour of the oxygen will be understood from the following table, giving the melting-point pressures

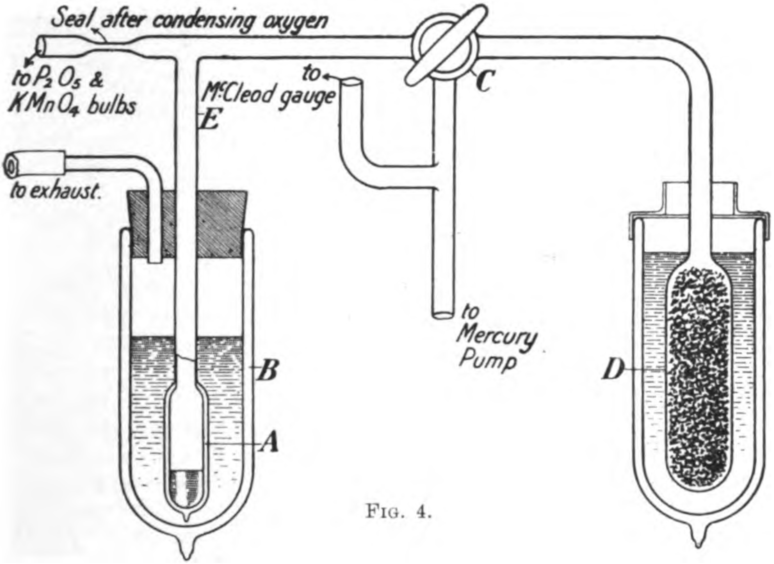
Seal after condensing oxygen
to P_2O_5 &
$KMnO_4$ bulbs
to
McCleod gauge
E
C
to exhaust.
to
Mercury
Pump
B
A
D

FIG. 4.

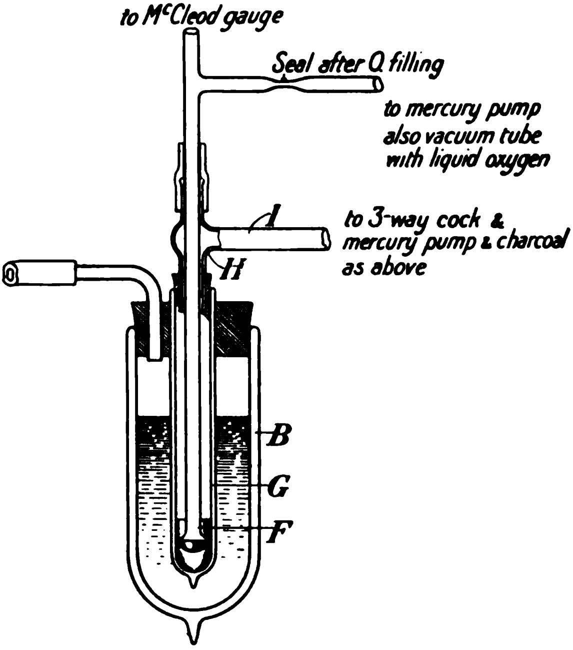
to McCleod gauge
Seal after O filling
to mercury pump
also vacuum tube
with liquid oxygen
I
to 3-way cock &
mercury pump & charcoal
as above
H
B
G
F

FIG. 5.

(in millimetres of mercury), and the corresponding temperatures (in degrees absolute) for the chief gases :—

	Melting Point Pressure	Absolute Temperature
Helium . . .	Below 2 mm.	Below 2°·5
Neon	350 „	35°
Argon	628 „	85°
Krypton . . .	158 „	104°
Xenon	86 „	133°
Hydrogen . . .	50 „	14°
Nitrogen . . .	90 „	59°
Oxygen . . .	0·9 „	46°

Any good air-pump, capable of maintaining an exhaust below 0·1 mm. with a reasonable leak, can easily cause oxygen to solidify if thermally isolated.

SEPARATION OF MIXED GASES.

The easy application of low temperatures, combined with the use of charcoal, has led to a means of separating mixed gases by the use of various liquids as cooling agents. When the least volatile gas is condensed, the residual pressure is due to the more volatile gases, and finally to helium, which is the most difficult to condense.

The use of liquid hydrogen and charcoal as agents in the separation of gaseous mixtures may be shown as follows :—Two long glass tubes, A and B (Fig. 6), each originally full of ordinary air, are joined, B to a charcoal bulb dipped into liquid air, and A to a plain tube cooled in liquid hydrogen. Under the electric discharge, both tubes show the ruddy glow of mixtures of neon and helium. When the two cooling vessels are removed the discharge in A, cooled by liquid hydrogen, stops almost instantaneously ; the discharge in the other tube, B, cooled by liquid air and charcoal, continues almost unchanged for some time. The reason being that the cooling effect of the liquid hydrogen disappears almost instantaneously, while in the other tube the gases are only very slowly liberated from the charcoal after the cooling by liquid air is stopped.

In the following table the results of the separation of the rare gases in the atmosphere by different methods are given, the figures in the last column being those obtained by using liquid hydrogen as the cooling agent.

RARE GASES IN THE ATMOSPHERE.

Per Million Volumes.

	1902 Dewar.	1905 Ramsay.	1910 Claude.	1910 Dewar.
	Float Method	Charcoal	Industrial Working	Liquid Hydrogen
Total	17·2	16·4	20·0	20·0
Neon	14·4	12·3	15·0	15·0
Helium	2·7	4·0	5·0	5·0

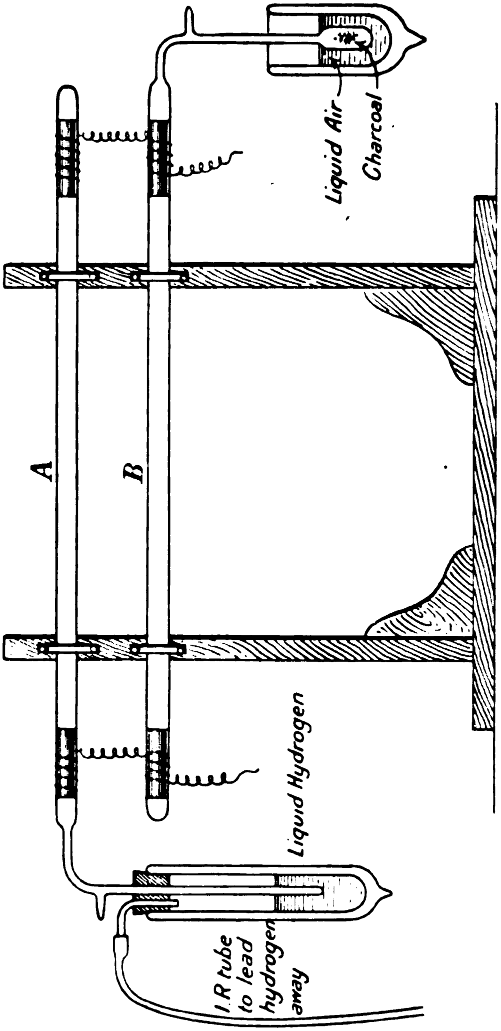
A
B
Liquid Air
Charcoal
Liquid Hydrogen
I.R tube to lead hydrogen away

FIG. 6.

THE PRODUCTION OF AN EXPLOSIVE GAS FROM CARBON DISULPHIDE. SEPARATION BY CONDENSATION AT THE TEMPERATURE OF LIQUID AIR.

Such experiments suggest that there might be gaseous bodies of so evanescent a character that their existence could only be detected by their sudden condensation and freezing out at low temperatures. Carbon monoxide has a boiling-point of − 190° C., and a melting-point of − 211° C., and is much more volatile than carbon dioxide whose boiling-point is − 78° C., and melting-point 57° C., and a parallel relationship might be anticipated in the corresponding sulphur compounds. When studying the nickel carbonyls in conjunction with the late Dr. H. O. Jones, such relationships seemed to be reversed in the case of the carbon sulphides. Pure carbon disulphide is colourless, has a pleasant odour, and boils at + 40° C., while the monosulphide, separated by chemical means, was a brown odourless powder. By analogy, therefore, it might be inferred that the boiling-point of the carbon monosulphide ought to be about − 180° C.

The reagents used to produce the monosulphide were thiophosgene and carbon disulphide acting on nickel carbonyl. The possible molecular reactions are three in number:

1. Nickel carbonyl + thio-phosgene = nickel chloride +
$Ni(CO)_4$ $CSCl_2$ $NiCl_2$
carbonic oxide + carbon monosulphide.
$4CO$ CS

2. Nickel carbonyl + carbon disulphide = nickel sulphide +
$Ni(CO)_4$ CS_2 NiS
carbonic oxide + carbon monosulphide.
$4CO$ CS

3. Nickel carbonyl + carbon disulphide = nickel sulphide +
$2Ni(CO)_4$ CS_2 $2NiS$
carbonic oxide + carbon.
$8CO$ C

When the densities of solid carbon disulphide, carbon dioxide, and this chemically separated monosulphide were observed, the following results were obtained: Carbon disulphide $[CS_2] = 52{\cdot}4$; Polymerized substance $(CS)_x = 24$; Carbonic acid $[CO_2] = 28{\cdot}7$; Carbon monoxide $[CO] = 28$. It thus appears possible that the supposed monosulphide is really $(CS)_x$, a polymerized body, and that the real monosulphide might be isolated by means of the freezing-out process.

ISOLATION OF THE MONOSULPHIDE.

With the object of attempting the isolation of such a substanc by the action of the "silent" electric discharge on carbon disulphide

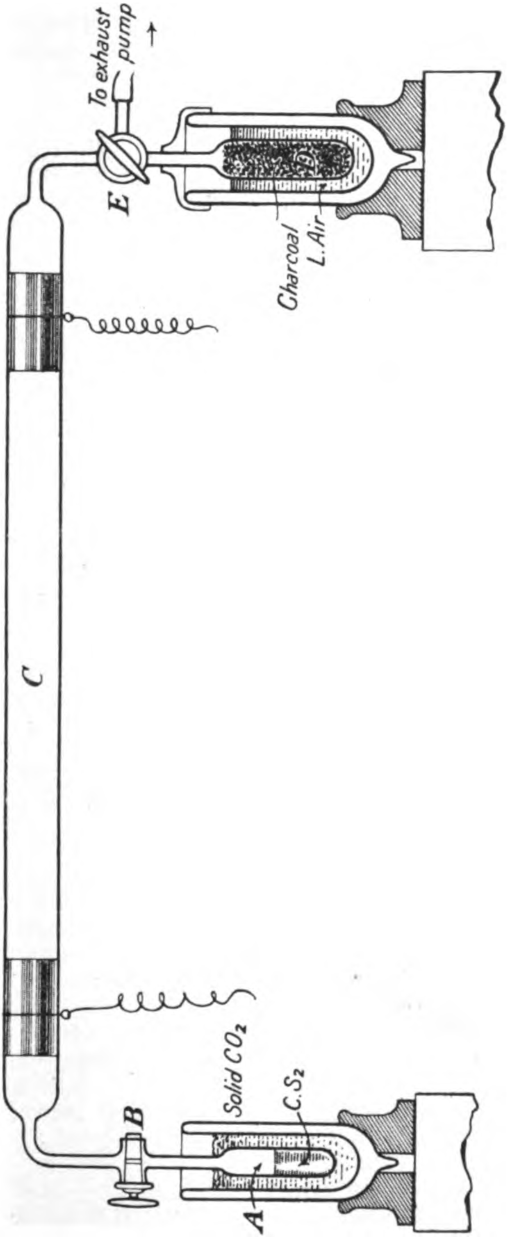

FIG. 7.

vapour, the following apparatus (Fig. 7) was devised. A discharge tube C, 3 ft. long and 2 in. diam., provided with outside tinfoil electrodes, is highly exhausted by a charcoal bulb in liquid air, so that no electric discharge will pass. A small carbon disulphide reservoir cooled to −80° C. in solid carbonic acid is also connected to the discharge tube through the stopcock. When this stopcock is momentarily turned on, the tube is charged with carbon disulphide vapour at about 1 mm. pressure, and the discharge is of a uniform lilac colour. This, however, rapidly diminishes in intensity, and in a few minutes ceases, showing that a high vacuum has been produced. The carbon disulphide is dissociated by the electric discharge into sulphur and carbon monosulphide, the latter rapidly polymerizing to a non-volatile form at the room temperature, as evinced by a brown stain produced on the interior surface of the tube.

Another method of detecting this monosulphide is shown in Fig. 8, where a litre bulb H containing carbon disulphide vapour that has been subjected to the electric discharge can be cooled locally by means of a pad of cotton wool saturated in liquid air, and thus any condensable material obtained as a deposit on the interior of the bulb. Before the discharge is turned on, the carbon disulphide distilling through the apparatus may be deposited by this local cooling as a white spot which volatilizes on warming, leaving no residuary mark. After the discharge is started the patch obtained in this way is yellow, changing quickly to dark brown on removal of the cooling pad, it does not volatilize on warming, but leaves a permanent brown film. The evanescent character of the body thus produced may be seen by stopping the discharge and leaving the tube for a few minutes fully charged with carbon disulphide vapour as it was before the local cooling. When the pad is again applied nothing but the white carbon disulphide can be condensed, and no brown film results.

The actual isolation of the volatile unstable monosulphide can be accomplished in the following way. The apparatus (Fig. 8), consists of a tube B containing carbon disulphide placed in a Dewar vessel filled with a paste of solid carbon dioxide and alcohol, communicating through a tap C with the annular space of the ozoniser A, the carbon disulphide thus giving off vapour at a pressure of 0·8 mm. of mercury, which has proved most suitable for the passage of the electric discharge to effect decomposition of the disulphide into the monosulphide. The resulting gas is passed through an exhausted U-tube L, a few millimetres in diameter, placed in liquid air. A little above the level of the liquid air a brown ring condenses in the U-tube. This is the real monosulphide. When the liquid-air vessel is removed the tube explodes, and the fragments of glass are found to be partly covered with a brown powder consisting of the polymerized monosulphide. It would appear that

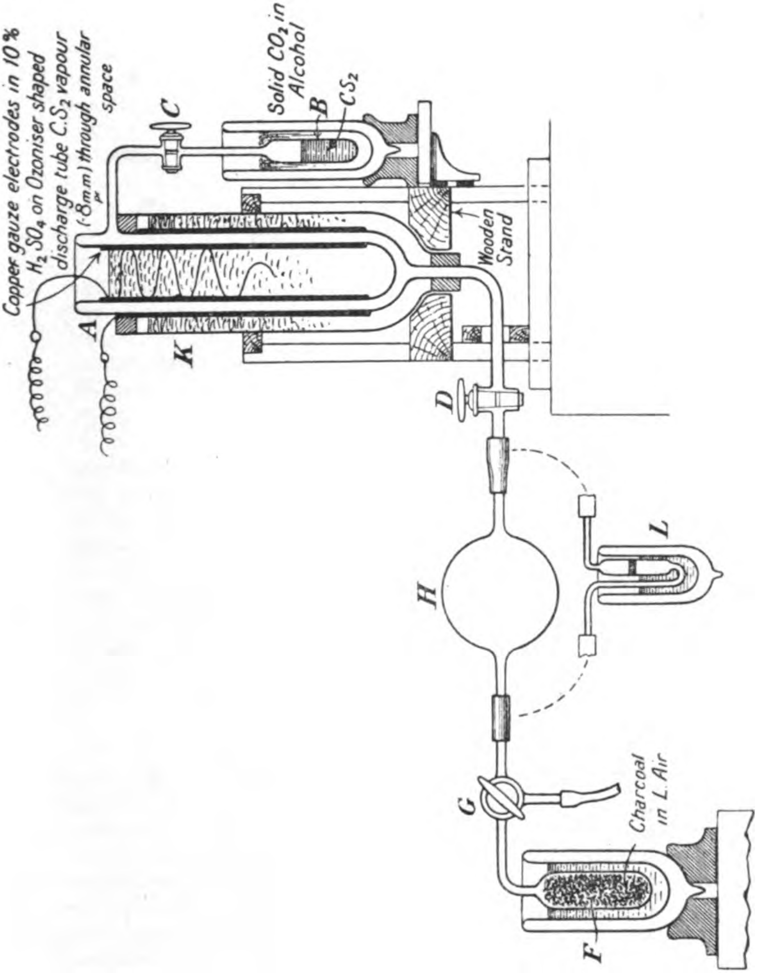
Copper gauze electrodes in 10% H_2SO_4 on Ozoniser shaped discharge tube $C.S_2$ vapour (·8m.m) through annular space
μ
A
K
C
Solid CO_2 in Alcohol
B
CS_2
Wooden Stand
D
H
L
G
Charcoal in L. Air
F

FIG. 8.

this explosive polymerization takes place a little above the temperature of liquid air, and possibly also at a still lower temperature, though more slowly. At normal temperature the real monosulphide can only exist for about half a minute; under ordinary circumstances it at once polymerizes, or its temporary existence, at any rate, cannot be detected.

A number of experiments made to ascertain if the phenomena were in any way dependent on the presence of impurities proved that when carbon disulphide, however purified, distilling from the liquid kept at − 78° C. into a charcoal condenser cooled to − 185° C. was submitted to the action of the silent electric discharge, and the vapour together with any new product condensed in a **U**-tube placed in liquid air, a brown solid resulted, the formation of which was accompanied by a glow or a flash and usually by a detonation on removing the liquid air surrounding the **U**-tube. Hence it is clear that these phenomena are not caused by the presence of impurities in the carbon disulphide, and that the evolution of light and heat are definitely associated with the transformation of the condensed gaseous product into the brown solid. It will be seen later that the flash and the detonation are to some extent variable phenomena.

The apparatus may be modified by connecting the ozoniser by means of a 3-way tap with two similar **U**-tubes packed with finely shredded cotton wool or asbestos offering an equal resistance to the passage of the stream of carbon disulphide vapour, and through a second 3-way tap to the charcoal condenser immersed in liquid air. When it was desired to observe the action of substances such as finely divided metals on the gases from the ozoniser these substances were either mixed with or deposited on asbestos, so that the deposit obtained when the gases were passed through it and subsequently condensed by liquid air could be compared with that obtained when the gases had passed over pure asbestos. Alternatively, either tube could be left free of any filtering material, and the character and proportion of the material deposited compared.

In order to ascertain if the rate of production of the brown solid could be increased, experiments were made in which the vapour of carbon disulphide evolved from the liquid at different temperatures between − 78° C. and 0° C. was allowed to pass through an apparatus in which an ordinary vacuum tube with flat platinum electrodes replaced the ozoniser, and from this entered the wide tube condenser which replaced the **U**-tube used in other cases. The surface of the vacuum tube soon becamec oated with a whitish deposit of sulphur, and later with the brown solid. The product condensed at the temperature of liquid air transformed into a brown solid with the evolution of light and heat as usual, and this also occurred when the vapour had passed through a cotton-wool filter.

Experiments were also made in which the ozoniser was either cooled to − 80° C. or heated to 220° C., the other conditions being as usual.

In both cases the brown solid was formed in the U-tube, but the quantity obtained was less than under the ordinary conditions of temperature.

In none of the experiments was the amount of brown solid obtained greater than in those in which the vapour from carbon disulphide at −78° C. was passed through an ozoniser, and these conditions were therefore adopted for all subsequent experiments.

The determination of the ratio between the amounts of brown solid formed in the U-tube and of carbon disulphide passed through the ozoniser was found to be difficult owing to the almost invariable breaking of the tube during the transformation.

The ratio of the weight of the solid to that of the carbon disulphide varied from 1 to 10 to 1 to 50 in different experiments, and was found to depend on the form of the apparatus and the amount of solid surface over which the ozoniser product passed. Another very potent influence was found to be the deposit on the ozoniser surface. A clean ozoniser gave a much larger quantity of brown solid than an ozoniser that was coated with a deposit.

THE TRANSFORMATION OF THE CONDENSED OZONISER PRODUCT INTO A NON-VOLATILE SOLID.

The solid deposited in the condenser at −185° C. is at first white, but soon acquires a brown colour, the darkest portion being nearest the liquid air surface. On allowing the temperature to rise, the brown colour spreads rapidly, and this change is usually accompanied by a glow or flash and a detonation which may be violent enough to shatter the condenser. The transformation may, however, take place quietly without an audible detonation or a visible flash, and, on the other hand, a detonation may take place while the condenser is still immersed in liquid air.

The interposition of a cotton-wool filter between the ozoniser and the condenser diminishes the intensity of the phenomena observed in the condenser to a very considerable extent, and at the same time increases the rate at which the brown solid is deposited in the ozoniser and leading tubes.

Quantitative experiment showed that the interposition of cotton-wool filters reduced the rate of flow of carbon disulphide to about one-fourth its former value, and the amount of brown solid to about one-fifteenth.

When the U-tube was replaced by a condenser of the test-tube form cooled to −210° C. by immersion in liquid air under exhaustion and the gases admitted, the solid deposit which was formed on the bottom of the wide tube was perfectly white. After 15 minutes, however, the deposit had acquired a brown colour, showing that even at −210° C. the transformation was taking place slowly. On allowing the temperature to rise to −185° C. the transition took place more rapidly.

EXAMINATION FOR THE PRESENCE OF SOLID PARTICLES IN THE GASES FROM THE OZONISER.

A litre bulb with a U-tube on either side was sealed between the ozoniser and the charcoal condenser, and an arc lamp arranged to project a beam of light through this relatively large space filled with the transforming material from the ozoniser. The glow in the ozoniser was screened off, and thus any solid particles in the bulb would be made evident by the illumination of the beam through the bulb.

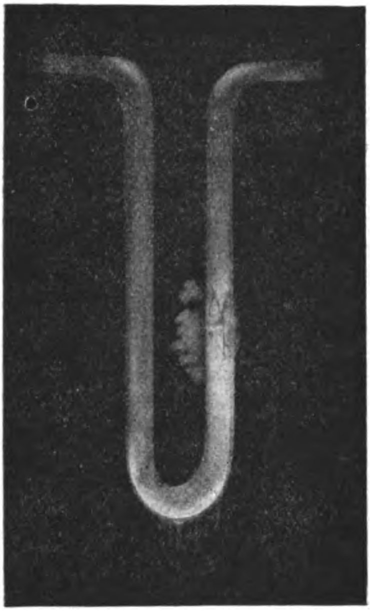

FIG. 9.

The arc lamp beam within the bulb was invisible, whether it was arranged converging to a focus or parallel. Several good patches of transforming material were condensed on the bulb by local cooling with a cotton-wool plug soaked in liquid air; 35 seconds after turning off the discharge in the ozoniser only white carbon disulphide was thus condensed, all the transforming material having gone. A five minutes' deposit in the second U-tube beyond the litre bulb gave the usual detonating transformation, and shattered the tube. A similar deposit in the first U-tube produced, however, a distinctly more violent effect, and 2 or 3 inches of the tube were completely shattered. No cotton-wool plug was used between the ozoniser and the bulb in the above experiment. Similar results as to the non-visibility of the beam within the bulb were obtained when a cotton-wool plug was used; but the deposits now gave less violent transformations. These observations indicate that the transformation of the gas into the brown solid only takes place in contact with solid surfaces.

Theoretically, the detonation of carbon disulphide with the production of carbon and sulphur could produce a temperature above 2000° C., and if the transforming ozoniser product is comparatively endothermic, then a similar rise of temperature might occur if it could decompose in the same manner.

The high temperature produced during the transformation has been proved by taking a photograph of the U-tube by its own detonating flash (Fig. 9). The cracks in the fractured tube are plainly visible, a flame is seen bursting through one such crack, and the light extends for a considerable distance through the tube.

Before allowing the deposit to flash by the removal of the liquid air, in some experiments the U-tube was filled with dry nitrogen at atmospheric pressure, through a 3-way tap on the charcoal bulb. The right-hand limb of the U-tube contained copper wire gauze to cool the entering nitrogen and thereby prevent any undue heating of the deposit.

Two seven-minute deposits were successfully flashed after filling with nitrogen. In both cases a violent detonation resulted, and the tube was shattered. The photographic plates showed good pictures of each flash, which was seen to be localized and not so intense, due no doubt to the relatively large pressure of nitrogen in the U-tube compared with previous experiments in which no inert gas was present.

PHOTOGRAPHS OF THE SPECTRUM OF THE FLASH.

The arrangement used to secure a photograph of the spectrum of the flashes consisted of a German silver U-tube with a quartz window at the top of one limb. The flash occurring in the tube was projected through this window by means of a quartz prism, and reflected on to the slit of a Hilger quartz spectrograph.

About seven flashes from deposits of 15 minutes' periods were necessary to get a good image of the spectrum.

The sulphur bands, wave-lengths between 384 and 392, and the cyanogen line 3885 were present. The principal hydrocarbon bands were plainly visible, of which that at 436–443 was the strongest in the whole spectrum. A strong band at 635–655 and scattered bands from 248 to 362 were present also, and fainter bands from 540 to 605.

The solid product obtained after the flashes taken in the metal tube was found to be a mixture of carbon and sulphur due to the high temperature of the flash, as the character of the spectrum tends to show.

A number of tests for electric effect in the transformation of the condensed ozoniser product had negative results.

ATTEMPTS TO CONCENTRATE THE NEW GAS.

It was found that the gas was completely condensed in a U-tube at −185° C., but that a similar U-tube, with a cotton-wool plug in the limb remote from the ozoniser, cooled to −120° C. by immersion in solid carbon dioxide under reduced pressure, allowed some of the gas to pass through it and to be condensed in a second U-tube at −185° C.

The resistance of the cotton-wool plugs was estimated by cooling the second U-tube in liquid air. In five minutes a deposit 15 to 20 mm. in length was formed.

The two U-tubes being thus at −120° and −185° C. respectively, the discharge was started in the ozoniser. At first no coloured deposit could be seen in the U-tube in liquid air, but later the deposit became coloured, and in 20 minutes a distinct brown ring was observed at a point 3 mm. above the liquid air surface.

Where the gases entered the first limb of the U-tube at −120° C. a white crystalline deposit was observed; this did not melt until the temperature had risen almost to 0° C., when it distilled back into the bulb containing carbon disulphide.

This indicates that the ozoniser product contains two substances, and shows that the substance which gives rise to the brown solid is not condensed by cooling to −120° C., but passes on and is slowly transformed into the brown solid in the U-tube at −185° C.

ACTION OF COCONUT CHARCOAL AND RUBBER.

The vapours were passed over coconut charcoal at −185° C., and the results showed that the ozoniser product is thereby absorbed, destroyed, or caused to polymerize. The same result was obtained when the charcoal was cooled to −78° C. or heated to 250° C.

The vapours were also passed over finely shredded rubber in order to absorb some of the carbon disulphide. The brown solid was formed as usual, but the amount of carbon disulphide deposited with it was distinctly less. It therefore appears that a partial separation of the transforming ozoniser product from the disulphide has been effected.

A complete separation of the two substances seems to be for the present out of the question on account of the rapidity with which the ozoniser product polymerizes in the solid state at low temperatures.

ANALYSIS OF THE SOLID PRODUCT.

A sufficient quantity of the brown solid was collected (0·1127 grm.) to allow of the estimation of sulphur in it,

which gave	S = 71·8 per cent.,
CS requires	S = 72·7 per cent.

Further, the ratio C : S was determined in another specimen collected from a number of experiments in which the condenser had been cracked and shattered. This specimen was probably contaminated with minute fragments of glass and also contained moisture. The material gave on analysis :—

S = 61·0 per cent., C = 24·6 per cent., and ratio C : S = 1 : 0·854,

or, allowing for the 5 per cent. of water present as impurity,

C : S = 1 : 0·93.

The substance produced in the manner described has the composition of carbon monosulphide, and it seems justifiable to conclude that, under the influence of the silent electric discharge, carbon disulphide has, like carbon dioxide under similar conditions, been dissociated, in this case into sulphur and gaseous carbon monosulphide, and that the latter can polymerize with the evolution of a considerable amount of energy to form a brown solid even at the temperature of liquid air. It is the remarkable instability of the solid or liquid gas at low temperatures that is so characteristic and renders the physical and chemical examination so difficult.

CHEMICAL REACTIONS OF THE OZONISER PRODUCT.

The action of a number of substances on the ozoniser product was studied by placing the substances to be examined, either mixed with or deposited on asbestos in a U-tube, and allowing the vapours to pass over them.

In this way it was found that the passage of the ozonizer product was prevented by finely divided platinum, nickel, or silver.

Ferric oxide (not ignited), yellow mercuric oxide, and silver oxide were found to react with the ozoniser product to form sulphides of the metals ; ferric oxide is much more efficient than the other two.

Solid caustic potash, silver nitrate, lead acetate, and sodium peroxide seemed to have little or no action on the ozoniser product, while barium peroxide reacted with it, forming barium sulphide.

The most striking reaction observed is that which takes place with concentrated sulphuric acid. After some experiments had shown that, as used, the vapour of carbon disulphide was not attacked by sulphuric acid, the ozoniser product was allowed to pass over the surface of sulphuric acid placed in a wide U-tube. The sulphuric acid rapidly acquired a yellow colour, changing gradually into deep orange-red, then became turbid, and deposited a yellow solid ; brisk effervescence occurred during the whole time, and even after several hours none of the ozoniser product escaped the action of the acid. The yellow solid which separated from the sulphuric acid was found to be sulphur.

The properties of the brown solid produced from carbon disulphide, which has the composition of carbon monosulphide, have been compared with those of the polymeric form of carbon monosulphide obtained from the interaction of thiophosgene and nickel carbonyl, and the results indicate that the difference between the brown substance obtained by the action of the electric discharge on carbon disulphide vapour and the polymeric form of carbon monosulphide obtained from thiophosgene is merely in the degree of polymerization.

The Fullerian Professorship of Chemistry was endowed by John Fuller in 1833 as a recognition of the great work of Faraday. He held the office for thirty-four years, and if the lecturer is spared for a few months he will be able to look back on the same long period of service. Some member of the younger generation of chemists or physicists will soon have to continue the work of research at the Royal Institution.

[J. D.]

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 26, 1912.

THE RIGHT HON. LORD RAYLEIGH, O.M. D.C.L.
LL.D. D.Sc. F.R.S., in the Chair.

PROFESSOR BERTRAM HOPKINSON, M.A. F.R.S. M.Inst.C.E.

The Pressure of a Blow.

THE scientific analysis of a blow requires first the determination of the actual pressures or forces set up between the colliding bodies, and second an investigation of the distribution of these pressures and of their physical effects. The pressure produced by a blow does not differ in kind from that produced by any other agency, such as an hydraulic press, but it differs in degree because of its great intensity and of its extremely short duration, and these characteristics, as we shall see, have a marked influence on the effects which it produces.

The first part of the problem, that is the calculation of the pressure in tons or pounds, is based on the familiar principles of mechanics which were first precisely stated in Newton's laws of motion. The cause of the pressure is the rapid change of motion of the colliding bodies which occurs when they come into contact, and, according to Newton's second law, the force is simply proportional to the rate at which this change is effected. The rate of change may be measured in terms of energy and distance, or in terms of momentum and time. Thus, a hammer head, moving at a rate of 16 feet per second, and weighing 1 lb., possesses 4 foot-lbs. of energy, because its velocity could have been acquired by falling freely through 4 feet. If it strikes a nail and drives it one-eighth of an inch, the energy which was generated by the weight of 1 lb. acting through 4 feet is destroyed in $\frac{1}{400}$ part of that distance, and the force necessary to effect this change of motion is 400 times as great, say 400 lbs. The same effect would be produced by a 4-lb. hammer striking with the velocity which would be acquired by falling through 1 foot, namely 8 feet per second. Regarding the same instance from the point of view of momentum, the 1-lb. hammer would take half a second to fall 4 feet, and the quantity of motion or "momentum," reckoned as the product of the force acting into the time required to generate it, would be one-half of a pound-second unit. While driving the nail in, the hammer covers a distance of $\frac{1}{8}$ inch with a velocity which starts at 16 feet per second and drops to zero. To cover the distance of $\frac{1}{8}$ inch with the average velocity of 8 feet per second takes $\frac{1}{800}$ of a second, which is

$\frac{1}{400}$ of the time ($\frac{1}{2}$ second) which it takes the weight of the hammer-head (a force of 1 lb.) to generate its motion. Thus the pressure required for the rapid stoppage is as before, 400 lbs.

We may take another instance essentially similar to the hammer and nail, but differing greatly as regards scale. A 14-inch armour-piercing shell weighs about 1400 lbs., and when moving at 1800 feet per second possesses about 31,000 foot-tons of energy, or about 15,000,000 times as much as our hammer-head. Such a shell would just pierce a plate of wrought iron $2\frac{1}{2}$ feet thick, and the average force which must be exerted to pull it up in that distance, which is of course the pressure which it exerts on the plate, is 30,000 divided by $2\frac{1}{2}$, or about 12,000 tons. This is equivalent to some 80 tons on the square inch.

When a hammer strikes a nail, the force acting during the blow is practically constant, and the average value obtained as above by dividing the energy by the distance moved, or the momentum by the time taken, is equal to the actual force exerted throughout the impact. In many cases, however, this force is not constant, and it is then necessary to divide the course of the impact into short intervals either of space or of time, calculate the change of energy or momentum in each, and add the result. A familiar instance is that of two billiard balls. We may suppose one ball to strike the other full with a velocity of 16 feet per second, which corresponds to a fairly hard stroke. It simplifies the consideration of the problem, if instead of one ball moving and the other at rest we suppose them to be travelling in opposite directions with equal velocities of 8 feet per second. At the instant when the balls first touch there is no pressure between them, but as they continue to approach, each flattens the other at the point of contact. The balls no longer touch at a point, but over a circular area which rapidly increases in diameter. Corresponding to any given amount of flattening or distance of approach, there is of course a definite pressure which might be measured by actually squeezing the balls together under known forces and measuring the corresponding amount of approach. Or the relation between pressure and distance could be calculated as was done by Hertz. The results are shown in the curve (Fig. 1), and the area of the curve connecting pressure and distance up to any point gives the number of foot-pounds of energy destroyed. When this is just equal to the original energy of the balls they will have been reduced to rest, and in the case supposed, the distance of approach is then $\frac{14}{1000}$ of an inch, and the total pressure between them 1300 lbs. This pressure is distributed over the circle of contact which is $\frac{1}{6}$ of an inch in diameter, and the average intensity of the pressure is 27 tons per square inch. The distribution, however, is not uniform, the pressure at the centre being $1\frac{1}{2}$ times the average. [The shape of the balls at the moment of closest approach was shown on a drawing with a magnification of 100.] The balls are then like compressed springs, their original energy of motion having been completely transformed into strain energy in their substance. The reason of the high intensity of pressure developed is that this strain energy is concentrated into a very small volume of ivory near to the point of contact. The balls then begin to separate, and the whole process of compression is gone through in reverse order, the strain energy being transformed back into energy of motion by the pressure. Finally the balls rebound unstrained, with nearly the velocity with which they approached.

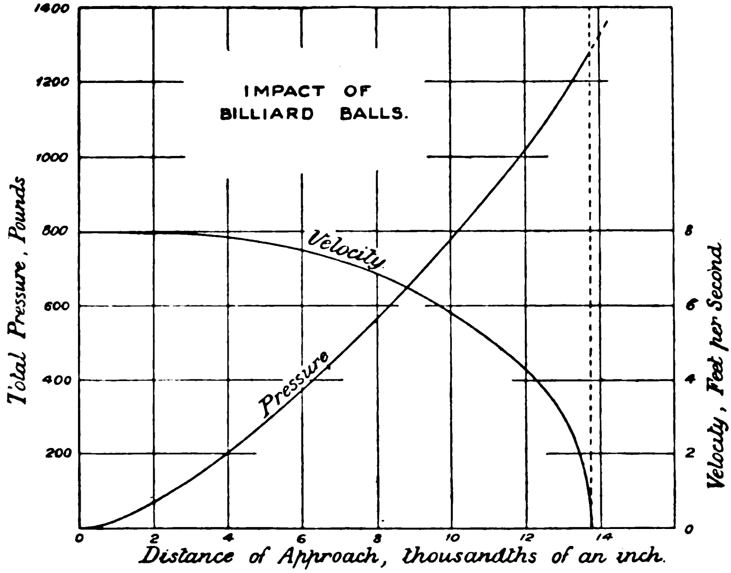

Fig. 1.

If for the ivory balls we substituted hollow balls of steel having the same mass, the pressure produced by the blow would be greater, because the steel is much more rigid than ivory and gives less under a given force. Thus the distance of approach is less, the circle of contact smaller, and the maximum intensity of pressure much greater. It reaches 280 tons per square inch averaged over the surface of contact. Such a pressure could only be sustained without permanent effect by a very hard steel. Ordinary mild steel would begin to flow when the pressure passed about 100 tons, a permanent flat would be left by the blow, and the balls would rebound with less velocity than that of approach. The theory whose results I have given does not, of course, apply to such a case, as it depends on the assumption of perfect elasticity.

It is rather remarkable that materials can sustain without injury such large pressures as are produced by these blows. Mild steel balls are not crushed perceptibly till the pressure reaches 100 tons per square inch; yet a short column of the same steel would be crushed by a pressure of 30 tons per square inch. One reason is the extremely short duration of the pressure—it has no time to produce much effect. The other is the fact that in the blow it is accompanied by large lateral pressures exerted by the metal surrounding the area of contact. Pressure equal in all directions, such as is exerted by the water at the bottom of a deep ocean, produces generally no permanent effect on solids or liquids. To produce breakage or permanent deformation there must be difference of pressure in different directions, and the most important if not the only factor determining whether such breakage or deformation shall occur is the amount of the difference. If for example, our column of mild steel, which in the absence of lateral support begins to crush at 30 tons, were surrounded by a jacket exerting a radial pressure of 30 tons, it is probable that the end pressure might be increased to 60 tons without any movement occurring. In the impact of balls the metal surrounding the point of contact by resisting the lateral expansion of the compressed part, sets up radial pressure of this kind. It can be shown, in fact, that the lateral pressure at the centre of the circle of contact corresponding to a maximum normal pressure of 100 tons per square inch is 75 tons per square inch, leaving 25 tons effective for producing deformation or breakage. The greatest difference of pressures, however, is not at the centre of the circle of contact but at points near the circumference of that circle. Thus, as was found by Hertz, fracture commences by the formation of a circular crank of small radius surrounding the point of first contact.

These calculations of pressure are based on theory, and it may be asked what direct experimental evidence we have that the theory is correct. It is not, of course, possible actually to measure the pressures over the minute circle of contact between the balls, nor is it possible accurately to measure the amount of the flattening. We can, however, pursue the calculation a little further, and determine the time during which the balls are in contact from the moment when they first touch to the moment at which they separate on the rebound. In the case of billiard balls moving with a relative velocity of 16 feet per second, this time is $\frac{1}{4000}$ of a second. A precisely similar calculation can be made for balls of steel or other metal, and it is not difficult to measure in the laboratory the time during which such balls remain in contact. The method is of considerable use in connection with impact problems, and it consists in making the two balls, by their contact, close a galvanometer circuit in which there is also a battery and resistance. A certain quantity of electricity, which is simply proportional to the time of contact, then passes through the galvanometer and produces a proportionate deflection in it. It has been found that the time of contact measured in this way for steel balls is exactly that predicted by theory, and it may be inferred that the theory is correct in all its details, and that the pressure calculated by its aid corresponds with the facts. This method was first used by Pouillet in 1845, and has recently been brought to great perfection by Mr. J. E. Sears,* who showed, among other things, that the relation between pressure and deformation of steel is almost exactly the same when the pressure is applied for an excessively short time, as in the case of impact, as it is when applied steadily, as in a testing machine. The assumption that this is the case lies of course at the root of the calculations, and its verification was therefore a matter of considerable importance.

When one billiard ball strikes another the effect of the blow is practically instantaneously transmitted to every portion of the colliding balls, or, to speak more precisely, the time taken to transmit the pressure is short compared with the total time of contact. Except for the minute relative displacement near the point of contact the balls move as a whole, every part having the same velocity at each instance of time and coming to rest at the same moment. In many cases of impact, however, and in those possessing the most interest from a practical point of view, this is by no means the case. We may consider, for instance, the impact of an elongated lead rifle bullet against a hard steel plate. Under the enormous pressures developed lead flows almost like water, and in the absence of lateral support it is as little capable of transmitting those pressures. Thus, when the nose of the bullet strikes, the metal thus brought into contact with the plate immediately flows out laterally, its forward motion being destroyed, but the hind parts of the bullet know nothing of what has happened to the nose because the pressure cannot be transmitted to them, and they continue to travel on with the original velocity until they in their turn come up to the plate and have their momentum destroyed. The process of stopping the bullet is complete when its tail reaches the plate, and the time required is simply that taken by the bullet to travel its own length. Thus a Lee-Metford bullet is 1¼ inch in length, or, say, $\frac{1}{10}$ of a foot, and if moving at 1800 feet per second, which is about the velocity given with a rifle, it would be stopped in $\frac{1}{18000}$ of a second. The bullet weighs approximately ·03 lb., and possesses with this velocity about 1·7 lb. second units of momentum. The force required to destroy this in $\frac{1}{18000}$ of a second is 18,000 multiplied by 1·7 lb., or, say, 15 tons. This acts over the sectional area of the bullet, which is $\frac{1}{14}$ of a square inch, giving a pressure of about 210 tons per square inch. This is the average pressure throughout the impact, but the pressure is probably nearly constant. It is to be noted that the pressure per square inch depends only upon the velocity (varying as its square), and not upon the length or diameter of the bullet. Increase in diameter only alters the area over which the pressure is applied, and increase in length the time during which it is applied.

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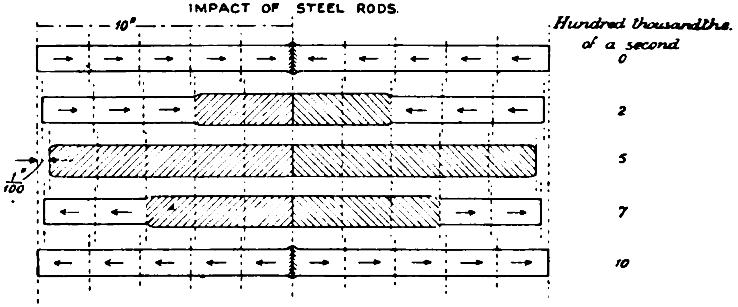
* Camb. Phil. Soc. Procs. vol. xiv. p. 257.

If for the bullet of lead we substitute one of hardened steel which will not flow, the problem at once becomes much more complicated. In order to reduce it to its simplest terms, and to bring the theory into such a form that it can be tested in the laboratory, we may suppose that instead of the bullet we have a cylindrical steel rod, say $\frac{1}{2}$ inch in diameter by 10 inches long, with flat ends, and that it strikes quite fair against an absolutely unyielding surface. The latter condition could not be fulfilled in practice, because there is no substance more rigid than steel. So far as the effects on the rod are concerned, however, it can be fulfilled by making two rods, moving with equal velocities in opposite directions, collide end on; and this device has been used in the laboratory for imitating the effect of impact against an unyielding surface. We have to consider how long it takes to stop the rod under such conditions. When the end first strikes it is pulled up dead, just as in the case of the lead bullet, only it does not now flow out sideways. The pressure, however, set up at the end of the rod cannot be instantaneously transmitted through it, and consequently the hind parts do not at once feel this pressure, but continue to move on as before. The pressure travels with the velocity of sound, which for steel is about 17,000 feet per second, and it takes accordingly $\frac{1}{20000}$ part of a second before the pressure has been transmitted throughout the 10 inches length of the rod. The course of the impact is shown in the diagram (Fig. 2). A wave of pressure is initiated at the first contact and travels along the rod. At any instant the part of the rod which has already been



Portions of rod in compression and at rest shaded.
Relative velocity at impact 34 feet = 400 inches per second.
Pressure about 18 tons per square inch.

FIG. 2.

traversed by the wave will be at rest and in compression (as indicated on a greatly exaggerated scale by the shortening and thickening on the diagram), while the remainder which has not yet been reached by the wave, and accordingly as yet knows nothing of the impact, will still be moving forward with the old velocity. Each section continues to move on until the wave reaches it, when it is stopped with a jerk, the sections thus pulling up successively until the whole rod is at rest, which happens when the wave has travelled to the free end. From the momentum of the rod, and the time taken to stop it, the pressure can be calculated by the use of the principles already illustrated. Thus a rod 10 inches long is stopped, as we have seen, in $\frac{1}{20000}$ second, and if it be moving with the moderate velocity of 17 feet per second, the pressure required to pull it up in this time is 13 tons per square inch. This pressure is constant throughout the impact, and it is obvious that here again the intensity of pressure is dependent only upon the velocity and not on the weight of the rod. For if with the same velocity the length is increased, the corresponding increase of momentum to be destroyed is cancelled by the greater time required for the transmission of the pressure wave, and if the area is increased, the total pressure is merely increased in proportion, the pressure per unit area remaining the same. For a hard elastic body the pressure is proportional to the velocity, a principle which is probably generally applicable in the initial stage of all impacts.

At the instant of greatest compression, when the rod is reduced to rest, it is like a compressed spring, and there being no pressure acting at its free end to keep it compressed, it proceeds to expand again. Starting at the free end a wave of expansion travels down the rod, the several portions being successively jerked into motion with approximately the original velocity. The whole process of restoring motion to the rod is completed when this wave reaches the impinging end, when the rod rebounds as a whole with the original velocity. The whole time of contact is then that taken by a wave of sound to travel twice the length of the rod. Here, again, by electrical measurement of the time of contact, it is possible to check the theory. It is found that the actual time is longer than that predicted. This is due to the fact that one cannot in practice make the rods hit absolutely true all over the ends; they strike at one point first, and the metal near that point has to be flattened out before the ends come into contact all over and initiate the simple plane pressure wave of the theory. The complete analysis of the discrepancies between theory and experiment so caused was long a puzzle to physicists interested in these matters. It was finally effected by Mr. J. E. Sears, who determined mathematically the corrections necessary on this account, and submitted his theory to experimental test with entirely satisfactory results.*

Another simple instance of the propagation of waves along rods illustrates a point of importance in regard to the general effect of blows. Instead of maintaining the pressure during the whole passage of the wave up and down, as in the end-on impact, a pressure is suddenly applied to one end, maintained for a short time, and then removed. A corresponding pressure wave travels along the rod. Each portion of the rod is only stressed or in motion during the passage of the wave over it, and after the passage of the wave it is left with a certain forward displacement, but without any velocity or stress. Furthermore, the whole momentum of the blow is concentrated in the short length of the rod covered by the wave. On its arrival at the other end the wave is reflected, but the reflected wave is a wave of tension. As it comes back the head of the tension wave is at first wholly or partially neutralised by the tail of the pressure wave, but after a time it clears this, and the rod is then put into tension of amount equal to the original pressure. If there be a crack or weak place in the rod at a sufficient distance from the free end, the pressure wave will pass over it practically unchanged; but on the arrival of the reflected tension wave the rod will part, because the crack cannot sustain the tension, and the forward part will move on, having trapped within it the whole momentum of the blow. The rest of the rod will remain at rest and unstrained (Fig. 3).

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* Camb. Phil. Soc. Trans. vol. xxi. p. 49.

[The propagation of waves in rods was illustrated by means of a model, consisting of horizontal wooden bars fixed at equal intervals to a vertical wire.]

The fact that a blow involving only pressure may, by the effects of wave action and reflection, give rise to tensions equal to or greater than the pressure applied, often produces curious effects. I shall choose by way of illustration some observations which I have been making recently, and which I think are new. I have here a small cylinder of gun-cotton. By the use of a small quantity of fulminate in the hole provided for the purpose it is possible to detonate the gun-cotton, which means that in an excessively short time it is converted into gas at a very high temperature. The time required is probably only 3 or 4 millionths of a second, and is so excessively short that the gas does not during the process expand appreciably into the surrounding atmosphere.

Thus the gas generated, which, when completely expanded, will fill a space several thousand times as great, is for a minute fraction of time confined within the volume of this small fragment of gun-cotton. This confinement implies great pressure, how much is at present a matter of doubt. I understand that Sir Andrew Noble estimates it at 120 tons per square inch. The only thing which restrains the expansion of the gas is the inertia of the surrounding air, and the pressure accordingly drops with very great rapidity. It is probable that the pressure is practically gone after $\frac{1}{30000}$ of a second. The same pressure is, of course, exerted by the gas upon any surface with which the gun-cotton is in contact, and it will be seen that the force so produced has the characteristics of a blow, namely, great intensity and short duration. If such a cylinder of gun-cotton weighing one or two ounces be placed in contact with

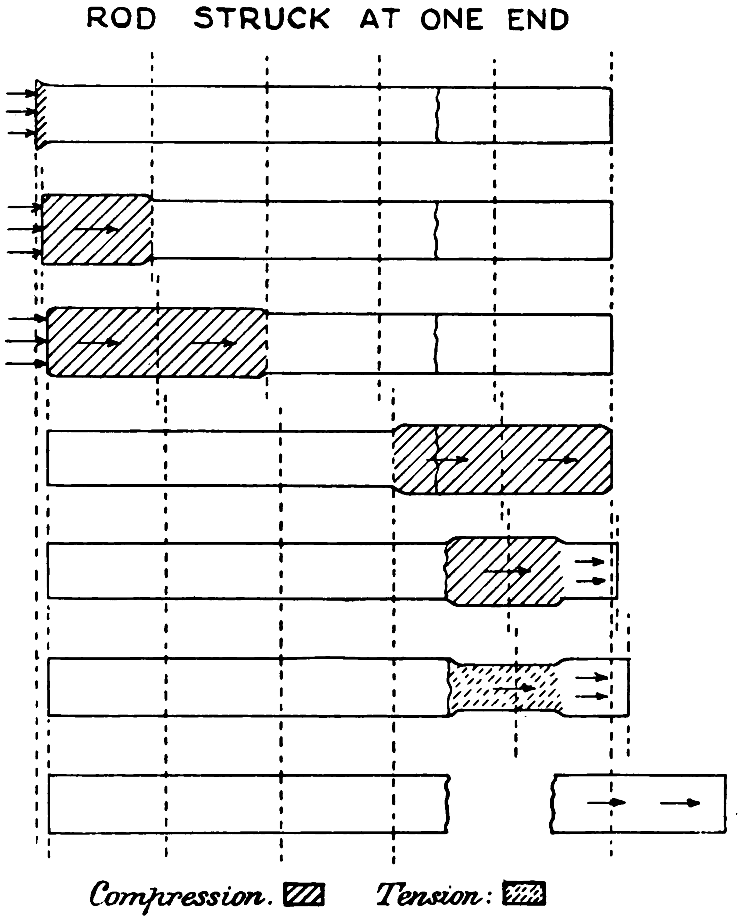


Fig. 3.

a mild steel plate, the effect, if the plate be half an inch thick or less, will be simply to punch out a hole of approximately the same diameter as the gun-cotton, just as though it had been struck by a projectile of that diameter. But if the plate be three-quarters of an inch thick, the curious result which I exhibit here is obtained (Fig. 4). Instead of a complete hole being made, a depression is formed on the gun-cotton side of the plate, while on the other a scab of metal of corresponding diameter is torn off, and projected away with a velocity sufficient to enable it to penetrate a thick wooden plank, or to kill anyone who stands in its path. The velocity in fact corresponds to a large fraction of the whole momentum of the blow. The scab behaves much in the same way as the piece which we saw would be shot off the end of a rod struck at the other end if the rod were divided or weakened, so as to be unable to sustain the reflected tension wave. The separation of the metal implies, of course, a very large tension, which can only result from some kind of reflection of the original applied pressure, but the high velocity shows that this tension must have been preceded by pressure over the same surface, acting for a time sufficient to give its momentum to the scab.

Wishing to ascertain how and where the separation originates, I caused a two-ounce cylinder of gun-cotton to be detonated in contact with a somewhat thicker plate. In this case no separation of metal was visible ; the only apparent effects being a dint on one side and a corresponding bulge on the other. On sawing the plate in half, however, I was gratified to find an internal crack, obviously the beginning of that separation which in the thinner plate was completed (Fig. 5).

The pressure exerted by the gun-cotton in the experiments which I have just described is practically confined to the circular area of contact between it and the metal, as is shown by the accurate agreement of the print on the plate with that circle. The effects of that pressure must, however, be largely conditioned by the fact that the metal upon which it acts is attached to the surrounding portions of the plate, and is held back by them. In order to get an idea of the effect of this factor, I have tried the experiment of removing this outside metal, leaving the steel cylinder opposed to the gun-cotton. If such a short cylinder of steel be placed in contact with a gun-cotton cylinder of equal diameter, the result of detonation was at first sight merely to flatten it out slightly, and to produce a depression on one side with something of a bulge on the other. No external crack was visible. But on sawing the piece in half a remarkable system of cracks was disclosed (Fig. 6) ; the cracks spread in all directions, as though tension had been acting in every direction ; in fact, it appeared as though the steel cylinder had begun to burst. The tension necessary to produce these cracks, which, as you will see, must have radial as well as axial components, must originate in some kind of wave action which follows the blow. The problem is very complicated, and I have not yet succeeded in finding a full explanation of the phenomenon ; but there cannot be much doubt that the longitudinal tensions are due to a wave generally similar to that which we have been discussing in connection with the rod. To account for the radial tensions which the cracks show also to have

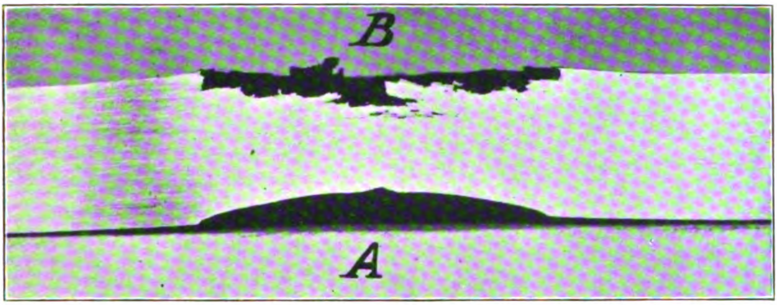


Fig. 4.—Section of Mild Steel Plate, $\frac{3}{4}$ inch thick, after Detonation of Gun-Cotton in Contact with the Side *A*. Scab Torn off from *B*.

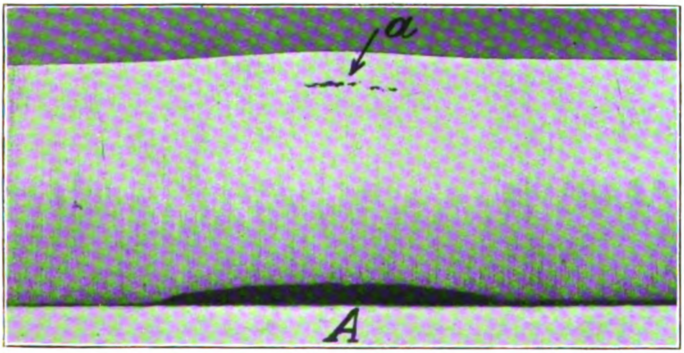


Fig. 5.—Section of Mild Steel Plate, $1\frac{1}{4}$ Inch thick, after Detonation of Gun-Cotton in Contact with the Side *A*. Crack Formed at *a*.

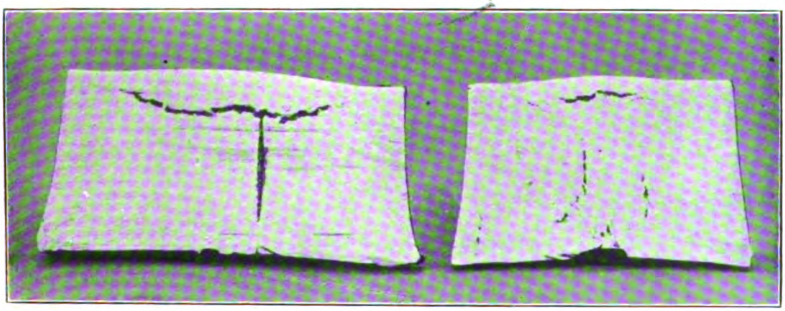


Fig. 6.—Axial Sections of Steel Cylinders, after the Detonation of Gun-Cotton in Contact with one End.

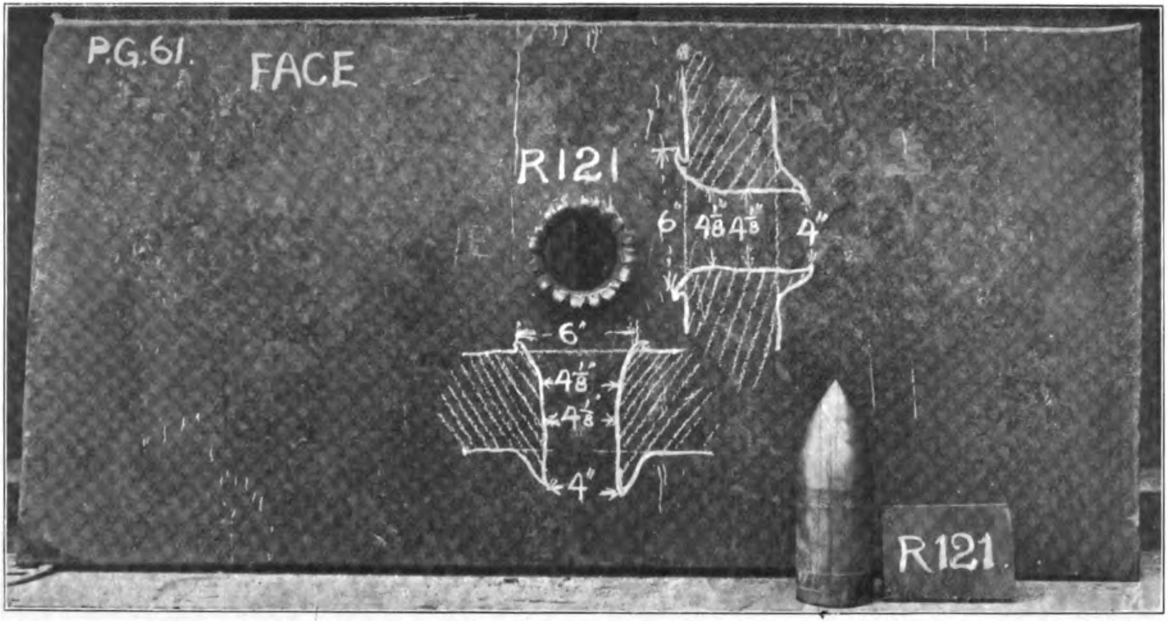


Fig. 8.

been present, it is to be observed that the shortening of the cylinder in the direction of its axis, which is the immediate effect of the blow, must be accompanied by a corresponding increase in diameter. This increase takes place very rapidly, and implies that at first the metal is moving out in a radial direction with a high velocity. The stoppage of this radial motion requires radial tension, and probably this is greater at points near the axis for much the same reason that when a stone is dropped into a pond the circular waves which it causes have their greatest amplitude at points near the centre of disturbance. In the case of the steel cylinder the radial tension wave travels inwards from the surface, and its amplitude increases as its goes in.

I have recently been attempting to measure the duration of the pressures produced by the detonation of gun-cotton. The principle of the method may be made clear from the diagram showing the effect of a blow on one end of a rod (Fig. 3). A wave of compression travels along the rod, the length of the wave corresponding to the time during which the pressure has acted; that is, it is equal to the velocity of sound, multiplied by that time. We may assume that the time was $\frac{1}{20000}$ of a second, which would give a wave just 10 inches long. This wave travels to the end of the rod, is there reflected as a wave of tension, and comes back. If the rod be cut across, the surfaces of the junction being accurately faced and in firm contact, the pressure wave will pass the joint without change, but on the arrival of the head of the tension wave at the joint, the parts will separate and the end piece will fly off. If the tail of the pressure wave has then cleared the joint, the separated end-piece will have trapped within it the whole momentum of the blow, and the part left behind will remain at rest and unstrained. In the case supposed things will happen in this way if the end-piece is more than 5 inches long. If it be less than 5 inches long, say 4 inches, there will, on the arrival of the reflected wave at the joint, be still 2 inches of pressure wave in the other part of the rod, and the corresponding quantity of momentum. In this case, therefore, only a portion of the whole momentum is trapped in the piece, the balance being left in the other part of the rod, which moves forward with the corresponding velocity. In order to discover how long the pressure lasts it is only necessary to try a series of experiments with the joint at different distances from the free end. It will be found that if that distance exceeds a certain amount, the rod which was originally struck remains at rest, the whole momentum being transferred to the free end-piece. If the distance be less, only a fraction of the momentum is so transferred, and the balance remains in the struck rod, which accordingly moves forward.

I have applied this method to investigating the curve of pressure developed by the detonation of gun-cotton. I will not weary you with the details, but the results, so far as I have obtained them, are shown in the curve (Fig. 7). This exhibits the pressure produced by the detonation of one ounce of dry gun-cotton on the end of a steel rod, distant three-quarters of an inch from it. With the gun-cotton hard up against the steel the pressures are probably about twice as great, and the curve similar in form. It is, of course, impossible to do this experiment within the walls of this lecture room, the explosion, while not very violent, is sufficiently so to necessitate its being carried out in the open. But I can readily show you a very similar experiment, in which the pressure is produced, not by the detonation of gun-cotton, but by the impact of a small rifle bullet. [Experiment shown.]

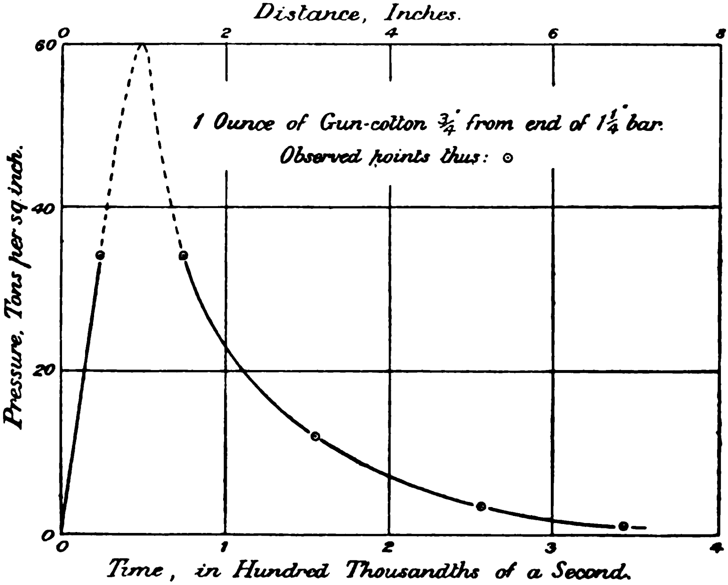


Fig. 7.

I have on the table some specimens to show the effects of detonating larger quantities of gun-cotton. Here is a steel plate which has been broken by firing a charge of about 1 lb. in contact with it. It is interesting to note the character the fracture produced. This plate is a good quality of mild steel, such as is used for making boilers. It would be possible by a steadily applied pressure to bend it double without fracture, yet as the effect of the blow delivered by the gun-cotton it is broken with very little bending, almost as though it were cast iron or very hard steel. Time will not permit of my going further into the interesting question, of course a very important one in connection with our subject, of the effect on the character of the fracture produced of very big stresses lasting for a very short time. This case of the fracture of mild steel by gun-cotton shows, however, that one result may be that the property of ductility largely disappears under the action of a sufficiently violent blow. The mild steel, in fact, behaves very much like sealing-wax, or pitch. The stick of sealing-wax which I hold in my hand has been bent by the continued action of a small force acting for several days, and there can be no doubt that the same force, had it continued to act, would ultimately have bent it double without breaking it. Yet under the application of a force many times as great, it snaps like a piece of glass.

The pressures produced by the detonation of gun-cotton are of the same order of intensity as those developed in ordinary blows. We saw that in the impact of billiard balls the average pressure over the area of contact may reach a value of 27 tons per square inch, and with steel balls moving at quite small velocities, such as 2 or 3 feet per second, it is easy to get pressures of 100 tons per square inch or more. These pressures, however, are very local, the area over which they act being a few hundredths of an inch in diameter only. By means of gun-cotton similar pressures may be applied over any desired area; but the intensity is no greater. About 120 tons per square inch is probably the limit of simple static gaseous pressures produced by known practical explosives. Probably greater pressures are produced with fulminate, but that cannot be used except on a very small scale. For the production of destructive effects on hard steel greater pressures than this are required, and in order to develop them on any considerable scale we must again have recourse to the dynamic action of collision.

We have already seen that a lead bullet moving at 1800 feet per second probably generates a pressure of 200 tons per square inch or more. We went on to consider the impact of rods of hard metal, and it appeared that two rods of steel colliding end on with a relative velocity of 34 feet per second would develop a pressure of about 13 tons per square inch over the whole section of either. The theory on which that conclusion is based has been subjected to experimental test —indirect, it is true, but sufficiently searching—and is certainly correct for velocities and pressures of that order. According to the theory the pressure is simply proportional to the relative velocity of the two rods, so that if they collided at 2000 feet per second, that is sixty times as fast, the pressure would be 780 tons per square inch, assuming that the theory continues to hold under these very different conditions.

One of the fundamental assumptions on which the theory is based, however, would certainly break down long before such a velocity was reached. That assumption is that the pressure leaves no permanent effect on the material. I do not know what is the strongest steel for this purpose which has been produced, but I think it may safely be asserted that no known substance would stand an end compression, such as results from the blow of the colliding rods, of more than 300 tons per square inch. If it were ductile it would flow so rapidly under this pressure that there would be appreciable deformation even in the very short time during which the pressure lasts. If it were very hard it would be instantly shattered. In both cases the circumstances of pressure transmission would be completely altered. It is, however, fairly certain that in neither would the pressure exceed that calculated on the hypothesis of perfect elasticity, and that in both it would be greater than that calculated (as for the lead rifle bullet) on the hypothesis of no elasticity.

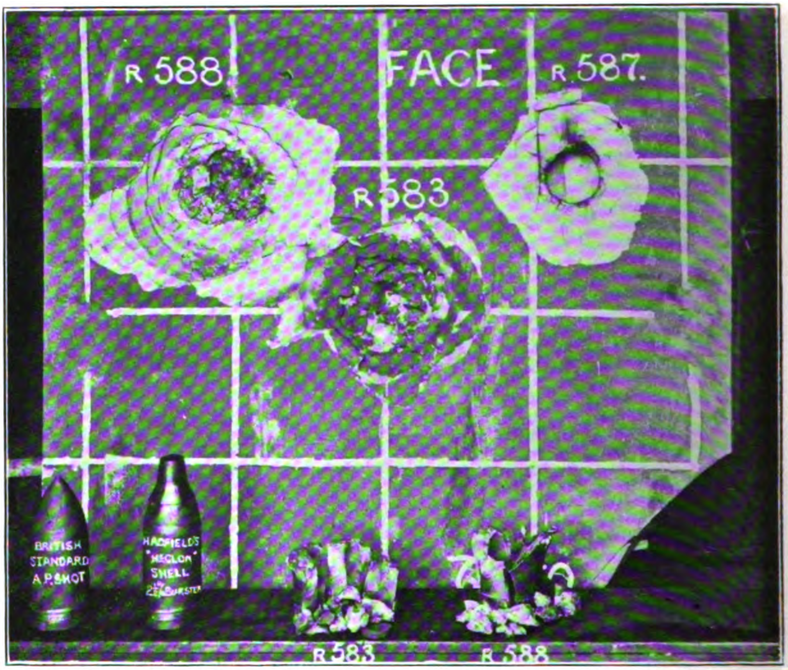
I am afraid, therefore, that at present our theories can throw but little light on the interesting question of the pressure developed when a hard steel armour-piercing shell strikes a hard steel plate with a velocity of 2000 feet per second. But a consideration of the visible effects of such a blow is suggestive in many ways, and by the kindness of Sir R. Hadfield I am able to describe and show some of them to you to-night.

You see before you specimens of modern armour-piercing shot, and their essential features are shown on the drawing. The shell is made of a special steel of great strength and considerable ductility, and after manufacture the point is hardened by thermal treatment, the base and most of the body of the shell remaining more or less ductile. In recent years it has become the practice to fit a cap of soft steel over the hardened point. I will speak of the functions of this cap later, and for the present we will consider the shell without it.

I first show the effect of firing an uncapped shell at a plate of wrought iron or mild steel (Fig. 8). In this case the metal of the plate is so soft that pressures that are quite without effect on the hardened point of the shell are able to make it flow very rapidly. The shell simply ploughs its way through, pushing out the wrought iron before it, and emerges quite unscathed. It will be noticed that on the striking side there is a rim or lip of wrought iron which has been squeezed out in a direction opposite to the movement of the shell. A similar lip is formed if a hole is blown in a lead plate by means of a gun-cotton primer, and there seems to be a good deal of analogy between the two cases.

Completely to stop a 14-inch shell, such as that which you see before you, would require a thickness of at least 2½ feet of wrought-iron, and almost as great a thickness of mild steel. I believe that some ships twenty-five years ago were fitted with armour of this sort of thickness, but, of course, the weight is almost prohibitive. Modern improvements in armour, whereby the same effective resistance is obtained with less than half the thickness, are based on the use of special steel having sufficient ductility to enable it to be worked and fixed in place on the ship while possessing greater strength than

1. TRIAL OF HADFIELD'S "ERA" STEEL HARD-FACE
2. TRIAL OF HADFIELD'S "HECLON" 10·5 C.M.



TEST OF PLATE.

1. Round 583. Hadfield's Cast Steel A.P. Shot (British Standard) Uncapped.

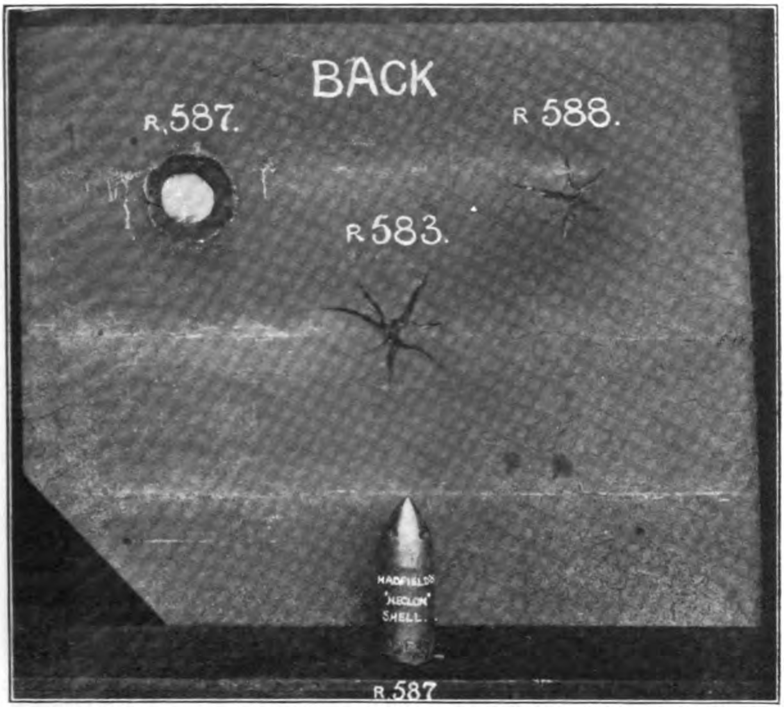
Striking Velocity.	Energy—Foot-tons.	Factor of Penetration
2010	920	2.32

Round 588. Hadfield's Cast Steel A.P. Shot (British Standard) Uncapped.

Striking Velocity.	Energy—Foot-tons.	Factor of Penetration
1920	840	2.18

ARMOUR PLATE, 4 FT. × 4 FT. × 4½ INS.

(4½ IN.) ARMOUR-PIERCING SHELL OF 32½ LBS.



TEST OF PROJECTILE.

2. Round 587. Hadfield's "HECLON" Cast Steel A.P. Shell, Capped.

Striking Velocity.	Energy Foot-tons	Factor of Penetration
1920	840	2.18

No. 872.

wrought-iron or ordinary structural steel. Even such a special steel, however, is handicapped as against the shell by the hard point of the latter, which is able to force the softer material aside though itself undamaged. This disability has been overcome by hardening the face of the plate so that it now possesses the structure indicated in the drawing, the back being tough and ductile, but the face as hard as it is possible to make it. When such a plate is struck by the shell it is a case of Greek meeting Greek, and this is the result (Fig. 9, rounds 583 and 588). Both the shell and the hardened face of the plate are shattered by the pressure, sufficient of which is transmitted through the substance of the plate to crack it right through, though, of course, none of the shell has penetrated it.

It would seem that when it acquired the hard face the armour plate more than over-took the shell in the race. Though the shell might by sheer energy pierce a somewhat thinner plate, I am told that it was apt to be smashed to pieces in the process. The balance has of recent years been more than restored by the addition to the shell of the soft steel cap. I have already shown you the effect of firing an uncapped shell; I will now draw your attention to that of firing the same shell with cap at the same plate (Fig. 9, round 587). The shell goes through minus its cap, but otherwise so completely uninjured, that I am told it might in many cases be used again. It punches a clean hole in the plate. The fate of the cap is interesting. The shell punches a hole in it, as of course it must do before it reaches the plate, and the cap forms a ring, which is held up by the plate and through which the shell passes. The fragments of the cap are found on the front side of the plate, and in some instances they have been collected and put together, forming a ring. I have one such ring here. Its largest diameter is that of the shell, its smallest about an inch less, and it looks as though the ring had got intact as far as the shoulder of the projectile, but had then burst into several pieces.

The usual explanation of this remarkable effect of a soft steel cap is that it supports the point of the projectile. As I pointed out in connection with billiard-balls, the destructive effect of pressure depends on the difference of pressures in different directions, and not on their absolute amounts, and it is obvious that by the exercise of a sufficient lateral pressure the point might be completely protected. The difficulty is to see how the comparatively weak material of which the cap is made can exert the very large pressures which are necessary for effective support. It seems hardly possible that such pressures could be generated by the mere act of stretching or expanding the cap over the end of the shell. If this be so, the inertia of the metal in the cap must play an important part. At the critical moment when the hard point of the shell meets the plate, there is a sudden distortion of the shell and plate near the point of contact. This distortion is the cause of breakage. One can see that the mass of mild steel surrounding the point of the shell, and pressed into firm contact with it, might by its inertia oppose a powerful resistance to this sudden change of form, and so support the shell during the minute fraction of time which determines whether it or the plate shall go.

[B. H.]

WEEKLY EVENING MEETING,

Friday, February 2, 1912.

Sir James Crichton Browne, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Sir James Mackenzie Davidson, M.B. C.M. *M.R.I*

Vital Effects of Radium and other Rays.

In the year 1902 it was my privilege to deliver a discourse in this place on "*X*-rays and the Localisation of Foreign Bodies." On this occasion we shall consider the influence of these rays on living tissues and pass in review, so far as time permits, the effect of various forms of radiation upon living matter generally.

Heat Rays.—If we are to adopt the chronological order in which these radiations were discovered and applied, we shall have to consider in the first place the effects of light and radiant heat. It is common knowledge that exposure to heat rays will produce changes in living cells, and, if sufficiently intense, will lead to their destruction. In the same manner light also will cause changes in living tissue. In the case of light and heat rays there is no conveyance of material from the source to the object affected. All that occur are wave-motions, set up in the ether of space, or, as it is sometimes called, the "luminiferous ether." One of our greatest statesmen, the late Marquis of Salisbury, who had such a profound knowledge of science, described the ether which fills all space as "the nominative of the verb 'to undulate'." It is the cardinal fact that ether undulates and that waves are produced within it. Isaac Newton thought that light consisted of corpuscles shot out from the sun into space at an enormous velocity, but now it is known that light is produced by waves in the ether, although it may be mentioned in this connexion that as we proceed to deal with radium we shall find that corpuscular projections proceeding nearly up to the velocity of light really do exist.

Some of the waves which are produced in the ether, impinging upon the living tissues, produce effects in a marked degree. Others do not seem to have any effect upon them at all. The rays which affect living tissues include heat waves, some of the light waves, *x*-rays, and some of the rays of radium. On the other hand, the long waves produced in space-telegraphy—the so-called wireless telegraphy—have no such appreciable effect. A powerful and constant magnetic field is apparently without influence upon living cells, although Professor Silvanus Thompson has shown that if this magnetic field made rapidly alternating, physiological effects will be produced. By placing the head between the coils through which powerful alternating currents are passing, the observer will experience a flickering sensation of light. The sensation of taste may also be stimulated in the same manner. Professor Thompson suggests that these effects are due to eddy currents set up in the nerves, and this seems highly probable.

Light Rays.—We may forego any further reference to heat rays, and proceed at once to consider which of the rays of light have most effect upon living matter. When a narrow beam of white light is split up into its primary rays, a spectrum such as can be seen on the screen is produced. In nature we are familiar with this phenomenon in the shape of the rainbow. If, now, a strip of bromide paper is stretched along the screen so as to receive the length of the spectrum, the action of the light will take place upon its surface and can be revealed by development in the ordinary way. On this exposed paper being brushed over with developer, the affected parts will be found gradually to darken, and it will be observed that the darkening takes place more especially under the blue and violet rays, and even beyond the violet end of the visible spectrum.

[Strip of bromide paper across spectrum, develop and fix, to show monochrome equivalent.]

These have been called the chemical rays, and it is precisely these rays which are most readily absorbed in their passage through matter. The difference between the absorption of the various rays may be observed in the case of the sun revealed through a fog. Yellowish at first, the sun becomes red as the fog increases in density, the reason being, that the red waves alone are able to get through the fog-bank, while the violet and the shorter wave-lengths generally are scattered.

[Glass cells before lantern, filled with hypo solution, with hydrochloric added, to show the absorption of the blue and violet rays.]

It is the shorter waves, at the violet end of the spectrum, which have vital effects upon the tissues. They are the cause of sunburn—a fact, which is well observed in the higher reaches of the Alps, where the atmosphere, being comparatively free from suspended matter, the blue, violet and ultra-violet rays get through so richly that, without any special sensation of heat, severe effects are produced upon the skin. If the ground be covered with snow, the rays are reflected, and a more intense action results. It is interesting, further, to remember that the blue and violet colour is the last to be seen at the close of the day; that when a person has his sight temporarily impaired by over-indulgence in tobacco he loses in the centre of his field of vision the perception of red and green but rarely that of violet; and that, in cases of blindness coming on gradually from wasting of the optic nerve, blue or violet is the last colour to go. Many years ago Sir James Dewar, in conjunction with Professor McKendrick, demonstrated that when light fell upon the retina of the eye an electrical current was generated in the optic nerve.

Finsen Light Treatment.—For a long period advantage was taken of the physiological effect of sunlight in the treatment of certain diseases, but Finsen, of Copenhagen, was the first to make a systematic study of the curative properties of light. The obvious drawback to this method of treatment is that sunlight is not always available. It is evident that if we were to depend upon sunlight in London for the treatment of certain diseases, cures would indeed be tardy. Finsen, therefore, adopted the method of using powerful artificial lights, and in this manner he soon discovered that the most effective rays were those of the violet and ultra-violet. He used the electric arc for the purpose, and, as ultra-violet light is intercepted by glass, he employed quartz lenses. The readiness with which these active rays were absorbed rendered them only useful in superficial diseases, and it was found that even blood intercepted them to so great an extent that treatment could be made more effective by introducting compression, whereby the blood was forced out of the area upon which the light fell. Finsen's results were very brilliant. More recently, a quartz mercury-vapour lamp has superseded the arc for the production of active violet light in large quantities. Its action upon living cells is very powerful, and it is most destructive, even after short exposures, to all forms of bacteria. But its ready absorption confines its influence to the production of surface effects, and it fails to reach deeply. The effects of light upon plant life are well-known, but the limited time does not permit us to deal with the subject. A familiar instance is that of a plant grown in the dark, which is deprived of its colouring matter.

X-rays.—We pass now to the consideration of the Röntgen or *x*-rays. It was in 1895 that Professor Röntgen's discovery that a Crookes tube emitted rays, which would pass through opaque substances as readily as light passes through glass, created immediate and world-wide interest. The fact that these rays could pass through our bodies and reveal the shadow of our bones so absorbed the interest of the early workers in this field as to mask for a time the terrible effects upon our living cells which followed over-exposure. The precise nature of the *x*-rays has not yet been ascertained. It is highly probable, as the late Sir Gabriel Stokes suggested, that they are impulsive in ether of extremely short wave-length. As they travel in straight lines from their point of origin into space, and are not affected in their course by a magnet, and can be neither diffracted nor refracted, their wave-length cannot be determined.

X-ray Burns.—The burning effect of *x*-rays upon the skin was in a certain sense discovered accidentally. The early tubes used for the production of *x*-rays were rather imperfect, and therefore very long exposures had to be given in order to obtain radiographs. In this way many severe burns were produced upon unfortunate patients. The operators themselves also began to suffer; and in this country we were all shocked by the untimely death of Dr. Blacker, of St. Thomas's Hospital, who was a most enthusiastic pioneer in *x*-ray investigation, and whose death was directly due to the destructive effect of *x*-rays upon the tissues. It was a sad instance of a young and promising life being cut short by this new, and, as it has ultimately proved, beneficial agent, before we had learned to guard against its dangers.

It is worthy of note that most, if not all, *x*-ray burns produced in operators began in the uncovered parts of their skin, such as the hands and face. A good deal of doubt still exists as to whether the primary *x*-rays alone are responsible for these manifestations. Having suffered from chronic *x*-ray burn in my hands, especially in my right hand, it seemed to me rather remarkable that the area of trouble at the back of the hand should end sharply at a line corresponding to the usual position of the coat cuff, for cloth is quite transparent to the *x*-rays; and the adjacent parts of the skin beneath the sleeve were, in my own case, equally exposed with the uncovered hand itself.

Secondary Rays.—Many views have been brought forward to explain the causation of some *x*-ray burns, without any very definite results, and it appears probable that the secondary or indirect rays given off from the surface of the glass may be, if not in some cases the primary factors, certainly largely contributory to these superficial skin burns.

The existence of these secondary and less penetrating rays can be very readily demonstrated by exciting a Crookes tube in the ordinary way, and suspending opposite the point from which the primary rays emerge a mass of lead, through which no primary or direct *x*-rays could possibly penetrate. Naturally, a shadow of this lead is cast by the *x*-rays coming from a fine point in the anode, and which we may at present call the primary rays; but within this eclipsed area we get shadows caused by other rays, and when these are traced they can be shown to be produced on the glass of the tube, which fluoresces green. These rays are much more richly produced in what is called a high or hard tube. I demonstrated them in my lecture here on April 25, 1902.

Mr. Campbell Swinton alludes to the existence of these rays in a paper on "The Source of the Röntgen Rays in Focus Tubes," published in 1898. He describes them as secondary rays from the green fluorescing glass of the *x*-ray tube, and has taken pin-hole photographs of them.

As a further confirmation of the possibly vital effect of these rays upon the skin, I may mention that Freund found that a tube so high as to give no fluorescence on the screen caused the hair to fall out, and also that with a tube having the electric current passed in the reverse direction, so as to produce only very weak primary *x*-rays, similar results were obtained. It would be interesting to construct a tube so as to employ, for therapeutic purposes, these secondary rays alone.

I have made comparative measurements of these rays by means of the fall of the gold-leaf electroscope. The simple instrument called an electroscope, which is merely a strip of gold leaf or aluminium leaf attached to a support, is discharged by the action of *x*-rays, which makes the air a temporary conductor, and it is a most convenient method to measure the degree of conductivity produced, the time taken by the gold leaf to fall through a given distance being taken in each case. Measured by the fall of the electroscope leaf, I found that with a "high tube" giving very penetrative *x*-rays, if the action of the primary rays were taken as 1, the action of the secondary rays would be $\frac{1}{2}$, and that with a low tube, or a tube giving *x*-rays of a low order of penetrability, if the primary rays were again taken as 1, the second rays would be $\frac{1}{7}$.

Professor Silvanus Thompson several years ago showed that the cathode stream, after impinging upon the target and thus giving rise to the primary *x*-rays, was reflected and impinged upon the glass walls of the tube, causing a green fluorescence. He called these reflected cathode rays paracathodic. Whether they produce *x*-rays upon this second impact or not does not appear to have been proved, but as Barkla and Sadler and others have demonstrated that *x*-rays outside the tube, impinging upon solid matter, give rise to secondary rays, it seems certain that *x*-rays, in passing through the walls of the tube in which they are generated, must give rise to secondary *x*-rays, and it may well be the case that the green fluorescing glass of an *x*-ray tube emits two sets of *x*-rays—one, as we have mentioned, produced by the primary *x*-rays in their impact on passing through it, and the other possibly by reflected cathodal rays. Be that as it may —and this is a matter for the physicist—I feel sure that their physiological action upon the skin must be considerable, especially as they are much more readily absorbed than the primary *x*-rays. In illustration of this, I can show you a photograph which has been produced by them. As far as I am aware, the physiological importance of these rays seems to have been entirely overlooked, and I am sure they present a field worthy of immediate investigation.

[Secondary rays from glass of tube shown by fluorescent screen, the primary rays having been cut off by a mass of lead.

Diagram 1, showing production of secondary rays from tube.

Diagram 2, showing how these rays may produce photographs.

Pinhole photographs. Photographs of different layers of tinfoil by means of the primary and secondary rays.

Show *electroscope*, and describe by means of which a comparison of the primary and secondary rays has been made.]

Protection.—Many years ago I made an experiment in which a Crookes tube was completely buried in a large quantity of red lead contained in a box, and when this was excited in a dark room the fluorescent screen showed that no *x*-rays at all were able to penetrate. Then a small opening was made, by scooping away the red lead until the primary rays got through, together with only a very few of the secondary rays from the small area of glass opposite the orifice. This was the most effective means of screening everybody from the rays except the individual under observation, but it was highly inconvenient, the apparatus being difficult to handle, especially when a fresh tube had to be embedded. Therefore a box was constructed, lined with a mixture of red and white lead, and a small hole was cut in it for the emission of the rays. Some such method is now fairly generally adopted, and is most important for the protection of the workers.

When a fluorescent screen is used for examining patients, it should be lined with thick lead-glass. This allows the shadows of the screen to be perfectly observed, while at the same time protecting the faces of the observers from the injurious effects due to the *x*-rays. Since these precautions have been taken the disasters to workers have been minimised, and should be entirely abolished. The opacity to the *x*-rays of any material is in proportion to its atomic weight. Therefore any material impregnated with a compound having heavy atomic weight would act as an effective protection.

Pastilles.—Great difficulty was experienced at first in the use of *x*-rays owing to the variability of the tube. No reliable means existed of knowing what dose—to borrow a term from medicine—was being given. At present we have several fairly reliable methods which have led to more precise results without bringing about the unfortunate injuries previously occurring. The method most in general use is that of the Sabouraud pastille, which consists of barium platino-cyanide in compressed form—the same material as that which is employed in the making of the fluorescent screen.

The pastille changes gradually in tint from a pale yellow to a deep orange, according to the amount of *x*-radiation it receives. It is arranged in a definite position, and by careful experiment sufficient data have been forthcoming to enable any desired biological result to be obtained with approximate precision. The great advantage of this method is that it can be used at the very time that the *x*-rays are being applied to the patient, so that whatever variations may take place in the output of the tube all of them are recorded, so to speak, by the tint produced in the pastille. With these safeguards the *x*-rays, applied for medical purposes, may now be said to be entirely free from all risk to the patient.

Penetrability.—Unlike the violet and ultra-violet rays, the *x*-rays penetrate deeply—can, in fact, pass right through the body easily—but they vary in penetrability according to the condition of the Crookes tube. The tube may be in a condition which is called "soft" or "low," so that the rays produced will cast a shadow of the hand without showing the bones at all, or it may be "hard" or "high," so that the rays, passing easily through, make the bones look quite pale on the fluorescent screen; and there are all intermediate stages.

I have already said that the opacity of a substance to the *x*-rays is in proportion to the atomic weight of that particular substance. Recently Dr. Lindemann has made a glass of light atomic weight which, when put in the tube as a window, allows many rays of very low penetration, which in ordinary tubes are prevented from escaping beyond the glass, to pass through. Already very valuable therapeutic results have been obtained by the use of these tubes.

[Show Lindemann tube.]

X-ray Effects on Cells.—The most striking and general vital effect of the *x*-rays is to be witnessed upon young and growing cells. Guinea-pigs a day or two old are killed with great rapidity, and before any visible effects of radiation in the shape of burning make their appearance. The adult animal, on the other hand, shows a greater capacity for resistance. In a recent communication to the Royal Society, J. F. Gaskell, who has studied the action of the *x*-rays on the developing chick, stated that he had found this action to be confined to a lowering of the mitotic (or cell-multiplying power) of the growing tissues. If this diminution were not too great complete recovery occurred, and the chicks were hatched out at the usual time, but if it fell below a certain degree, all further development was arrested. On the same principle of attacking young and growing cells, the *x*-rays injure the hair follicles and bring about the fall of the hair—a convenient method of epilation now employed with signal success in the treatment of ringworm in children. Prolonged exposure to the rays will produce total baldness. The sweat glands can also be destroyed in the same manner. The action of *x*-rays upon the blood is limited chiefly to the white blood corpuscles, the red blood corpuscles being very resistant. The central nervous system also, fortunately, has great resisting capacity.

Latent Period.—When the skin is exposed to the *x*-rays, no immediate visible effect is produced. But if the duration of exposure has been sufficiently prolonged to bring about what is called a reaction, the period intervening between the time of application and the appearance of the reaction in the skin will be found generally to vary between a couple of days and about three weeks. The duration of this "latent period" depends upon the amount of the dose. The stronger the dose, the shorter the latent period. There is no doubt that the *x*-rays produce certain changes or injuries to living cells, which remain hidden until the absorption of the abnormal products due to these changes causes redness and inflammation to become evident. At first it was thought that these effects were confined to the skin, but it soon became apparent, on researches by Haenisch and others, that some of the deep organs were more susceptible to the action of *x*-rays than the skin itself. The most sensitive of all the tissues are the lymphoid tissues generally, especially the spleen, which shrinks and becomes strongly pigmented under their attack.

Plant Growth.—In considering the effect of *x*-rays upon the lower forms of life, we have first to note their influence upon plant growth. Wetterer, the German radiologist, relates an interesting experiment in this connexion. He has taken collections of sunflower seeds and applied *x*-ray doses of varying strengths. The seeds which had received very small doses, equal to or slightly above a dose which would produce an inflamation of the skin, sprouted in about the same time as seeds which had been untouched by the rays. In seeds which had received heavier doses the sprouting took place much later, and the plant showed clear symptoms of degeneration. Seeds which had received still stronger doses gave no sign of growth whatever. The effect of *x*-rays upon bacteria, however, seems to be very feeble, if, indeed, it exist definitely at all. And this is in marked contrast to the bactericidal action of ultra-violet light previously mentioned.

Skin Diseases.—It is evident that an agent having such vital effects as those described—and there are many other effects which could be demonstrated if time permitted—will find a wide field of application in the treatment of disease. In many skin diseases it is of signal service. In certain diseased conditions of the spleen also its action is very marked. In malignant tumours, while it may be of service in arresting the rapidity of the growth, it cannot be looked upon as a method of cure, although, after the removal of such growths by operation, the application of the rays to the involved area may assist in destroying any of the malignant cells which the surgeon's knife has missed, and preventing re-development. And as the technique of the *x*-rays is being improved, the field of their utility in therapeutics is being gradually extended.

Radium.—We may now proceed to consider the action of radium and other radio-active substances. It is necessary before approaching the question of the vital effect of these rays to review briefly their physical properties. Immediately after the discovery of the *x*-rays, in

1895, Henri Becquerel proceeded to investigate the possibility of fluorescent substances giving out invisible rays similar to those of Röntgen. Very fortunately, as it turned out, he selected uranium for his purpose. Wrapping a photographic plate in black paper, so that it was completely shielded from all light, he placed upon it a salt of uranium, his idea being that the fluorescence induced by the light in the uranium compound might give rise to invisible rays like those from a Crookes tube. He found that the photographic plate was, indeed, blackened underneath the salt, but that such an effect was produced equally well without the uranium being exposed to light. In short, it became evident that the property of emitting these invisible rays was a property inherent in the uranium element itself. Further investigation proved that these rays from uranium, like the *x*-rays, had the power of discharging electrified bodies.

Radio-Activity.—This discovery of Becquerel was really the discovery of that which we now know as radio-activity. The term "radio-active," as applied to elements which spontaneously emit invisible rays that possess the power of passing through opaque substances, has now come into the general vocabulary. The rays of radio-active substances also have the power of discharging electrified bodies, of acting on a photographic plate, and of making certain substances fluoresce. They cause air and other gases to become temporarily conductors of electricity, and, finally, they generate heat spontaneously.

[Show pitchblende.]

Becquerel's discovery was also the starting-point for the investigations of the Curies, which resulted in the separation by chemical processes of the various active substances in a large quantity of residues of the mineral pitchblende. This mineral was found to be more radio-active than the uranium it contained. Madame Curie, after separating a substance which she called *polonium*, succeeded ultimately in isolating the substance which is now known as radium, and which proved to be two million times more radio-active than a corresponding amount of uranium. Radium, chemically considered, is an ordinary element, very similar to barium or calcium and producing similar salts. The sulphate, chloride, and now more generally the bromide of radium, are the salts used. Metallic radium has been isolated by Madame Curie.

[Show calcium which resembles Radium.]

The Radium Atom.—Up to the time of the discovery of radium the atom had been looked upon as indivisible. Rutherford and Soddy found that radium was disintegrating, and Sir William Ramsay, with Mr. Soddy, investigating this disintegration of radium, proved that helium was one of its products. This discovery may be looked upon as the first actual proof of the transmutation of elements. Chemists had long been familiar with the energy evolved by the rearrangement of molecules, such, for example, as in the explosion of dynamite, but the enormous energy evolved by the disintegrating atom was found to be almost incredible. There is, however, this marked difference, that while we have control over the molecular combinations, we have no control whatever over the disintegration of the atom, which is a purely spontaneous action. This disintegration proceeds at the highest temperatures exactly in the same way as has been proved to be the case in the lowest temperatures. The beautiful researches of Sir J. J. Thomson have helped us to realise that an atom consists of a large number of corpuscles. Soddy has stated that the amount of heat evolved by any quantity of radium every hour is just about as much as is required to raise an amount of water equal in weight to the radium from the freezing-point to the boiling-point. The quantity of radium obtainable is so small, being only one part in five millions of the best pitchblende, that it has never beeen experimented with as a drug in the ordinary sense to any extent, and its biological properties in this respect are consequently unknown.

It is the radiant energy evolved by the disintegration of the radium atom which is of value to us in medicine. The average life of radium is 2500 years, so that anyone who buys radium can have no personal anxiety about its lasting qualities. So far as we are concerned this evening we have to consider the three rays, called Alpha, Beta and Gamma rays, which radium gives off.

In the process of flying to pieces, or disintegration, the radium atom first gives off an atom of helium, at a velocity of about twenty thousand miles per second. This is called the *alpha* ray, which is beautifully demonstrated by a little instrument devised by Sir William Crookes and called the spinthariscope. The remainder of the atom evolves as a gas or "emanation." The emanation in its turn decomposes, losing half its energy in about four days, and finally gives rise to an active deposit of rapid change. In its turn also this deposit gives off alpha, beta and gamma rays, and these are the three kinds of rays, together with the emanation, which are of importance in the consideration of the vital effects of radium. It is very probable that the ultimate product of this disentegration is lead.

[Show spinthariscope.]

Alpha Rays.—The alpha ray, as already mentioned, consists of an atom of helium travelling with a comparatively small velocity. It has a very feeble power of penetration, being stopped by two or three centimetres of air, while a sheet of paper will suffice to prevent it from issuing. The action of the alpha rays on living cells is uncertain.

From some experiments with the "active deposit" from thorium, I think that the action of these rays upon the skin must be very slight.

Beta Rays.—The beta rays consist of what are called corpuscles, being extremely minute material particles about one thousand times smaller than the alpha ray, travelling at varying velocities from half that of light, or less, almost up to the velocity of light itself, which is 186,000 miles per second. They carry a negative charge of electricity, and their power of penetration varies with their velocity, but all of them are stopped by lead one centimetre in thickness.

Gamma Rays.—The gamma rays, which are almost certainly a wave disturbance in the ether, are similar in that and in other respects to the *x*-rays, but they are of much greater penetrability, passing easily through one centimetre of lead. No Crookes tube, even with the highest vacuum, could give rays as penetrative as the gamma rays produced spontaneously by radium.

[Show sparkler burning.]

Emitting as it does these three kinds of rays, radium will affect a photographic plate, cause a diamond to shine in the dark, give a sensation of light to the human eye, and make barium platino-cyanide and willemite fluoresce. It has definite biological effects, and it will also discharge an electroscope. I have already described the electroscope, which enables us to measure the strength of a particular specimen by comparing the rate of fall of the leaf with the similar action in the case of some standard quantity.

> [Show radium tube, containing 20 mgr. of pure radium bromide. Show also radium lent by Mr. Francis Fox.
>
> Slide showing magnetic deviation of alpha and beta rays.
>
> Slide : Different penetrability of α, β, γ rays.
>
> Slides : (1) The "A" slide, showing the feeble penetrability of the alpha rays, and the absence of effect on a photographic plate when a piece of paper was interposed.
>
> (2) Slide showing the darkening of a plate exposed to gamma rays through a lead box one centimetre in thickness.
>
> Effect of radium on platino-cyanide screen and willemite.
>
> Discharge of electroscope under radium rays.]

Reaction.—Becquerel's accidental discovery that the presence of a tube of radium in the waistcoat pocket brought about a severe burn on the skin, demonstrated the physiological effect of this substance, and it only remained for the medical profession to find out in what cases such action would prove beneficial, and in what manner radium could be best applied. The effect upon the skin appeared to be very similar to that of the x-rays. At the time of the radium application no sensation is experienced, nor is anything abnormal observed for a varying period afterwards. In this respect the phenomenon which, in the case of the x-rays, has been referred to as the "latent period," is repeated. A certain amount of redness and discomfort—the latter in the form of a feeling of heat or burning—then make their appearance. This is the radium reaction, and its severity depends upon the duration of the primary dose. One essential difference between the x-rays and radium is that while the quality of the rays from a Crookes tube varies considerably from time to time, the output from the radium is quite constant, and therefore the duration of the application is the important factor in its use.

Biological Effects.—The biological effects of radium have been closely studied upon a large variety of organisms. Its influence upon plant development has been observed, among others, by Koernicke, who found * that the general result of the exposure of seedlings was to cause retardation and eventually cessation of growth of stem and root. In some cases the growth was resumed after an interval; in others, the plants never recovered. The first effect of the radium rays on dry bean and turnip seeds was to accelerate germination, but after a time the beans ceased to develop, although the turnips showed no signs of arrest—doubtless they would have done so had the exposure been prolonged. A tube of radium placed on a leaf will cause the irradiated area to lose its chlorophyll and to assume autumn tints.

Micro-Organisms.—A considerable amount of research has been carried out upon micro-organisms. Anthrax microbes have been found not to develop at all if left for 24 hours in an atmosphere charged with radium emanations. In Metchnikoff's laboratory at the Pasteur Institute recently † it has been proved that certain toxins tend to lose their virulence after being made radiferous. When diphtheritic toxin was left for thirty days in contact with minute quantities of radium sulphate, the poisonous effect of the toxin was found to be much less rapid than in the case of the same toxin which had not been treated in this fashion. An interesting exception was noted in the tetanus toxin, upon which radium seemed to have no retarding influence.

The virus of rabies has been speedily destroyed when exposed directly to radium emanation, and Phisalix ‡ has demonstrated the sensibility of certain snake poisons to its action. Cobra poison, which is distinguished by its resistance to destruction by heat, is readily destroyed by the radium radiation, and the observer found later that the virus of the viper can be destroyed in the same manner by the emanation. Not only so, but the radium which destroys the poison can also destroy with equal rapidity its antidote, quickly reducing the active properties of the anti-venomous serum of Calmette.

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* Himmel und Herde, Oct. 1904.

† Archives d'électricité médicale, Sept. 25th, 1911.

‡ Academy of Sciences, Paris, February 22nd, 1903.

Fish and Mammals.—In aquatic life the effects of radium have also been investigated. Professor Coleridge Farr, of Christchurch, New Zealand,* testing the artesian wells of that country, has found a destructive effect upon young fish and developing spawn in water containing radium emanation. Coming to the mammals, we find that radium exercises a rapid destruction upon the epidermis and the derma of young animals. In the early period of radium experiment it was discovered that, as in the case of the x-rays, young animals were killed far more readily than adults. Danysz † demonstrated this greater sensibility of the tissues, especially the epithelial tissues, of the young. According to this same observer also, under identical conditions of exposure to the action of radium rays, the skin of guinea-pigs is much more sensitive than that of rabbits, and a treatment sufficient to produce in the case of the guinea-pig a very extensive sore will only accelerate the growth of a rabbit's hair. Mice have frequently been made the subject of radium experiments. They have been found to be paralysed and to die after twenty days of uninterrupted exposure to radium placed at three inches distance from the mica window of their cage.

In all such cases it is worthy of remark that paralysis and death have been found to be due to internal hæmorrhages. The nerve cells have not shown any appreciable alteration. The central nervous system in all cases is, indeed, very resistant to radium action, but it suffers indirectly from the effects upon the vascular tissues.

Own Cases.—A wide field was opened up to radium, once its vital effect on living cells was demonstrated. The date of my own first application of radium to the treatment of disease was May 1903. The case was a large rodent ulcer, just below the right eye, which was rapidly progressing in spite of x-ray treatment. Two glass tubes, each containing five milligrams of radium bromide, were applied tentatively for a short time to the upper border of this ulcer, and the application was cautiously repeated in the course of two or three days. The improvement was so manifest that the tubes were applied in the same manner over the general surface of the ulcer, which finally healed perfectly without scarring, and has remained well during the nine years that have since elapsed.

The success encouraged me to try this method in the treatment of other cases of rodent ulcer, and with equally striking and satisfactory results. While incapable of effecting a cure in certain severe and old-standing cases, their progress was often arrested in a marked manner, and considerable improvement of the condition was attained.

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* British Medical Journal, Oct. 14th, 1911.

† Academy of Sciences, Paris, December 28th, 1903.

Spring Catarrh.—Radium has also proved to have a markedly specific action upon a troublesome disease of the eyelids known as "spring catarrh." This occurs in young people, the upper eyelids more particularly being covered with rough tesselated granulations. The disease was quite incurable until I applied radium to a little boy sent to me by Mr. Arnold Lawson in February 1906, and with the use of radium every case treated during the six years has been completely and painlessly cured. A matter also worthy of remark is that from the commencement of the treatment, even before any appreciable improvement is visible, the patients express themselves as feeling the eyes much more comfortable, and they are able to use them in their ordinary occupations during the period of treatment.

["Spring catarrh" case of Dr. Dawson Turner, of Edinburgh. Right eye treated by this method, other eye not. Notice difference. Case got well.]

In external diseases of the eye, radium has also proved to be of the greatest use.*

Cure of Chronic X-rays Dermatitis.—A further indication for radium-therapy was discovered in the case of *x*-ray dermatitis. It was my misfortune to suffer from a chronic manifestation of this trouble, and three or four years ago a burnt patch on my head became ulcerated. One portion was so threatening as to suggest malignancy, which is, unfortunately, a common result of these lesions. As nothing that was tried would effect a cure, it became a question of excision, but before resorting to this I applied radium in glass tubes, and was agreeably surprised to find that it completely cured the condition.

The tube contained 20 milligrams of pure radium bromide, and was left in position for 25 minutes. Nothing happened for twelve days, and then there occurred a certain amount of swelling and redness, the size of the black crust increased, and serum exuded from the side. This gradually subsided, and when the crust peeled off the normal healthy thin skin was found to be beneath it.

Slide 1. Left hand with spot of *x*-ray dermatitis.
2. Same hand, with radium tube in position for treatment.
3. Hand showing reaction.
4. Hand cured, spot having disappeared.
5. Very severe case of *x*-ray dermatitis.
6, 7 and 8. Dr. Hall Edwards's hands.]

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* *Vide* Lawson and Mackenzie Davidson paper.

Apparatus.—For convenience of application, those who possessed a large quantity of radium spread it on surfaces protected by varnish. This enabled a large area to be treated, and, as the material interposed between apparatus and screen was slight, it allowed a considerable quantity of radiations to pass and impinge upon the skin. Dr. Dominici, of Paris, was the first to use dense metals, such as lead and platinum, also gold and silver, for screening purposes. By this means the less penetrating beta rays were stopped, and only the gamma rays and beta rays of high penetrability were able to traverse the filter. Thus prolonged exposure became possible without injury to the skin, and the deeper parts that it was desired to affect could readily be reached. By means of these metallic filters, which can be made of different material and of different thickness, rays of any desired penetrability can be employed.

Embedding Radium.—A further procedure in the adaptation of radium to the treatment of tumours was to enclose the substance in small tubes of metal, such as platinum, and, an opening having been made deeply into the centre of the tumour, to lodge the tubes in that position, and allow them to remain embedded there for varying periods. Sometimes this has been combined with outside applications, so that the tumour has been attacked at once by radium rays from within and from without. Dr. Abbé, of New York, has made openings in this manner into malignant tumours, and has placed in position small celluloid tubes, into which the radium can be let down, and which can be easily removed, if desired, without any inconvenience to the patient.

Superficial Diseases.—A wide field has been found for the use of radium in the treatment of superficial diseases. It is very difficult to suggest a limit to the powers of the substance in this respect. Some diseases, of course, yield more readily than others. Lupus, which is tuberculosis of the skin, is very resistant to radium treatment, but a great many other skin diseases, as well as *small* cancers of the tongue and lip, can be cured, while large, rapidly-growing tumours, like sarcoma, can be destroyed by the method of burying tubes within them. Here, again, the fact is illustrated that these rays seem to concentrate their attack upon the young and most rapidly growing cells. That is probably why sarcomas are so vulnerable to attack when compared with carcinomas, which are of slower growth. But in spite of its greater potency and convenience in application, the same thing has to be said of radium as was said of the *x*-rays, that it cannot in any sense be looked upon as a cure for cancerous growths of large size. It will inhibit the growth of these tumours and destroy them locally, but rarely completely, and it does nothing to prevent the usual secondary deposits.

[Rodent, and malignant wart on face. Dominici's and own cases, illustrating the effect of radium.]

Radium Emanation.—From the radium itself we pass to its emanation. The emanation is a heavy gas which can be dissolved in water and be injected into the tissues, or can be confined in a metal tube and buried, the effect being practically the same as with radium itself. The emanation gives us 75 per cent. of the energy which would be obtained if the radium from which it comes were embedded in its stead. The emanation, of course, can also be swallowed in solution and inhaled, and it is now much used in this manner. Several natural waters, such as those of Bath and other spas, contain radium emanation.

Emanation Effects.—The manifestations of the emanation are much the same as those of the radium itself, and its paralyzing effect upon young animals is practically identical. When introduced into the system by inhalation, by injection, or by swallowing, the emanation seems to have a capacity for stimulating the body ferments. This holds true, probably, of many ferment actions in the body, although there are others in which the emanation appears to have no demonstrable influence. The excretion of sugar by diabetics, for instance, is stated to remain unaffected. In a lecture before the Berlin Medical Society, a year ago,* Professor Wilhelm His, after dealing with cases of chronic rheumatism and uric acid gout, a large proportion of which had shown improvement under radium-therapy, said that, in 15 cases out of 18 under the influence of the radium emanation, the blood lost its uric acid within a few weeks. He pointed out that emanation behaves like every other gas which is not a normal constituent of the body, and is excreted to a very slight extent through the kidneys and to a large extent by means of the breath.

[Radium emanation standard.]

Sir William Ramsay has very kindly given me a "Standard" for the emanation. This little bottle contains $\frac{1}{1000}$ mg. of pure radium.

Selective Action.—I may be allowed to add a word or two about the action generally of the radium in the body. In the diseases for which the rays possess curative properties their action is extraordinarily selective, so that, if the dosage is well-timed they destroy the abnormal cells without destroying the normal. To find out in any given case which are the rays to use is an important matter, of course, for investigation, and as knowledge in this direction becomes more precise, the cures that may be set down to radium will be extended and made more certain.

Other Radio-active Substances.—The great disadvantage in this field of research is the scarcity of radium. As the knowledge of its utility is diffused, the demand for it increases, which seems to indicate that it must always remain exceedingly scarce and of a forbidding price. Naturally, therefore, those interested in its therapeutical properties are anxious to know whether among other radio-active substances there may be any which can take its place, and perhaps be more easily available to the profession, and, through them, to the public. Uranium, thorium and actinium are radio-active, and have all been suggested in this connexion, but they are too feeble to have real efficacy. The discovery by Otto Hahn of a substance known as meso-thorium, which is one of the disintegration products of thorium, and was found in the course of an attempt to separate radio-thorium directly from thorium, has aroused considerable expectation.

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* January 18, 1911.

Meso-thorium is obtainable from the residues in the preparation of thorium for the manufacture of incandescent gas mantles. It emits the same rays as radium, and, weight for weight, is more powerful. As the supply of thorium is much larger than that of pitchblende, from which thorium is obtained, we may hope to have a larger supply of meso-thorium. It will cost much less, but, on the other hand, it has a short life of only about seven years, as against radium which lasts 2500 years. It is being experimented with medically in Germany. I applied the tube now shown, which is equivalent to 10 milligrams of pure radium, to a chronic patch of x-ray dermatitis on my hand, and a reaction followed 15 days after application, and the result promises to be favourable.

[Show meso-thorium.]

Thorium Emanation from Radio-Thorium.—Radio-thorium gives off thorium emanation richly. This is a heavy gas, lasting 76 seconds, giving off alpha rays in profusion. These rays, impinging upon a sulphide of zinc screen, cause it to glow or fluoresce. Sulphide of zinc crystals sparkle and emit light when mechanically crushed or disturbed, and therefore the alpha rays, striking these crystals, cause them to give out light. A remarkable spinthariscope effect is observed when the screen is viewed with a magnifying glass.

The emanation of radium will act like that of thorium, but, instead of lasting only seventy-six seconds, it will lose half its strength in about four days. Thorium emanation also resembles that of radium in giving rise to an "active deposit." This becomes concentrated on the negative pole in an electric field. A metallic surface can in this way be made intensely radio-active, giving forth the alpha, beta and gamma rays. The "active deposit" from thorium emanation lasts for several hours.

[Show thorium emanation from radio-thorium and screening effects. Blow emanation into sulphide of zinc screen and also blow through long tubes.]

Conclusion.—There is a war in this world which ceases neither day nor night. It is the war against disease and death. Those engaged in this battle, whether they be in the crowded city or in the lonely swamp, are ever seeking some new weapons with which to meet the foe, and it has been my privilege to-night to explain, however imperfectly, some few of the most recent additions to the scientific armoury of medicine.

NOTES.

Radium, life	2500 years
Emanation, average life	5·57 days
Active deposit	about 3 hours
Thorium, average life	unknown
Meso-thorium I., average life	7·9 years
Meso-thorium II. „ „	8·9 hours

Radiation, Beta and Gamma Rays.

Radio-thorium, life	2·91 years
Alpha rays	—
Thorium X	5·35 days
Alpha rays	—
Actinium	—
Emanation	5·6 seconds

Rutherford gives the following :—

Thorium Series.

		Half-period of Transformation.	Nature of Radiation.
	Thorium	15 × 10^{10} years	Alpha
	Meso-thorium I.	5·5 years	
	Meso-thorium II.	6 hours	Beta and Gamma
	Radio-thorium	2 years	Alpha
	Thorium X	3·6 days	Alpha
	Emanation	54 seconds	Alpha
Active Deposit	Thorium A	10·6 hours	Beta
	Thorium B	1 hour	Alpha
	Thorium C	?	Alpha
	Thorium D	3 minutes	Beta and Gamma

GENERAL MONTHLY MEETING,

Monday, February 5, 1912.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

E. Baynes Badcock, Esq., M.A.,
The Rev. Prebendary Jeakes, M.A.
Mrs. George Stibbard

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to G. H. Griffin, Esq., for his Donation of £21 to Fund for the Promotion of Experimental Research at Low Temperatures.

The Managers reported, That they had received the sum of £300, being part of the Legacy of the late Miss Wolfe to the Royal Institution.

The Chairman read the following letters which had been received from the Honorary Members elected at the General Meeting on December 4, 1911:—

FRA
REKTOR VED DET KONGELIGE,
FREDERIKS UNIVERSITET.
Kristiania den 31-12-1911.

THE SECRETARY OF THE ROYAL INSTITUTION.

DEAR Sir,

Some time ago I have received from you at first the information that the Royal Institution of Great Britain has elected me an Honorary Member of the Institution, and a little later on also the diploma of this Membership, as well as a volume of the Proceeding for the year 1909, etc.

I must ask you to excuse me, that my health has prevented me to send you before an acknowledgment of my reception of your letter and the diploma. Then I must ask you, on my behalf, kindly to present my respects, and to express my most hearty thanks for the great honour conferred upon me to the President of the Royal Institution, His Grace The Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., as well as to the Society.

To have been elected an Honorary Member of this illustrious Institution that has reckoned since the start and still reckons on the list such a number of most prominent investigators and benefactors of mankind, is an exquisite honour, that I appreciate as the highest distinction and courtesy paid, not only to me myself, but primarily also to the University of Norway.

Yours respectfully,

(Signed) W. C. BROGGER, Ph.D. LL.D. D.Sc. F.R.S, etc.
(Professor at the University, Kristiania, Norway.)

[Translation.]

HEIDELBERG,
12*th December*, 1911.

To the Secretary, SIR WILLIAM CROOKES,
Royal Institution of Great Britain, London.

HONOURED SIR,

I hasten to express my thanks for the great honour that has been conferred on me by making me a Member of the Royal Institution of Great Britain. I regard it as a special and undeserved mark of distinction to be enrolled among the Scientists and those who devote their lives to research, famous and distinguished men of the past and present of this great Society.

Requesting you to convey my thanks to His Grace the President, and to the other Members of the Society,

Believe me, honoured Sir,
Yours most faithfully,
(Signed) THEODOR CURTIUS.

[Translation.]

UNIVERSITY OF GENEVA,
16*th November*, 1911.

FACULTY OF SCIENCE.

To
HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S.,
President of the Royal Institution of Great Britain, London.

YOUR GRACE AND MUCH HONOURED PRESIDENT,

I have just received the Diploma, signed by you, informing me that the Royal Institution of Great Britain has elected me to be one of its Honorary Members.

I was particularly pleased and gratified at the honour conferred on me by a learned Institution which is distinguished by such great names as Davy, Faraday and Tyndall. It is a great encouragement to me to pursue the scientific career on which I have embarked.

I am writing to ask you if you will kindly act for me at the Meeting of the Royal Institution and convey to its Members my sincere and grateful thanks on this occasion.

Believe me, honoured Sir,
Yours very faithfully,
(Signed) PH. A. GUYE.

[Translation.]

PARIS,
8*th December*, 1911.

HIS GRACE, THE PRESIDENT.

YOUR GRACE,

The Royal Institution of Great Britain is conferring a great honour upon me in making me an Honorary Member of its distinguished Society. I am extremely grateful, and I ask you to be so good as to offer my best thanks to my new colleagues who unanimously elected me: nothing could appeal to me more than this high mark of distinction.

Believe me, honoured Sir,
Yours very faithfully,
(Signed) G. LEMOINE
(Member of the Institute, Officer of the Legion of Honour, Inspector-General of Bridges and Roads, Professor of Chemistry at the Polytechnic School.)

BERLIN N.W. 7
NEUE WILHELMSTRASSE 16.
8. 12. 1911.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
F.R.S., President of the Royal Institution.

SIR,

I am very proud of having been elected an Honorary Member of the Royal Institution of Great Britain, and I beg to offer my sincere thanks for the great distinction you have bestowed upon me.

I am, Sir,
Your very obedient Servant,
(Signed) HEINRICH RUBENS.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Lords of the Admiralty—Nautical Almanac, 1914. 8vo. 1911.
The Secretary of State for India—Report on Government Meteorological Department, 1910–11. 4to. 1911.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XX. 2° Semestre, Fasc. 10–12. 8vo. 1911.
American Academy of Arts and Sciences—Proceedings, Vol. XLVII. Nos. 8–9. 8vo. 1911.
American Geographical Society—Bulletin, Vol. XLIII. No. 12. 8vo. 1911.
Armstrong, Professor H. E., Ph.D. LL.D. F.R.S. M.R.I.—Det Kongelige Frederiks Universitet, 1811–1911, Festskreft, Vol. II. 4to. 1911.
Asiatic Society, Royal—Journal for Jan. 1912. 8vo.
Association of Accountants—Journal, Vol. IV. No. 16, Jan. 1912. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXII. Nos. 1–2. 8vo. 1911.
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WEEKLY EVENING MEETING,

Friday, February 9, 1912.

SIR WILLIAM CROOKES, O.M. LL.D. D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

JOHN ALLEN HARKER, ESQ., D.Sc. F.R.S.

Very High Temperatures.

[ABRIDGED.]

EXACTLY a century ago this month Michael Faraday entered the Royal Institution for the first time. He was then a youth of twenty, in the last year of his apprenticeship to a bookbinder in Blandford Street. Among the meagre records we possess of Faraday's early life we find the following :—

"I had the good fortune, through the kindness of Mr. Dance, who was a customer in my master's shop and also a member of the Royal Institution, to hear four of the last lectures of Sir Humphry Davy in that locality. The dates of these lectures were Feb. 29, March 14, April 8 and 10, 1812." It was Faraday's habit to occupy the seat in the gallery over the clock. He made very full notes of the lectures, and afterwards wrote them up, indexed and bound them with his own hands into a volume of 300 pages, which is now preserved at the Royal Institution.

Some months later Faraday writes : "Under the encouragement of Mr. Dance I wrote to Sir Humphry Davy, sending, as a proof of my earnestness, the notes I had taken of his last four lectures. The reply was immediate, kind and favourable."

In March 1813, apparently largely on the strength of the impression made upon Davy by this volume of notes, Faraday was engaged as assistant in the laboratory of the Royal Institution at a salary of 25*s.* a week, with two rooms at the top of the house.

The first lecture of Davy's course referred to was on "Radiant Matter," and dealt, among other things, with the action of electric sparks on gases. Ever since Volta's discovery in 1800 Davy had been occupied with the study of the pile and the effect of the new currents in producing heat and chemical change, thus leading up to his decomposition of the fixed alkalis and the isolation of potassium in 1807.

Following on this discovery, Davy proposed that a fund "should be raised by subscription for the construction of a large and powerful battery, worthy of a national establishment, and capable of promoting

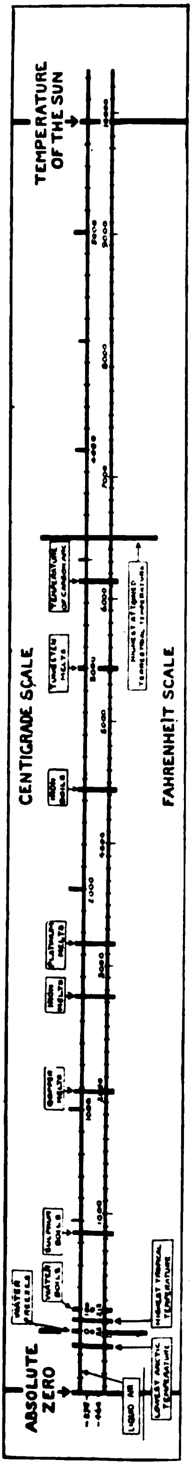

Fig. 1.

the great objects of science, and that this battery be erected in the laboratory of the Royal Institution." The sum required, a little over £500, was soon got together, and at the concluding lecture of the 1812 season the battery was put in action for the first time. We read in Davy's "Elements of Chemical Philosophy," iv. p. 110, an account of how he applied the battery to the running of an electric "arch" between two carbon rods. Parts of Davy's battery are still preserved at the Royal Institution.

I begin my lecture thus, merely to emphasize once more the truth of the adage of 3000 years ago: "There is no new thing under the sun."

In 1912, when considering the subject of "very high temperatures," we can claim, comparatively speaking, to be capable of little more than Davy accomplished a century ago. In his arc he melted all the most infusible materials known to him, including lime and magnesia, which are among the most refractory materials in use at the present day.

Turning now from the historic to the present aspect of our subject, permit me to begin with a few elementary considerations as to our conception of temperature. I think I am correct in saying that everyone has some idea in his own mind of a temperature-scale, a kind of intuition which is generally a fairly useful one for practical purposes. Probably I am not exaggerating when I say that even men of science, who always think for their professional purposes of temperatures on the Centigrade scale, find themselves obliged to convert to Fahrenheit for an idea of the temperature of a room or of a summer's day.

I have endeavoured to give a graphic representation (Fig. 1) of the temperature scale as we know it, both in Centigrade and Fahrenheit degrees. You will notice the smallness of the interval between the extreme temperatures that prevail in the arctics and the tropics; and how restricted the "cold" region down to absolute zero is compared with the possibilities in the other direction. While, on the one hand, Kammerlingh Onnes by the evaporation of liquid helium under low pressure has succeeded in getting during the last few weeks to within 1·1° C. of absolute zero, the highest recorded terrestrial temperature—that of an electric arc under high pressure—falls short of the sun's estimated temperature by some 2000° C.

Some landmarks in our available range of temperature are given in Table I. It may be remarked that the three substances last quoted in the table are all in extensive use for electric lamp filaments.

TABLE I.—VARIOUS TEMPERATURES.

	Deg. C.
Absolute zero	−273
Helium boils (0·2 mm.)	−272
,, ,, (760 mm.)	−269
Hydrogen boils	−253
Oxygen boils	−183
Carbonic acid boils	− 78
Mercury freezes	− 39
Water freezes	0
Water boils	100
Tin melts	232
Lead melts	327
Mercury boils	357
Zinc melts	419
Sulphur boils	445
Aluminium melts	657
Common salt melts	801
Zinc boils	918
Silver melts	961
Gold melts	1062
Copper melts	1083
Cast-iron melts about	1100
Pure iron melts	1500
Fire bricks soften	1400–1800
Silica softens	1500–1600
Platinum melts	1750
Silver boils	1950
Tin boils	2270
Copper boils	2310
Lime and magnesia melt about	2400
Iron boils	2450
Tantalum melts	2900
Tungsten melts	3000
Carbon melts	?

Table II. gives examples of various flame temperatures, which we have at our disposal.

TABLE II.

Temperatures attainable in :—	Deg. C.
Bunsen burner flame	1100–1350
Méker burner flame	1450–1500
Petrol blow-lamp flame	1500–1600
Oxy-hydrogen flame	about 2000
Oxy-acetylene flame	,, 2400
Thermit	,, 2500
Electric arc	,, 3500
Electric arc (under pressure)	,, 3600
Sun	,, 5500

Some of the methods for measuring temperature with their limitations are briefly recapitulated in Table 3. I have only time to refer to one or two points. We have recently had the opportunity at the National Physical Laboratory of subjecting a number of mercury in silica thermometers to a critical examination. These thermometers which are made in England, possess in a high degree the qualities of constancy, large range, and such complete freedom for temporary zero change that I feel safe in prophesying they will inevitably replace the present International standards, which are made of verre dur.

TABLE 3.—SOME INDICATION OF THE PRESENT RANGE OF TEMPERATURE MEASURING INSTRUMENTS.

Method	Range in Degrees C.	
	Practical	Extreme
Expansion thermometers—		
Gas thermometer	Up to 1200°	−272° to 1550°
Mercury in glass	− 39° „ 500°	− 44° „ 575°
Mercury in silica	− 39° „ 600°	− 44° „ 700°
Electrical thermometers—		
Platinum resistance	−100° „ 1100°	−250° „ 1400°
Thermocouples:		
— platinum alloys	300° „ 1400°	Up „ 1750°
— base metals	−100° „ 1100°	−250° „ 1200°
Total radiation pyrometers	500° „ 1400°	No upper limit
Optical pyrometers	600° „ 3500°	No upper limit

In regard to high temperatures most of us rely to some extent on colour in estimating temperature. Table 4 gives a very fair notion of the temperature we may reasonably associate with the colour of a fire or muffle furnace (experiment shown). The intensity of the light varies according to well-known laws which have been studied

TABLE 4.—TEMPERATURE AND COLOUR OF A FIRE.

Colour	Cent.	Fahr.
"Grey," lowest discernible temp.	About 450°	About 850°
Very dull red	„ 500°	„ 950°
Dull red	„ 700°	„ 1300°
Cherry red	„ 900°	„ 1650°
Orange	„ 1100°	„ 2000°
White	„ 1300°	„ 2400°
Dazzling white	Above 1500°	Above 2750°

up to sun's temperature. If we know the law of variation we can measure the temperature by the use of some kind of photometer—which is what all optical pyrometers are.

For obtaining really high temperatures electric furnaces are our only resort. Small gas furnaces can reach 1600° with difficulty; large industrial furnaces attain 1800° C. in some instances.

Mr. Cook, of Manchester, has kindly lent me for this occasion a number of electric furnaces. These are constructed by winding tubes of fire clay or alumina with nichrome or platinum wire: the external lagging is of kieselguhr. Steady temperatures up to about 1000° and 1200° C. respectively can readily be got with power from a commercial circuit of 100 or 200 volts. With thicker wires and current at lower voltage these upper limits can be appreciably extended.

For higher temperatures we have to make use of carbon or graphite, and electric heating was first applied by such means in the form of the arc furnace. Such a furnace has many inconveniences—the heating is intensely local, and there may, for example, be a gradient of 2000° C. in a single inch. There is practically no temperature control, and there is every possibility of the final product becoming largely contaminated with carbon. Most of the early isolated so-called elements have since proved to be largely carbides.

Resistance heating is usually much more convenient, and this is the principle of carbon-tube furnaces, some essential features of which were employed by Prof. Dewar many years ago. They will stand rough use, and are much more controllable than the arc furnace. It is as easy to control a temperature of 2500° C. as one of a red heat.

Such furnaces usually have their end-terminals water-cooled, and are surrounded by lagging of lamp-black or charcoal.

The furnace tubes are either straight if made of carbon (Fig. 2), or spiral if made of Acheson graphite (Fig. 3). In the latter case they are provided with an internal liner-tube of carbon. There is no special difficulty in cutting the spirals from the solid; graphite, unlike amorphous carbon, is an extremely tractable substance to machine.

We have used these carbon resistance furnaces a great deal at the National Physical Laboratory, and Mr. Greenwood, at Manchester, carried out his experiments on boiling metals by the aid of such a furnace. The boiling of a metal (shown) forms a not-impossible lecture experiment, and a projected image of the surface of boiling tin displays all the usual phenomena of ebullition. The heating up of carbon is somewhat strikingly shown by passing a heavy current through a thin broad carbon strip provided with water-cooled terminals (experiment shown). The stream lines of heat flowing from one terminal to the other are well illustrated at one stage of the heating.

Among other methods of electric heating are the induction furnace, which is of great value in refining crude materials, and the flame spark, in which it is possible to volatilize as refractory a substance as an incandescent gas mantle.

Some time ago we endeavoured at the National Physical Laboratory to make a furnace for very high temperatures without employing carbon. The introduction of the Nernst lamp was suggestive. It was found that a great number of substances could be made to act like a Nernst filament, e.g. a piece of the stem of a churchwarden pipe, if sufficiently strongly heated, can be made to conduct electricity well enough to become incandescent. Carborundum crystal behaves similarly, and requires no initial heating (experiment shown); in this case the temperature can be raised high enough to volatilize off the silicon, which burns, forming a cloud of silica. A cascade furnace was constructed on these lines: a tube made up of zirconia and a little yttria was raised by means of an insulated nickel winding to 500° or 600°, at which temperature the tube conducts sufficiently well to enable a heating current to be passed through it. There is no difficulty in melting platinum, for example, in such a furnace using a quite small heating current (about 2 amps.) A zirconia tube from such a furnace was taken out after it had been run for six

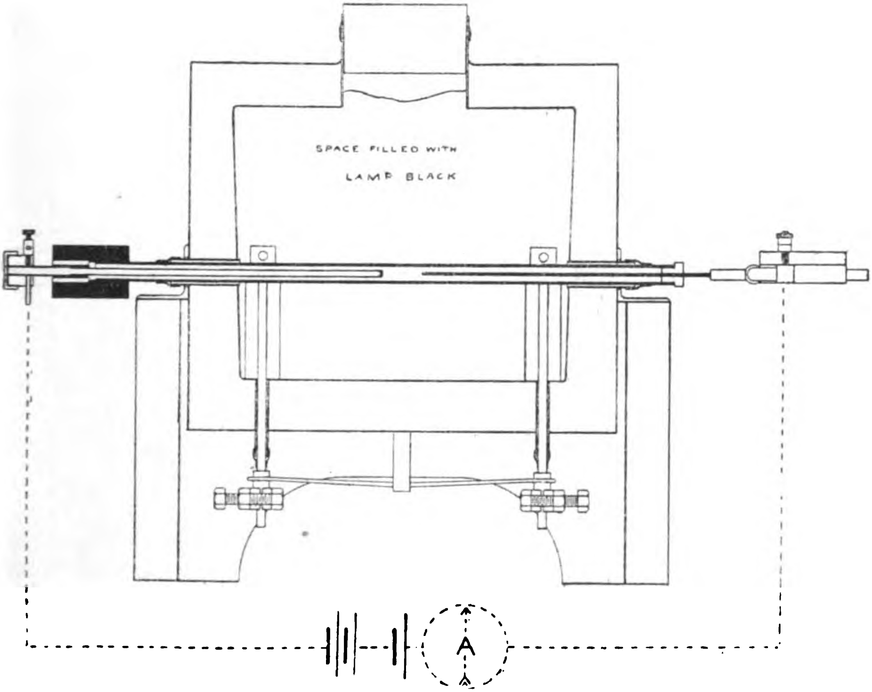

Fig. 2.—Small Model Straight Carbon-Tube Furnace.

months or so; it was then found to be quite translucent. The possibility of constructing in such a way refractory gas-tight materials at once suggested itself, and we proceeded to manufacture "pottery" at high temperatures. Great difficulties have been encountered in the experiments. Whereas, for example, the potter in baking his wares at temperatures up to 1300° C. looks for a shrinkage of 5 per cent. or so, we were confronted with a shrinkage of 37 per cent. For the purposes of the fritting we employed carbon-tube furnaces of one of the types mentioned above. Now it sometimes happened that the outer surface of the zirconia tubes, instead of having the white and hard appearance of the rest, was found to be carburised and crumbly after baking. The action was not merely superficial, but extended to an appreciable depth. On the other hand the inner surface of the tube, though freely open to the furnace atmosphere, was much less affected. The blackening occurred to a much less extent if the tube was shielded. It seemed as though particles, possibly electrified, were shot off from the carbon walls of the furnace across a space of some 5 or more mms. into the material of the refractory tubes. Dr. G. W. C. Kaye and I were led to investigate the cause of these phenomena, and yesterday we gave an account of some of the results to the Royal Society. I propose to

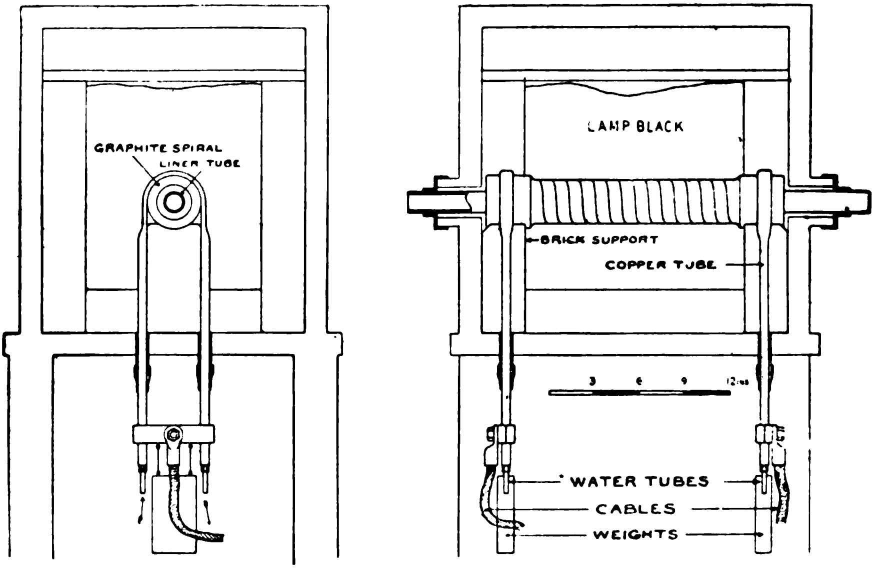

FIG. 3.—CARBON-TUBE FURNACE WITH GRAPHITE SPIRAL HEATER.

devote the remainder of my lecture to a description of the methods employed and the results obtained in what proved to be a very interesting investigation.

Many experiments have been conducted, notably by Prof. O. W. Richardson, on the corpuscular emission of electricity from carbon at very low pressures, but scarcely anything is recorded for pressures approaching atmospheric. Positive ions and material particles are also discharged by carbon, as well as by hot metals, at suitable temperatures and pressures.

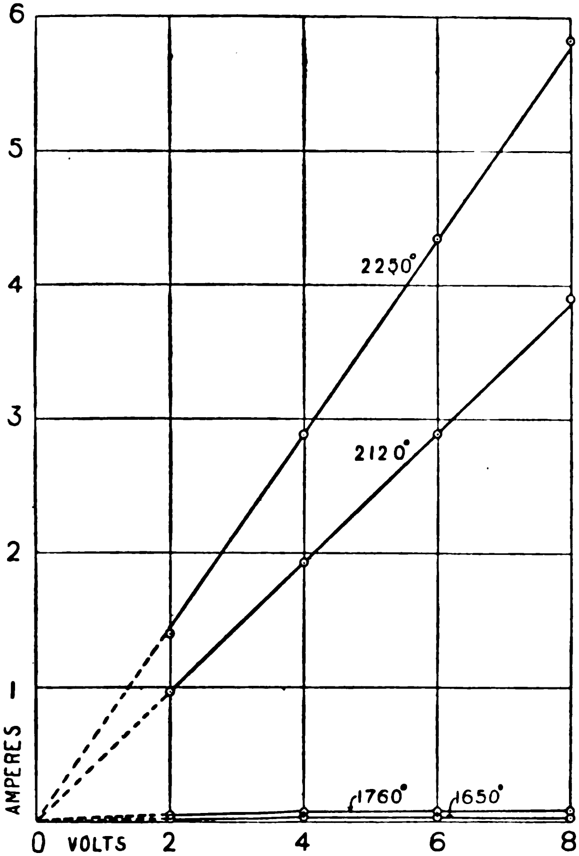

Fig. 4.—Relation between Ionization Current and Applied Potentials for a 1 cm. Gap between the Electrodes.

It is to be understood that in all the experiments now to be described the pressure remained atmospheric, and alternating current was employed for heating. Access of air to the interior of the furnace was prevented by windows at each end, perforated as required.

In the early experiments we inserted within the carbon furnace tube two insulated carbon electrodes, one of them being hollow, so that a Siemens optical pyrometer could be sighted through it. The two electrodes were joined externally to an ammeter and a battery of cells (see Fig. 2), and we proceeded to determine current-voltage curves at various furnace temperatures. Some of these curves are shown in Fig. 4 for an electrode gap of 1 cm. No appreciable current could be detected below 1400° C. with applied potentials up to 8 volts, but as the temperature rose the current rapidly increased until at 2500° or more currents up to 10 amperes were recorded. At the lower temperatures the currents soon attain what appear to be saturation values. At higher temperatures there is a linear relation between potential and current. As the length of the gap increased the current diminished at a regular rate; but the decrease was small.

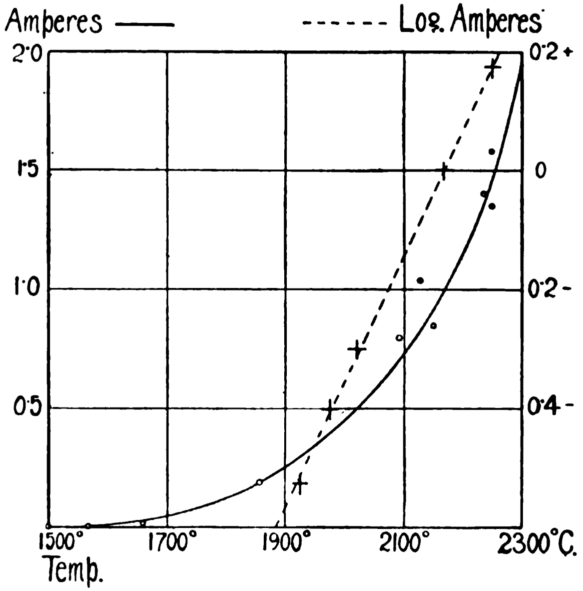

Fig. 5.—The Full Line Curve shows a Relation between Ionization Current and Temperature, for an Applied Potential of 2 Volts on a 1 cm. Gap between the Electrodes. The dotted straight line is plotted from the log. of the current and the temperature.

Fig. 5 exemplifies the exponential relation between temperature and current for a 1 cm. gap and an applied potential of 2 volts. The dotted straight line was plotted to axes of temperature and logarithm of current.

The magnitude of the currents made it evident that in spite of the high pressure the atmosphere of the furnace was ionized to an unusual degree at high temperatures, and we were led to investigate the effect of temperature alone. The battery was accordingly cut out and one of the two carbon electrodes was mounted on a sliding carriage so that it could, at will, be moved in or out of the hot part of the furnace, i.e. away from the fixed electrode or back towards it. The movable electrode would thus be temporarily cooler than the fixed electrode which remained steadily in the furnace. The ammeter in the circuit indicated a current which amounted to 2 milliamperes at 1400° and nearly 2 amperes at 2500°; the cooler electrode was the positive one. The currents died away when the two electrodes attained the same temperature.

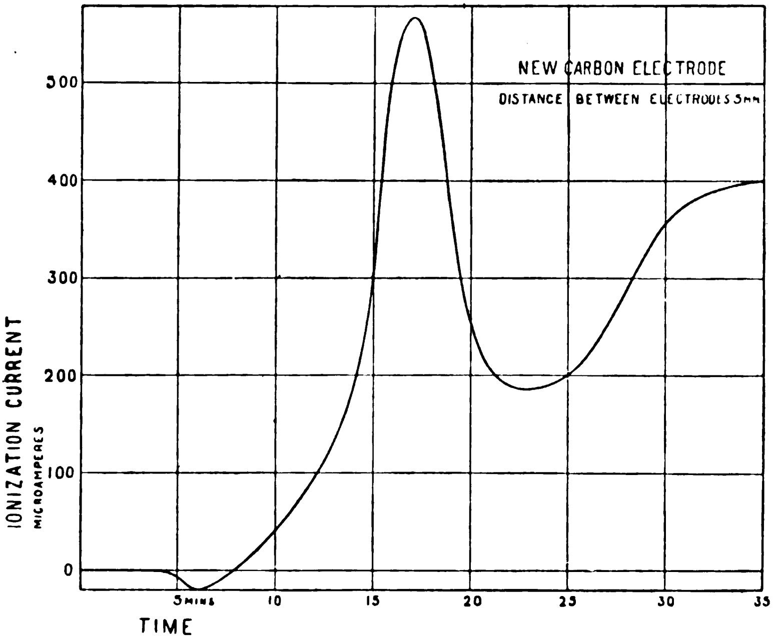

Fig. 6.—Relation between Ionization Current and Time with a Steadily Rising Temperature. The "cold" electrode was water-cooled; the hot electrode was of new carbon. No potential was applied.

The production of an alternating current of very low frequency is thus rendered possible by the use of some periodic device. In one form of the experiment (shown) the movable electrode is attached to a crank which, rotated slowly by clockwork, performs the necessary displacement of the electrode within the furnace. The ionization currents produced are sufficient to make a nest of small glow-lamps light up brilliantly, the illumination waxing and waning as the movable electrode moves in and out.

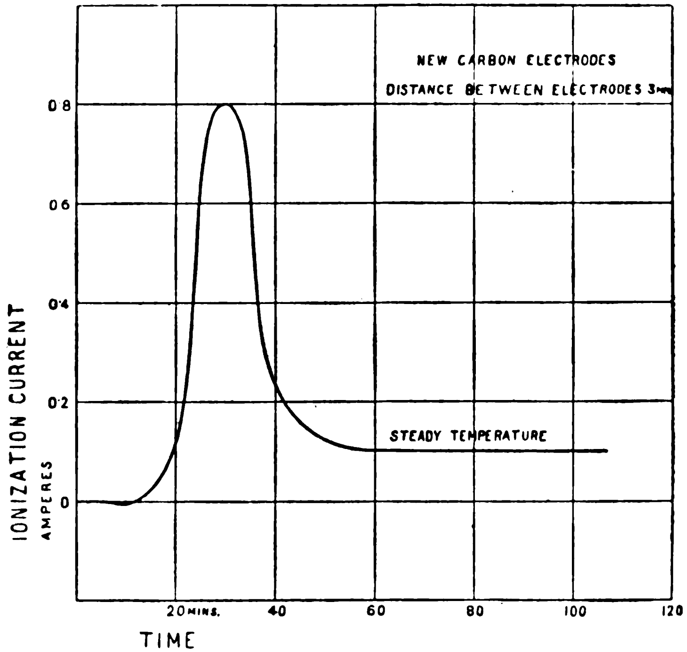

Fig. 7.—Relation between Ionization Current and Time for Two New Carbon Electrodes, one Hot, the other Water-Cooled. No potential was applied. The temperature was rising for the first fifty minutes, and was afterwards steady.

We have been able to repeat some of these results with furnaces of a non-electric character.

In a further series of experiments, various modifications were introduced. The two electrodes were replaced by two co-axial tubes which were mounted within the furnace. The central smaller tube was of brass, through which a rapid current of water was sent; this formed the "cold" electrode. The surrounding larger tube of carbon constituted the hot electrode, and received its heat from the furnace.

The electrodes were insulated as before, and into the annular space between them hydrogen or nitrogen was continually passed. No potential was applied, and the currents we obtained with a steadily rising temperature and a new carbon electrode are shown in Fig. 6. It will be seen that there was first a small "positive" current (which would be produced by positive ions crossing from the hot to the cold electrode), which soon changed into a much larger "negative" current (in the usual direction); the intensity of the latter dropped, and then showed a progressive increase with temperature. On taking down the apparatus we found that the brass tube was coated over most of its length with a thick and coherent deposit of carbon which had evidently crossed over from the hot electrode. Towards one end the deposit was rarer and whitish—presumably silica. We associate the maximum negative current of Fig. 6 with the passage of silicon and other impurities, which are volatilized at about 2000° C. out of the carbon electrode. On a second heating neither positive rays nor a negative maximum were detected, but the ionization current increased steadily with temperature. The transference of carbon from the hot electrode to the cold may prove to be a complete explanation, not only of the contamination phenomena which gave rise to these experiments, but also of the comparatively large accompanying currents.

Fig. 7 illustrates the results obtained when steps had been taken to increase the difference of temperature between the hot and cold electrodes. The carbon was new, and the negative maximum again appears. Afterwards the furnace temperature was steadied, and the ionization current also kept steady in consequence. It will be noticed that we were now dealing with currents amounting to large fractions of an ampere, and the experiments may fairly be regarded as providing a novel means of generating electricity. Their direct bearing on the problems of the electric arc and the carbon filament lamp is obvious, and we are continuing the research with a view to elucidating the many underlying phenomena.

[J. A. H.]

WEEKLY EVENING MEETING,

Friday, February 16, 1912.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L.
LL.D. F.R.S., President, in the Chair.

The Right Hon. Sir John H. A. Macdonald,
K.C.B. LL.D. F.R.S.(L. & E.) M.Inst.E.E.,
Member of H.M. Road Board.

The Road: Past, Present and Future.

The Road, during the latter half of the nineteenth century, was a matter in which the public took little if any interest. When the railroad era set in, the mail-coach, the travelling carriage, and the long-distance delivery wagon were swept off the road, which was used only for short journeys. Thus the highway, which before that time had presented a goodly show of traffic, became a secondary consideration in county affairs. The Bath Road furnishes a graphic illustration of the change. The 140 mail-coaches crossing one another on that route each day disappeared, and nothing remains now to tell of the active road bustle of that time but the disused pumps standing at every interval of two miles, from which the entire road was watered daily to keep down the dust-cloud, which so many people seem to think had no existence until the motor-car came on the scene. So little was efficiency thought of in road management that the post of road-surveyor became the consolation gift to the worthy local man who had been a failure at everything else. The appointment once again fell into the position described at an earlier period of which we read in evidence given before a Select Committee, that the surveyors in a particular district included "a miller, an undertaker, a carpenter, a coal-merchant, a publican, a baker, an infirm old man, and a bedridden old man who had not been out of his house for several months." And it was declared that nineteen times out of twenty, the appointment was "a perfect job." This is not to be wondered at, for in the same Blue Book we read that it was usual to appoint "a prodigious number of commissioners for the care of ten or fifteen miles of road; and thus a business of art and science is committed to a promiscuous mob of peers, squires, farmers and shopkeepers, who are chosen not for their fitness to discharge their duties, but from the sole qualification of residence within a short distance of the road to be made or repaired."

These quotations relate to the early years of the last century. Doubtless there was some amendment when Macadam and Telford came upon the scene; but, as I have said, things fell back again sadly when the railroad carried off nine-tenths of the traffic. Thus, "like master, like man"—as it was with the surveyor, so it was with the road man. Too often he was an old labourer bending under rheumatism, who was no longer fit for his ordinary employment, and, the choice being between putting him on the rates as a pauper, and giving him wages to do road work for which he had no training, and which he was unable from senile decay to do efficiently, he was put on and kept on the road as a make-believe roadman, to do work which calls for intelligence and full virility. How often did one see him in the fifties of last century, and for many years later, laboriously and slowly scraping off the mud which the traffic had squeezed out of the road in wet weather! In dry weather it was the turn of the stones whose binding had been loosened, and they lay on the surface to cause horses to stumble, or to lame them by wedging between the frog and the shoe. These loose stones the decrepit roadman should have raked off, a duty which I can vouch from what I saw and suffered in my youth was but intermittently performed, and often not performed at all.

What a change has come over the scene since the new century was entered upon! The road is now one of the ruling elements, not only in local but in Imperial politics. For once more the traffic of the country, whether for pleasure, for business, or for commerce, is being largely conducted by road, and it is manifest that in the future it will be so in an ever-increasing degree. Millions of miles are now run every month by road-vehicles where, twenty years ago, the mileage run was but a small fraction of what it is now. The rapidity with which this revolution in road transit has taken place has been a great surprise to all—even to those who welcomed it. It has excited varying emotions in the community. Some rejoice: others—a gradually diminishing number—utter anathemas in no gentle tone. But all are in agreement on one thing: that the change by which the road has once more become a principal and not a mere local provision for transit of passengers and goods is a change for all time. The most bitter opponent of the modern power vehicle has to confess that the situation must be accepted. It required a course of years to remove the scales from some eyes. About four or five years ago, a very worthy friend of mine delivered himself to me in solemn tones, thus: "In another ten years there will not be half the motor-cars on the roads that there are now." I grieve to say he did not live to finally test his own prediction, but during the four years that passed he never referred to the subject again, as he lived to see the numbers increasing by tens, or even twenties, of thousands annually, and found himself unable to resist his own family, when they desired to use motor-cars. To be consistent, he would not buy, but he hired. This affords a curious instance of how history repeats itself. Exactly the same kind of remark was made shortly after the introduction of railroad transit. In 1838 Sir Henry Parnell, in his Treatise on Roads, declared that—"the attaining of a speed of 25 or 30 miles an hour cannot be justified on any principle of national utility—how it can be made to contribute very much to the benefit of the country at large it is not easy to discover." And he added his prophecy: "After the means of moving at the rate of ten miles an hour is universally established, there seems to be no very great advantage to be derived from going faster," and he declares that railway transit cannot possibly pay, so that—"the cheaper method of using horse power will be adopted." One can but smile on reading this in the light of what has taken place since.

That the Council of this Institution should welcome a Paper on the Road, and that you should be here to listen to it, is proof incontrovertible of the great change. I must apologize for speaking of "a Paper." In Royal Institution parlance, I am delivering a "Discourse," and it seems natural that it should divide itself into three heads. The first is the Past, and what is to be said upon it may be interesting and useful for comparison with head two, the Present, and head three, the Future.

It is clear from history that the first maker of roads in Britain was the conqueror from Rome. We can see to-day how the invading Roman made solid, strong roads to facilitate the transfer of his forces, thus lessening the time necessary for moving his legions from point to point, and so making one phalanx worth two in many circumstances of fighting emergency. These roads testify to the skill of the Roman military engineer, solid, strong, well-drained, and with an upper crust of close-laid pavement, thoroughly efficient, an example that later generations were slow to follow.

After the Roman period, and for hundreds of years, road-making with design was unknown. The State did nothing, and it fell to the Church to do a little, as a work of piety and charity. We read that in the 14th century the Bishop of Durham remitted part of the penalties on the sins of those who did good work in helping to "smooth the way of the wanderer," and granted 40 indulgences to contributors to road repair. But zeal declined and mud increased. The road user had to be his own road-maker. The poorer class trudged on foot, where now they ride in comfortable carriages and feed in dining-cars, the richer class, kings, queens, nobles, knights, gentry, merchants, rode on horseback, the ladies being sometimes carried on pillions, or by litters, instead of first-class saloons. The pack-horse team was the mode of conveyance for goods. The road was formed by the traffic following the footpaths. To this is to be attributed the very tortuous character of many of our old ways, the path-maker naturally moving round obstructions, and the horse following widened the track, which later the road-maker also followed, without straightening the line or relieving gradients by cutting and embankment. As was to be expected, roads thus formed at haphazard were soon impassable at many places. The travellers then went aside, in fact they were enjoined to do so by an old statute, which declared that "where any highway is worn deep, and incommodious, another shall be laid out alongside." Thus the curving character of the road was aggravated, and a state of things produced the very antithesis of the old direct firm permanent Roman way. The road was a quagmire full of stones, which rolled about loose when fine weather dried it up.

Of course, looking back over a long distance of time, it all seems highly picturesque to the imagination. The illustrations in the history books of our childhood showed us kings and queens making journeys, and conveyed to us no idea of discomfort or hardship. A Queen Elizabeth or a Queen Mary was represented as arriving at some stately castle after a long day on the road, her velvet skirts without a speck of dust or clot of mud besmirching their beauty. How different must the reality have been! As regards driving, which was being introduced in Elizabeth's time, we read of that Queen complaining to the French ambassador that she had been "full of aching pains for days," consequent upon a drive through London in a regal chariot, which she was using for the first time. And it must have continued to be much the same for many generations. We may set beside Elizabeth's short drive through London Sydney Smith's humorous wail of much later date, when he professed to know, as he said, "approximately" the number of "severe contusions" he received on a journey from Taunton to Bath, his figure being 10,000 to 12,000 jolts. These distresses were the consequence of road construction and maintenance being left to the haphazard attention of each locality, instead of being treated as a matter of general interest, calling for well thought-out scientific treatment. Even when laws were passed, by which the inhabitants of each locality were ordered to supply six days' labour on the roads annually, the work was done on no system, was evaded by many, was scamped shamefully, and the money provided by those who commuted was squandered and often pilfered. A traveller's book of 1770 gives a graphic description of a main road at that time—that from Preston to Wigan.

"I know not, in the whole range of language, terms sufficiently expressive to describe this infernal road . . . let me most seriously caution all travellers to avoid it as they would the devil; for a thousand to one but they break their necks or their limbs by overthrows or breakings down. They will here meet with ruts which I measured, four feet deep, and floating with mud. . . The only mending it receives is the tumbling in of some loose stones, which serve no other purpose but jolting a carriage most abominably. . . I actually passed three carts broken down in these eighteen miles of execrable memory."

This is a tale of mud. Sheridan, in "The Rivals," gives us the tale of dust, when he makes Bob Acres shout on his arrival at Bath:—

"Warm work on the road, Jack—Odds whips and wheels! I've travelled from London like a comet, with a tail of dust all the way as long as the Mall."

These instances, the real and the illustrative, apply to a comparatively late period, when the post-chaise had come into common use. Earlier travelling by vehicle was confined to the broad, lumbering, 2½-miles-an-hour wagon or wain, drawn by a string of six, eight, or even more horses, that it might be forced through the slough of mud collected in the ruts, which, apparently, were never filled up until the wagons had their axles scraping the ground. The rate of speed under good conditions was 20 miles a day, wains from Lancashire to London taking 10 days in summer and 11 days in winter.

Notwithstanding all the difficulties, efforts were used by enterprising people to introduce stage coaches, which gradually made their way, until in the eighteenth century they were boldly advertised as "Flying Coaches"—the Newcastle to London, named "The Flyer," flew for nine days on the way—a speed of about 25 miles a day. A few years later an advertisement appeared which said that "incredible as it may appear, this coach will actually arrive in London four days and a half after leaving Manchester."

Then, just as now, these modest steps of progress were denounced by the wiseacres of the day as tending to ruin the country. Carriages were spoken of as "upstart hell-carts and coaches," just as now reverend gentlemen speak of motor-cars as satanic. We read that innkeepers complained of the speed at which "people flew through the country." One John Cressit declared that "these coaches are one of the greatest mischiefs to the public, destructive to trade and prejudicial to lands"—and specially injurious to tailors and hatters, as gentlemen who rode on horseback spoiled their clothes and hats and required to renew them often, whereas clothes lasted longer when one travelled by coach, and so the clothier and hatter lost custom. And Taylor, the poet-wherryman, bursts forth :—

> "Carroches, coaches, jades and Flanders mares
> Do rob us of our shares, our wares, our fares,
> Against the ground we stand and knock our heels,
> Whilst all our profit runs away on wheels."

All this time the roads were as bad as ever. Here is an eloquent wail indicating what had to be endured :

"Is it for a man's health to travel with tired jades, and to be laid fast in foul ways, and forced to wade up to the knees in mire . . . to have their tackle, or pearch, or axle-tree broken, and then to wait hours to have them mended, and then to travel all night to make good their stage ?"

One more quotation, referring to the year 1737, is interesting as it speaks of a road which must have passed not many hundred yards from this spot. Lord Hervey, who wrote from Kensington in that year, says :—

"The road between this place and London is grown so infamously bad that we live here in the same solitude as we would do if cast upon a rock in the middle of the ocean; and all Londoners tell us there is between them and us an impassable gulf of mud." At the best of times, if there were no breakdown, a drive from Kensington to St. James's Palace occupied two hours.

How many of you, ladies and gentlemen, have rolled in luxury through Kensington to this Institution to-night? Yet such was the state of things in the time of the Georges. It will not surprise you to hear that about the same period fourteen days was considered a good run from London to Edinburgh, and that Sir Henry Herbert said solemnly in the House of Commons, "if a man were to propose to convey us regularly to Edinburgh in coaches in seven days, should we not vote him to Bedlam?"

So dreadful was the state of things in the eighteenth century that in the first seventy years 530 Acts of Parliament were passed, ordering Statute labour, authorizing turnpikes, etc., etc., but so indifferent were the public, and so inefficient was the work, that no real improvement was made. That the road should be rutted by the traffic till the vehicles sank to the axles was looked upon as inevitable, and no other remedy was thought of than to bring up the level by pouring in large stones. Thus, as Young sums it up, "we were either buried in quagmires of mud, or rocked to dislocation over pieces of rock, which they called mending." It is interesting to notice that from the time of the Romans down to the middle of the 18th century there is no record of any good road being made, till General Wade—again for military purposes—constructed a road through the Scottish Highlands which, when order was restored after the "'45," proved of great service to the country. An obelisk erected on the road bore this inscription, which contains as good a bull as ever was produced in Ireland:

> "Had you seen this road before it was made,
> You would hold up both hands and bless General Wade."

THE PRESENT.

A great improvement took place when Macadam and Telfer brought the results of their study and their inventive powers to bear, giving a road well laid below and a crust of small angular stones, which when pressed down close produced an infinitely better road than had been known before. But it must be admitted that while they provided better materials for a good highway their mode of completing it entailed upon the road user and his horse and vehicle a great deal of unpleasant road-making work, involving much temporary discomfort, and much wear and tear to animal and carriage. It is hardly to be appreciated by most people to-day, but an old man like myself can remember how in the fifties and even the sixties of last century the road-maker's laying work was ended when he had piled his surface stones loose upon the roadway. The road user had to apply his vehicle to roller work, to force the stones into a closely packed surface. Perhaps some who are present may recall to memory, as I can, how in those days the road man, when he had laid a covering of new metal, placed trestles at intervals, one on one side of the road and another on the other side, so as to compel the driver to zig-zag back and forward over the sharp-edged and pointed stones, his horse's feet acting as a tamping machine, and his wheels as a roller, until the hundreds of shoes and iron tyres had pressed down the stones into a macadam crust, and, when this was effected on the part of the road that was free for transit, the trestles were moved into position on the made road, and the traffic forced on to zig-zag No. 2, until all the surface had been brought to a macadam consistence, the whole road being packed close, by the stones descending gradually, breaking and grinding their own packing off themselves as they were slowly forced down, rubbing one against another. When completed it was a good road for the traffic of the day, but, oh ! it was trying work when the road user's vehicle not only conveyed his passengers or his goods, but was compelled to act the part of a road roller.

A great change took place when the heavy steam roller was introduced, which in one operation pushed the stones down into position. It afforded a blessed relief to those who used the roads with horses. But though advantageous in saving the road user and his horse and carriage and more expeditiously completing the new surface, it was not possible to provide a closely fitted road which should have its interstices filled up by the chip and grit from the stones themselves, and which was an essential desideratum according to the macadam theory. His principle was that no water should enter the surface of the road or penetrate beneath the crust. To keep water out of the road was one of the most essential points if it was to be efficient. He strongly condemned any insertion of loose material into the interstices of the metal, or allowing water to enter between stones of the crust. These maxims of Macadam came to be disregarded when steam rolling was introduced. When rolling was to be done in one operation, a device had to be resorted to that the spaces between the stones might be closed by added packing, and this has been done by making what can only be described as a soup of dirt and water and pouring it upon the stones and brushing and rolling this liquid mud into the crust of the road. The road thus when opened for use is crusted with a coating of stones whose only binding is water thickened with dirt, or perhaps dirt diluted with water is the proper description. The result is that it can never be a good road in wet weather, and can never be a good road in dry weather. As long as it is in a slightly damp state, and not subjected to severe wet weather or long-continued drought, it may be a fairly good road. In wet weather water can get in where it has come out, reproducing the mud soup, and the traffic squeezes it up and out of the road. In dry weather the binding being reduced in bulk and loosened by the evaporation of the moisture, which gave the inserted dirt some cohesion, the stones move and are picked out of the surface, and so holes are left for the water to lodge in the dirt below when again rain begins to fall. What would Macadam say, if he could visit the scene of his scientific labours, to hear the phrase " water binding " used to describe the means employed for consolidating the crust. To call a water-formed road a macadamised road is a contradiction in terms. His emphatic declaration was: " Every road is to be made of broken stones, without mixture of earth, clay, chalk or any other matter that will imbibe water."

But, further, the road roller has not in another aspect proved to be an unmixed blessing. For it is not uncommon to see that its use has developed another evil. The heavy road roller coming on to a layer of stones, surrounded with liquid, and therefore non-resisting, mud, and pressing down the stones by its weight, necessarily must move the water and the dirt in suspension, otherwise the stones would not go close together. The liquid is therefore squeezed out of the way, and as the great width of the roller, prevents its escape sideways, except at the edges, it must go forwards, and (water being practically incompressible) forces the water and the dirt and the stones in front upwards, forming a ridge before it. (Diagram shown.) The roller advances, and when it cannot force the ridge farther forward, it then mounts it and descends in front, and so *da capo*, with the consequence that the road becomes a series of ridges and furrows, and when drying up resembles a mackerel's side, a series of dark-toned wet hollows, and light-toned dry mounds. The road becomes bumpy for the traveller, and is unable to free its surface from water, and the material remaining soft and wet in the furrows, and becoming dry in the ridges, is open to destruction by the feet of the horses and the wheels of vehicles dumping down and striking compressing blows in the hollows. No worse state of matters for the traveller and his vehicle, or for durability of the road surface, can be conceived. Yet any observant person must have seen this road condition extending for long distances within a very short time after the crust has been relaid.

Another evil which developed itself as a consequence of the adoption of steam rolling has been the too common use of much larger stones than the size prescribed by Macadam. His metal had to pass through a gauge of 1½ inches. Such stones packed very efficiently when slowly moved down by the traffic. But road makers, when they adopted the liquid mud binding, fell into the practice of using very much larger stones. Of course their road metal when prepared consisted not of stones of the same size and weight, and this state of things brings about very damaging results. Here is a stone of the Macadam gauge, and here are stones that I have taken out of road metal laid down for use, or found pushed out of the road by the traffic. The Macadam stone, as specified by him, was to be only six ounces in weight. I have here stones of larger size which weigh a pound and even more. Let me illustrate to you the evil effects of the use of large stones. I have laid in this box as near a representation of a macadam road as I can, with several stones on the top layer of different sizes. To each of them I have attached a strip of bamboo, with a coloured disc on the outer end. I now press down on the Macadam gauge stone, putting the pressure over one-half of the width. I press at the same distance in from the end of each of the others, and you see how the larger stones are tilted up free of the surface, being loosened out of the binding. The same thing happens when the edge of a horse's shoe or the angle of a wheel tyre presses near the end of a large stone, and thus the surface of the road is broken up. A hole is left in which water can lodge and work its way down with fatal result. The effect is even worse if large rounded stones are used. Here are two stones which I found in the middle of a road—where I would rather not disclose, as I do not wish to be personal. Suffice it to say that it was not fifty miles from London. Such stones cannot stay in the surface of a road. They must roll loose and leave a hole, to be widened and deepened, and filled with water when rain falls.

Such was the state of matters on the main roads until within a few years of the present century, and it is still the same in many parts of the country. Another evil has come in, that on many roads it has been the practice to pile stones on the centre of the way, without making such arrangements as will keep the camber or curve of the road low. The result is that drivers, to escape the extra strain on their horses by driving along the side of a slope, and to avoid slipping down to the gutter when the road is greasy, keep on the very centre of the road, on which two ruts are soon formed which gradually get deeper, and in which water lodges, with most disastrous effect, the water being splashed out by the wheel, and mud being forced up and out by the blow struck when the tyre goes down into the hollows formed. (Two diagrams shown.)

All this being so, the greatly increased traffic, and the increased speed of passage, consequent on the development of mechanical carriages, have caused much attention to be directed to road construction.

THE FUTURE.

What is the road of the future to be? It is a question which all who are associated with the management of roads have come to see calls imperatively for an answer. The problem is to find the best mode by which a road can be constructed which will not have its surface broken by traffic, and will make transit easier both for passengers and goods, and shall neither form puddle holes nor exude mud to clog the vehicles and to form thick dust when the weather is dry; in short, that there shall be no loose material from the road, except the small quantity caused by surface wear, which it is found is but trifling when a sound crust has been rolled in. That such a road can be laid anyone may see by paying a visit to the Thames Embankment, the traffic on which was small formerly, the road being shunned as one of the worst in the country, but which is now used by an enormous number of vehicles, often as many as 1600 in an hour. It will be seen there that water on the surface dries off very quickly, there being no mass of mud to hold it, and that in the driest weather there is practically no dust. No watering is done during the day, the surface receiving one washing during the night, because of the horse traffic. But there is no need for the use of water carts by day. Even during the long drought of 1911 there was no watering, yet there was no appreciable dust.

The necessity for the development of road improvement as a matter of national concern is now recognised, and this has led to the establishment of the Road Board as a Government Department, to the charge of which the money raised by taxation of motor vehicles and motor fuel is handed over to be administered in aid of road improvement. The motor users are thus made to pay a large tax, over and above their contribution to the ordinary rates for road assessment—the new taxes amounting to a sum approaching a million pounds sterling yearly—and so to assist in developing mudless and dustless roads, and roads which shall be durable and keep an even surface. It may give an idea of the amount contributed by those who pay the petrol tax, if I mention that the owners of every motor omnibus in London are mulcted in £45 a year by the Exchequer for the petrol used, a sum amounting from London omnibuses alone to £76,500 a year.

The Board encourages road improvement by giving grants in aid to those road authorities who undertake works of improvement in their districts. The Board have also been conducting, and will continue to conduct, through their engineer and technical advisers experiments both in the laboratory and on the road itself. The laboratory experiments are directed to ascertain what are the best materials, to determine the proportions of the different substances found to be suitable to fix the proper thicknesses for the road crusts which have to bear the traffic, and last, and not least, to endeavour to determine whether any, and if so what, protecting skin shall be placed on the surface so as to come between the hoof or wheel and the bearing crust to protect it from being attacked directly by the traffic.

I shall conclude by calling your attention to some of the results of recent experiments, by which I think you will see that it has been possible for the Board, with the aid of its staff and the experience of numerous surveyors who have been experimenting for many years, to obtain valuable and practical information, both as regards the choice of material, its manipulation, its proportions, and the mode of laying it, which may ensure that good roads can be made, roads which will keep their surface sound for twice as long as the water-bound macadam road, and will not become uneven and break into holes, which was the fate of all the roads of the past. Such roads in their last years were rough, muddy, and dusty according to the weather, deteriorating in a marked manner within a short time after they had been laid down, and in times of heavy and continuous rain having their binding washed out or carried down to the gutters, or to lie thick at the bottom of the slope wherever there was a gradient of any steepness. And let it be realized that in proportion to the degrees in which improvement becomes attainable, not only will the comfort and convenience of the community be augmented, but the wear and tear of horses, of vehicles, and of tyres will be lessened in a marked degree, and the moving of passengers and goods be facilitated more and more, leading to economy in transit. The change will come rapidly. It may seem extravagant to say that in another decade the main roads will have become practically mudless, dustless, and smooth ways; but I do not doubt that my audience will agree with me that if anyone had prophesied on the first day of the new century that before twelve years had passed there would be more than 200,000 motor vehicles upon the roads of Great Britain, that of the 500,000 horses in London 420,000 would have disappeared from the streets, and that reverend rectors who denounced the mechanical vehicles as "engines of Satan," and lashed at motorists with their whips, would have to receive their Bishops on diocesan visitation arriving in motor-cars, the gifts of the faithful, the presumptuous prophet who said such impious things would have been considered fit only for Hanwell or Colney Hatch. So those who hope, as a consequence of the now irresistible march of the power vehicle, to see the road revolutionized for the benefit of the whole community hold the hope confidently, and are willing to be laughed at, if you, Ladies and Gentlemen, choose to do so. . . . We feel sure that we do not go contrary to the Yankee mentor with his "Don't you prophesy unless you know." We do not prophesy, but we do know. We know we say what must happen. And if the words have not become the exclusive property of the politician, I would say, "Wait and see."

Considering what is to be the road of the future, it is unnecessary to say much about the foundation of the road. Telford insisted on a foundation of large stones packed together. Macadam did not consider this to be necessary, and he proved to be right. In the past the road surveyor has recognized what the lower part of the road which is to carry the crust should be, and it need not detain us. But the question "What shall be the weight-bearing crust?" is one of vast importance, and this is engaging the attention of the advisers to the Road Board. I do not intend to dogmatize on the subject, but only to show you what steps of progress are being made—what has already been consummated in the production of roads which are to the old water-bound macadam what the genuine macadam was to the old track of foot deep mud and bulky stones, which before Macadam's day led to much strong language, of which I have given you some specimens, also to the untimely destruction of many a horse and vehicle, and enormous commercial waste of time and material. One thing is now universally recognized, that the road of the future shall be a truly bound road, in which, whatever kind of stone is used—a matter into which there is not time to enter—that stone shall be held together by some pitchy or bituminous material, so that it shall be indeed a crust and not something which has no real cohesion, and into which Macadam's enemy, the water, can make its way whenever water falls. That this result has been attained in a practical way is manifest from the pieces of road crust cut out after they have been under traffic for long periods, and which I now show you. (Specimens exhibited). You will see, if you care to examine them afterwards, that the stones are in their places as they were laid down with their plastic binding, that no water has penetrated and that no stones have been picked out or forced out by the traffic, and that the stones have been held so that they have kept their angularity, and therefore do not tend to come out of the road. Experience tends to show that such a road will remain sound, and that for even twice the period that is possible in the case of a mud-water bound road. That you may see how cohesive such a road is Colonel Crompton, our engineer, has prepared for me these specimens of road, showing how they will hang as a solid piece even with weight attached, which of course would be impossible with water-bound material. It could not be held up at all, but would fall to pieces in the very act of taking it out of the road. (Specimens shown.)

Roads formed as regards the crust in this way are now common. Many can be seen in Kent and other counties near London, and stretches are being laid throughout the kingdom. Great success was attained by many surveyors, and notably at an early date by Mr. Hooley, of Nottingham, in putting together road crusts with the aid of tarry components substituted for mud binding. But there is reason for thinking that possibly it is unnecessary, and indeed disadvantageous, to use any stones of size—even of Macadam's 1½-inch—in making the crust. Experiment is tending to show that suitable stone, reduced to very small size, such as I show you—(specimen shown)—and properly treated with a binder, will produce crust material of a most satisfactory kind. I have here numerous briquettes of such a combination of small stone bound with suitable material which have proved to be of great strength.

It will be observed, if the briquettes are examined, that the cohesion is so close that when the fracture occurs the stone, in many cases, is not pulled out of the structure, but is broken across, its halves remaining embedded in the binding material. The mode followed in testing these tentative pieces of crust has been to set the briquettes on supports at both ends, having 7 inches without support, to apply a gradually increasing weight where the briquette is unsupported until fracture takes place. The figures I have given as to pressure requisite to cause fracture indicate how different is such a combination from the ordinary crust material, which could not even be set on two such supports as these without breaking on a slight pressure, far less stand up to a weight of many pounds; it is the difference between material that will crumble and material that will cling together—between stone set in mud and stone bound with a strongly cohering substance.

I have probably said enough to show that a good road, which shall keep smooth, be impervious to water, and not tend to disintegrate, is now an accomplished fact, and I only need to add that the cost, taken over a series of years, will not be more, indeed there is good ground to believe it will be less, than that of a road as it has been constructed in the past. But the saving to the road user will be very great. Roads will not be heavy in wet weather, putting strain on both horses and vehicles, and giving discomfort to travellers after the rain has ceased to fall. In dry weather there will not be mud gradually turning into a dust carpet, to be whirled about by the wind or thrown up by fast-travelling motor-cars. In this there will be much saving of wear and tear of animals and vehicles, of clothes and tempers, the last being not the least of the evils caused by mud and dust.

One question remains—will it not be well to endeavour to provide an elastic skin or carpet to lie between the vehicle and the bearing crust? This question is also engaging attention. Can we find some material for the exposed surface of the road which shall be resilient, yielding to traffic, but resuming its form and surface? Just as in a golf club house we protect our floor from being dug into and worn away by the hob-nailed shoes of the golfer by using a thin film of semi-resilient covering, such as kamptulicon or ribbed indiarubber, so it is a question whether we may not protect our road floor—the road crust—by covering it with a film of some substance which will yield and recover without disintegrating in the process. The laboratory experiments made seem to indicate that this will be accomplished. There are certain materials which at ordinary temperatures are, though solid to look at, really in a liquid though viscous state. They will flow, but slowly. Their surface if pressed down will yield, but in time will resume its level, and solid bodies, if of lighter specific gravity, placed at the bottom of a vessel containing such a substance will, though not disturbed, float up to the top. A material to be used for an elastic carpet for the road crust must have this viscous liquid quality. Research has been made with pitch and with bitumen, and the conclusion has been reached that pitch will not give satisfactory results, but bitumen will do so. Here is a stick of pitchy material, and you can see that it has very little resiliency when subjected to strain. Here is a bitumen stick of the same size, and you will observe that it is capable of being twisted without fracture and when freed slowly resumes its shape.

It is expected that with such material laid on the top of the main road crust and integrated with it a valuable road protection will be supplied, so that the road crust will be practically permanent, the upper protecting sheet being re-made up and re-laid as required.

Time will not permit that I should go into detail in this matter, and in any view it is still in degree in the experimental stage; but let me indicate how such a protective film will operate. We know that if a wooden floor be left bare, then persons going over it will cause much noise, and if nailed boots are used will more rapidly affect the surface than if felt shoes were worn. As regards the noise, which is the thing most thought of, it is of course a fact that it is only an effect of the motion of the floor caused by shock as it is struck by the successive blows of the feet coming down upon it. Let a person slide over it without raising the feet and there will be little or no noise. But the blows of the feet make the whole floor shake, and the vibration causes the noise. It is by preventing the vibration that we prevent the noise, so we put carpets or kamptulicon or oil-cloth over it. The blows fall just the same, but the covering, yielding and absorbing the effect of the blow, prevents the floor from vibrating under shock, and so the annoyance of noise is cured. It is the same in the case of the road. The horse strikes a blow on the ground at every pace, and the vehicle going over the unevennesses of the road also strikes blows at every turn of the wheels. This operates downwards on the road and upwards on the passenger in the vehicle. The road as we have it suffers from these blows from the stones being caused to move and to lose their sharpness and become loosened in their hold. It is astonishing to find how far in a macadam road the effects of the blows administered to the surface go down. Here are some stones taken out of a macadam road on which there is heavy traffic. They were taken out at a depth of 5 inches below the surface. They show the direct action at that distance down of the blows delivered by the traffic. They have been so knocked about that all their holding surfaces are gone, and they can no longer cling together properly to make a firm road.

But if we can clothe the road with a compressible and elastic skin, which will yield and recover, then the main crust may last efficiently for an indefinite period, and the road be less unkind than it often is to persons and vehicles.

To fulfil these ideas of providing a partially elastic carpet, or topping, as the road surveyors are in the habit of calling it, which will carry out the silencing of the rolling noise of vehicles and reducing shock, our engineers at the Road Board have already decided on experimental roads which will consist of a strong road crust intended to carry the weight of the traffic, and on this to superpose the renewable carpet, and for this purpose Colonel Crompton has kindly prepared for me a number of specimens of road crust showing the arrangement proposed. These are on the table. I would hand them round but they are rather heavy. (Specimens shown.)

For the carpet or topping the case is somewhat different from the crust. Here strength is not of so much importance as the elastic and silencing qualities, and the freedom from liability to produce any dust in summer or mud in winter. Another requirement which is very difficult to meet in this transition age is that of giving a surface good for motor and mechanical transport, and which will not be slippery, and will afford good foot-hold for horses.

The specimens before you show these qualities in various degrees. The carpet must be a compromise; it must not be as hard as motorists would wish for, but just so hard that it will wear a little and yet be cheap and easy to maintain. Our engineers tell us that this ideal road of the future need not be a costly one. On the contrary, that once the matter is thoroughly understood, and the road men trained to the new methods, the rapidly increasing traffic of the time before us will be carried with little or no increase in the cost of maintenance, and probably in some cases with a decrease.

To Colonel Crompton and to Mr. Maybury of Kent, Mr. Hooley of Nottingham, and Mr. Wood of Fulham, I am indebted for much useful aid in preparing this Discourse, and I am also thankful to Mr. Aitken of Fife for his complete "Treatise on Roads," and to Mr. Edwin Pratt for his recently published work on "Inland Transports," from both of which I have derived much assistance in regard to past history. Mr. Lyon Thompson—son of my late friend, Mr. Thompson, the first user of solid rubber tyres, and the inventor of the first pneumatic tyre—has given me much valuable assistance.

Ladies and Gentlemen, I trust the Discourse has been like a good road, neither dry-as-dust nor water-logged. I thank you for your patience, and ask to be allowed to express the hope and to make the request that you will take an interest in road improvement, which will not only add to your own comfort, but will tend to increased commercial prosperity, to greater efficiency of land defence, and last, but not least, will be a powerful agent in improving sanitary conditions, particularly in the towns. May our future roads be in true contrast in efficiency, in durability, and in cleanliness to the roads of the past. Whether you are sportingly inclined, or practically inclined, or philanthropically inclined, or it may be a little of each, give your hearty encouragement to all who are endeavouring to make the road more smooth, more convenient, and more cleanly, and so more healthy. The raising of the quality of the road is a work which can do harm to no one, and must do good to all. Help, then, by your influence those who are engaged in the work of road improvement. In doing this you will not only earn our gratitude, but will do a good work for the whole community, which will not be without reward to yourselves.

[J. H. A. M.]

WEEKLY EVENING MEETING.

Friday, February 23, 1912.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

George K. B. Elphinstone, Esq., M.Inst.E.E.

The Gyrostatic Compass and Practical Applications of Gyrostats.

The employment of a moving body for the indication and control of "direction" has occupied the attention of scientists for many years.

In explaining what is meant by "indication" of direction, let the subject be divided into two parts:—

(1) Indication of direction, or measurement of angles in a vertical direction.

(2) Indication of direction, or measurement of horizontal angles, such as in the case of a Compass.

This property of moving bodies is a fundamental law, laid down by Newton in his "First Law of Motion." "Every body continues in its state of rest, or of uniform motion in a straight line, unless it is compelled to change that state by external forces."

As regards the use of Gyrostats, in order of date:—

The measurement of *vertical angles* is of the first importance in all navigation; the height of the sun or other heavenly body above the horizon furnishing a means for the determination of latitude directly, and of longitude when used in conjunction with a means of knowing Greenwich time.

The earliest record of a practical application of a Gyrostat which the lecturer has been able to trace is of the Gyrostatic Artificial Horizon constructed by Mr. Searson in the year 1744.

Mr. Searson submitted his invention to the Admiralty, and was instructed by them to proceed for a cruise in H.M.S. "Victory." The ship was lost with all hands, being blown against the rocks in a very heavy gale off Ushant in the year 1746.

King George III. expressed considerable interest in Mr. Searson's device, and the Commissioner of the Admiralty submitted some sketches made by Searson to George Adams, an instrument maker of Fleet Street, who constructed for King George III. in 1762 an exact repetition of Searson's Gyrostat which was lost in the "Victory."

This actual instrument is on the table, and can be shown at work, owing to the kindness of the authorities of King's College, who have allowed the lecturer to have this old piece of apparatus out of their museum.

It is of some interest to note that the success of the present Gyro Artificial Horizon, which is really based absolutely on Searson's original model, has taken place within the last six years, or 160 years since Mr. Searson made his original model.

The success of the present form is very largely due to the method adopted of running a revolving Gyrostat in a small chamber from which the air has been exhausted, so that the Gyro keeps in rotation for time enough for practical observations to be made.

There are, of course, many very important practical applications of Gyrostats to determine the amount of a vertical angular movement, and further to use such determination for controlling moving bodies or keeping them level.

By the kindness of Mr. Brennan a working model was shown demonstrating the application of a Gyrostat in his important Monorail Apparatus.

As regards the Second Section of the subject: Indication of direction in a horizontal plane:—

Edward Seng, of Edinburgh, in the year 1836 mentions the possibility of a revolving disc, or Gyrostat, being employed to indicate direction.

Sixteen years later Leon Foucault carried out his experiments, with a pendulum to demonstrate the rotation of the earth, in the Pantheon, in Paris, the pendulum swinging in a definite plane while the earth rotated under it.

Foucault experimented with Gyrostats to replace his pendulum, to demonstrate the earth's rotation, the Gyrostat being delicately suspended so as to maintain its plane of rotation fixed in space while the earth moved under it.

Springs have been used, and perhaps the most important of all the practical applications of Gyrostats is the spring-driven Gyrostat used to control the course steered by a torpedo; there are probably more Gyrostats actually at work in this application than any other.

By the kindness of the Admiralty the lecturer was enabled to show an actual Gyrostat used in a Whitehead torpedo.

So long as the torpedo travels on the course originally assigned to it the plane of rotation of the Gyro travels parallel to its original plane; but if the torpedo for any reason deviates from its course the Gyro remaining on the old course, so to speak, operates one or other of two small valves which control the admission of air to a small cylinder, in which a piston moves attached to the rudder, so that if, for instance, the torpedo deviates to the left, the rudder is turned to the right, and vice versa, and in effect a zigzag course is produced; the more freely everything works the narrower the curved path of travel.

The problem of a *practical* Gyrostatic Compass has attracted the attention of many, but the credit of being the *first* to produce a practical working instrument belongs to Dr. Anschütz, who, with those associated with him, has devoted some twelve years of patient work and no inconsiderable sum of money in experiments. Since then some important work has been done by Hartmann and Braun in Germany, and Mr. Sperry in America, details of which are not available.

Few people have any idea of the difficulties attending the installation and correct adjustment of a magnetic compass on board a large steel ship, and more particularly on a battleship or cruiser, so as to work surrounded by huge masses of steel, and in order to withstand the terrific shocks caused by the firing of heavy guns, and the problem would to-day be impossible had it not been for the theoretical work of Sir George Airy, the applied genius of Lord Kelvin, and the present practical improvements introduced by the Superintendent of Compasses at the Admiralty.

A magnetic needle can only point in the direction of the lines of magnetic force at the place where it is set up, and it is well known that there are very few places on the globe where the magnetic needle points true North and South.

Dr. Anschütz attacked the problem of a Gyrostatic Compass with enthusiasm, and has continued to work at it in the face of many and great disappointments with a thoroughness and patience which is characteristic of his nationality. The construction of the Compass meant new designs for everything in connexion with its motors, etc., etc.

His first experiments were with Gyrostats suspended with the Gyro free to move about its three principal axes, or, as it is termed, having three degrees of freedom; but it is easy to show how impossible it is to construct such a Gyro so as to be sensitive to small movements, and yet really accurate in practice.

To make use of the gravity effect of the earth Dr. Anschütz mounts his Gyrostat in the form of a pendulum; as the earth rotates the Gyrostat tends to maintain its plane of rotation parallel to its original plane in space.

The earth's gravity acts against this tendency, and a precession results, the only position of equilibrium occurring when the Gyro axis has set itself parallel with the axis of rotation of the earth.

Several models were shown to demonstrate this tendency, and to illustrate how the Gyro Compass *sets itself* North and South.

In the actual Compass the friction of the universal joint carrying the pendulum arrangement must be very small for the Gyro to take an ultimate position with accuracy—the length of the pendulum, and hence the effect of gravity, must be small, so as to keep the Compass free from disturbances—and therefore the precession is very slow, and the Compass would swing to and fro on either side of the meridian indefinitely; its mean position would, it is true, be the true North and South line, but valueless for practical use.

One or two slides were shown illustrating the effect of the damping, and a model demonstrated how this was effected by the air-blast set in motion by the Gyro itself.

The sizes, weights, and speeds chosen are such as to result in a Compass having many times the directive force of a Magnetic Compass, and therefore responding to much smaller alterations of direction than can readily be observed with a Magnetic Compass.

The Compass itself being quite non-magnetic can be put down under armour—in a position where a Magnetic Compass could work only with very special precautions and under grave difficulties.

The action of the contact is to control a small electric motor, which moves a plate away from the contact on the card as soon as it touches it, and then the motor stops; the motor drives a transmitting device which controls as many receivers as are wanted in the ship.

A complete Master Compass was shown—which is destined for a very celebrated war ship—and several receivers were shown in action, an electric motor being attached to the binnacle so as to turn it round, and so imitate the movements of a ship relative to the meridian.

The receivers are merely electrical counters, and can be put in any position; the small dials make one complete revolution for only 10° change of course, and these are geared after the manner of clock-hands to the outer dial, one turn of which corresponds to a turn of thirty-two points, or 360°; they are arranged to turn at a quicker rate than any large ship can turn in the water.

The movement which the inner card makes, for a very small alteration of course, is considerable, and takes place instantly; and, owing to this fact, enormous improvement is possible in the steering of a large ship when the helmsmen have become used to the appearance of the dials.

The diagrams on the screen showed actual records taken of the courses in several large warships, and the varying amounts of helm employed to maintain those courses.

On the table was an actual recorder, electrically connected to the receiver circuit from the Master Compass, and tracing the actual movements which the binnacle makes to and fro.

Attention was drawn to the necessity of corrections which have to be applied to the readings of the Compass.

The first correction is an interesting one, as it is not apparent at first sight; it is common to every form of Gyrostatic device which takes the earth's rotation into consideration.

If a ship with a Gyro Compass is steaming due East or due West, the ship's speed is added to the speed of rotation of the earth in space, or deducted from it.

Suppose the ship steams due North, then the resultant travel of the ship in space is along the diagonal line, as it is moving from West to East by the earth's rotation, and South to North by its own steam.

Therefore it is travelling in space about some axis which it sets its own axle and its N. and S. line on the compass card, parallel to which is not the North and South axis of the earth.

The speed of the ship, the course and the latitude, come into this correction, for which tables are made out, the maximum correction which has to be considered being some 3° ; for all manœuvring this correction can be neglected.

The second correction which has to be taken into account is due to the existence of the air blast used in damping, the damping checking the precession whenever this takes place.

The precession varies with the cosine of the angle latitude ; the air blast is constant in its effects in all latitudes, depending only on the speed of rotation of the Gyro—therefore there is a varying cause and a constant retarding force, and in consequence a varying result.

The effect is that for every 10° of latitude a correction of about $\frac{1}{2}$° has to be applied—$\frac{1}{2}$° in a distance of 600 miles.

The Gyro Compasses in use in the English navy are adjusted to be correct at 50° north latitude, so that for all cruising in the Channel and say up to the Firth of Forth, this correction does not require consideration.

Both these corrections can be treated arithmetically by adding to or deducting from the reading on the card the same quantity *all the way round* ; it does not vary in different parts of the card, as is the case when applying a deviation correction to a Magnetic Compass reading.

The worst difficulty which the Gyro Compass is faced with is the effect produced by violent rolling or great vibration in a ship.

This has been receiving a great amount of attention from the inventors during the last eighteen months, since practical experience at sea showed the necessity of some improvements in this respect.

Fortunately the results of the investigations have led to considerable improvement, and to a complete cure of the trouble experienced in this way, so that it will shortly be possible for Gyro Compasses to be installed in ships which are quite independent of the rolling motion or vibration of the vessel.

[G. K. B. E.]

WEEKLY EVENING MEETING,

Friday, March 1, 1912.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

WILLIAM J. S. LOCKYER, ESQ., M.A. Ph.D. F.R.A.S.

The Total Eclipse of the Sun, April 1911, as observed at Vavau, Tonga Islands.

IT is my pleasant duty to-night to give you an account of the expeditions which were sent out to observe the total eclipse of the sun of April of last year visible in the South Pacific Ocean.

Unfortunately, owing to very unfavourable weather, the eclipse was observed in the presence of a large amount of cloud. I am only able therefore to place before you a very few results in comparison with those I could have shown you had we been more kindly treated.

However, an opportunity is afforded me to attempt to give you some ideas as to why expeditions are sent out to observe these phenomena, and how a large expedition is organized when it is known that the assistance of one of His Majesty's ships is available. Let me remind you in the first instance that a total eclipse of the sun only takes place when the moon passes directly between the earth and the sun and completely covers the latter. At such times the brilliant glare of day is discontinued for a brief interval of time, and in this semi-darkness and weird illumination we see objects which surround the disk of the sun that is usually only visible to us.

Total solar eclipses are more common than is generally supposed, for they occur almost every year; but as the apparent diameter of the moon is never much greater than that of the sun, the area on the earth's surface—on which, at any one instant, the eclipse appears total—is very seldom on the average greater than 100 miles in diameter.

The rapid motion of the moon round the earth, part of which is counterbalanced by the rotation of the latter, causes the region on the earth—from which the eclipse can be seen—to move along faster than 1000 miles per hour, producing a shadow track on the earth seldom wider than 100 miles, but sometimes more than 5000 miles long. On this track the eclipse is total some time between sunrise and sunset. On either side of it the sun is seen partially hidden and appears crescent-shaped, but such partial phases are of little or no value to those who study the physics of the sun, and only of passing interest to even the "man in the street" as a spectacle.

Total eclipses, as I may further remind you, occur in families, and each member of a family is separated in time by intervals of about 18 years and 11⅓ days. The eclipse of April 28 of last year belongs to the family the last two deceased members of which lived and died on April 16, 1893, and April 6, 1875, while the next one yet unborn will exist for a brief interval on May 9, 1929.

It is not every total eclipse that lends itself to study, for some are visible only from such unfavourable places as oceans, others from the inaccessible arctic or antarctic regions, and others again from very cloudy districts.

The sum total of time spent in useful observation at all the eclipses which have been observed up to date is very short and amounts probably to less than three hours, for an eclipse cannot last longer than eight minutes, and does not as a rule exceed three minutes. The line of totality of last year's eclipse extended across the Pacific Ocean, commencing at New South Wales, Australia, and terminating in the ocean just to the west of Central America. The central portion of the track passed near the Islands of Tofua, Vavau, Tau, Nassau and Danger Islands. Tofua being an active volcano, and Tau, Nassau and Danger Islands difficult of access, most of the expeditions located themselves on Vavau, where there was a very safe anchorage for ships and where stores were obtainable. All the parties settled close to Neiafu, the chief village of Vavau, and there the duration of totality was computed to be three minutes thirty-seven seconds, or 217 seconds.

In the earlier days of eclipse expeditions those who took part in them had to be content with eye observations alone. The discovery and rapid advance made in the sensitive photographic plate and its successful application in 1860 to eclipse work revolutionized eclipse programmes altogether, so that an abundance of facts may now be photographed in a brief interval of time, and these be examined at leisure, at a less exciting moment.

It is well to remember that many inquiries, which in the earlier days formed part of eclipse programmes, need attention no longer. Thus, for example, the corona was first thought to be the illuminated lunar atmosphere until observations proved it to be a solar appendage. Further, during eclipses, the corona was supposed to be either quickly rotating or pulsating visibly, but subsequent observations have shown that during those times it is apparently as rigid and stationary as an Indian order suspended in the sky.

The prominences, those ruddy brilliant tree-like forms, which appear during totality at the edge of the moon's limb, were also considered as belonging to our satellite, until observations in 1860 demonstrated them as belonging to the sun. While we know that they are solar, there is even now no necessity to waste time during eclipses, on either the study of their forms, positions, or chemistry. The reasons for this are that in 1868 a method was devised by which they can be individually studied *visually* any day when the sun shines, and in 1891 a means was afforded of *photographing* in a few minutes, on one plate, all the prominences situated on the sun's limb.

The solution of these and other problems which might be mentioned are gradually reducing the importance of observing eclipses, and it is well within the bounds of possibility that in the near future all the main solar enquiries will be able to be conducted without waiting for their occurrence.

In recent years, among the most important work of eclipse expeditions, that of the study of the form and chemistry of the chromosphere and corona has taken first place, the chromosphere being that ring of rosy light from which the prominences project.

Even now the research on the chemistry of the chromosphere is in process of being divorced from eclipse work. This is due to the magnificent work that is being carried on at the Mount Wilson Solar Observatory with large-scale instruments. At that Observatory the chromospheric spectrum has been photographed in full sunlight. The method employed, while surpassing, in accuracy of wave-length measures, those made from eclipse spectra, may in time equal or even possibly exceed them in detail.

Thus the chemistry and form of the corona are practically the only large inquiries which are restricted to eclipses, and probably we may not have long to wait before even these form part of the daily routine of solar physics observatories situated in good observing localities.

Time will not permit me to tell you even briefly how the special results obtained during eclipses help the advancement of solar and celestial physics.

When it is remembered, however, that our sun has a temperature of about 7000° at its surface, and perhaps several hundreds of thousands of degrees at its centre—that the very sunspots which appear to us as black spots on his surface are brighter than the brightest arc lamp—then the importance of the study of every attainable part of this very effective group of furnaces in and out of eclipse is imperative for the advancement of knowledge.

If one be permitted to refer briefly to the progress of our knowledge of the form, origin, and chemistry of the corona, you are well aware that its shape is not the same at every eclipse, but that there seems to be a systematic change going on, extending over several years (Fig. 1). A study of these forms has shown that the changes repeat themselves about every eleven years, and since the mean daily areas of sunspots are known to have a periodicity of this length of time, their close association is generally conceded. It happens, however, that when the coronal streamers are most prominent in highest solar latitudes, and when at the same epochs the mean daily spotted area is at a maximum, the mean latitude of the spotted area is very low and only about 15°. Thus there seems reason to question the conclusion that sunspots at such a low latitude can originate coronal streamers so distant as the solar poles.

It must not be forgotten that a study of the frequency of solar prominences has disclosed the fact that not only have these phenomena a periodicity of about eleven years, synchronizing exactly with the spotted area, but that when their frequency is at a maximum, they are conspicuous at the highest solar latitudes. Thus there occur at the same time prominences and coronal streamers near the solar poles, a very possible and probable condition for cause and effect.

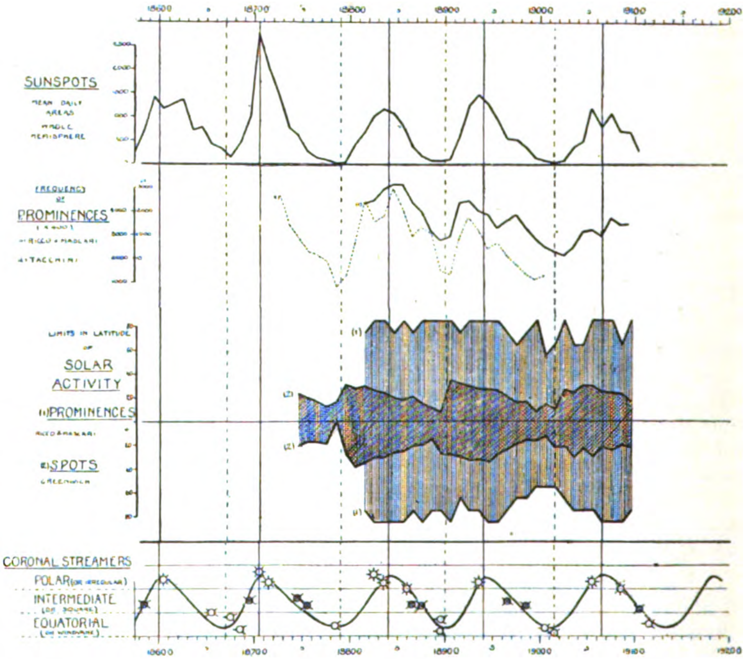

FIG. 1.—CURVES TO SHOW THAT WHILE THE DIFFERENT FORMS OF THE CORONA EXHIBIT A REGULAR VARIATION, CORRESPONDING IN TIME TO THOSE OF SUNSPOT AREAS AND PROMINENCE FREQUENCY, IT IS THE PROMINENCES (WHICH, UNLIKE THE SPOTS, ARE NOT LIMITED TO ANY LATITUDE) THAT ARE RESPONSIBLE FOR THE VARYING SYSTEMATIC CHANGES OF FORM OF THE CORONA.

In the eclipses of 1901 and 1905, several striking photographs were secured illustrating intimate association between prominence and the overlying coronal material, thus affording further evidence of their close connexion.

While, therefore, prominence activity is most likely responsible for providing and raising the material from the body of the sun in the various latitudes according to the different epochs of prominence activity, what action is it that organizes and arranges the streamers which extend sometimes five or six millions of miles into space ?

The close association between the occurrences of terrestrial magnetic storms and solar disturbances, and the results of the researches which were described in this Institution in 1909, namely, the discovery of solar vortices and the presence of powerful magnetic fields which result from the revolution of the negatively charged particles, termed corpuscles, in them—these suggest strongly a cause, namely electromagnetic action, to explain the effect.

So long, then, as the corona can only be observed during eclipses, the study of its general form and its structure in close proximity to prominences should be minutely recorded and discussed.

About the chemistry of the corona nothing is known. The spectroscope on many occasions has permitted observers to photograph the many radiations that it emits, and while numerous determinations of the wave-lengths of these radiations have been made, no terrestrial equivalents have yet been discovered. Thus its composition is still a mystery.

For the occasion of last year's eclipse the main work intended to be accomplished by the expedition of which I was in charge was a study on a large scale of the spectra of the chromosphere and corona together with the form of the corona.

For the spectroscopic work, two large instruments giving large dispersion were constructed, adjusted and taken out, and several coronagraphs of different focal lengths, namely 16, 7, 4 and 3 feet, were utilized for recording the form of the corona.

The large spectroscopes were of two kinds, one being of the "slit" type employing a 10-foot concave grating, while the other was of the "slitless" or prismatic camera type, in which the dispersion was secured by four six-inch prisms of 45° angle. With the former it was specially intended to secure photographs from which the wave-lengths of the lines in the spectra of the chromosphere and corona could be determined.

This slit form of instrument is the most efficient for such measures, for the spectrum consists of images of the fine slit and appears as a series of sharp fine lines, the wave-lengths of which can be accurately measured.

Such a spectrum, however, tells one nothing about the form of the object the radiations from which are being studied, but simply about the chemical composition of that portion of the image the light of which passes through the slit.

Now, the case is different with the prismatic camera, a form of instrument first used visually in the eclipse of 1871 and photographically in 1875. In place of the individual monochromatic images of the slit, monochromatic images of the whole object itself take their place. While the degree of accuracy in the measurements of the wave-lengths is somewhat less than in the slit form, the great advantage gained is that the form of the object photographed is given.

Thus in the case of the application of this instrument to eclipse work, it is possible to determine and differentiate between the chemistry of the chromosphere and corona; to measure the depths of the chromospheric layers; to locate the regions of strongest coronal radiations, besides securing the forms, positions, and composition of each prominence if such data were desired.

It will thus be seen that the use of both kinds of instrument is very desirable for eclipse work, as they supplement each other with regard to the results that can be derived from them.

For the observation of the eclipse of last year, two official and one private party proceeded from England; Australia was represented by a large party of observers, under the leadership of Professor Baracchi, and two other observers went out separately.

One of the British official parties consisted of Father Cortie, S.J., and Brother McKeon, representing the Eclipse Committee of the Royal and Royal Astronomical Societies, and the other was composed of myself representing the Solar Physics Observatory of the Board of Education. These parties left England on February 3, by the Orient Liner s.s. "Otway." I had previously made arrangements that Mr. Frank McClean should join me as a volunteer assistant, and he boarded the "Otway" at Marseilles. It was through him that other valuable volunteers in the persons of Mr. John Brooks of Sydney, Mr. Henry Winkelmann of Auckland and Mr. Raymond of the Sydney Observatory were secured, and I was fortunate on my way to Australia to obtain additional volunteer help in the form of Mr. R. C. Anderson, of Southampton.

Some time before leaving England we had been informed that the Admiralty had placed H.M.S. "Encounter," of the Australian Squadron, at the service of both the Official British parties, to take us to Vavau, assist us with our observations, and return with us to Sydney. In our preparations, therefore, a considerable increase was made in the instrumental outfit, because experience has taught us that a much greater programme of work can be attempted when we have such skilled assistance as is afforded by the officers and men of a man-of-war.

On our arrival at Sydney, on March 16, we at once put ourselves in communication with Captain Colomb, of H.M.S. "Encounter," and made all the necessary arrangements with him. Sydney was left on March 25, and on the 2343 mile run to Vavau, volunteers were called for from the ship's officers and men, and more than enough sent in their names. Activity was displayed not only in giving lectures to make known the kind of assistance that was required, but subsequently in arranging the different branches of work among the volunteers. The following list shows the sub-division of labour as finally adopted for my party, but it must not be forgotten that several officers and men in addition to the above assisted Father Cortie's party.

DISTRIBUTION OF WORK.

—	Assisted by	
	Officers	Men
6-inch Prismatic Camera Group. Dr. Lockyer in charge	1	8
10-foot Concave-Grating Spectrograph Group. Mr. McClean in charge	2	9
4-inch Coronagraph Group. Mr. Brooks in charge	2	11
6-inch Steward Eql. Group. Mr. Anderson in charge	..	3
Photographer Group. Mr. Winkelmann in charge	..	2
Eclipse Clock Group. Capt. Colomb, R.N., in charge	..	2
Shadow Band Observing Group:—		
On Ship, Comm. Mellor, R.N., in charge	..	3
At Camp, Eng.-Lieut. Weeks, R.N., in charge	..	3
,, Lieut. Billing, R.M.L.I., in charge	..	3
Small Cameras, Landscape Moon Shadow, Stars, Animals:—		
At Camp, Staff-Surgeon Milln, R.N., in charge	..	20
On Ship, Comm. Mellor, R.N., in charge	1	8
Muikilekila, P.O. Taylor, in charge	..	7
Talau Hill, C.P.O. Corral in charge	..	8
Disk and Corona Sketching Group. Lieut. Hunt Grubbe, R.N. in charge	2	16
Meteorology. Lance-Corp. Beirne in charge	..	..
Chronograph Record. P.O. A. J. Davis in charge	..	..

Total.—Officers 14
Men 107

Vavau was reached on April 2, and the anchor was let go off Neiafu, the chief town, at 3.40 p.m.

The medical officer of health reported that Vavau had recently been suffering from an epidemic of measles, that there were still a few cases, one of them being a European. This condition suggested at once that a camp some distance from Neiafu would be preferable to one in or near the village, not only for my own party but for the ship's company. As a matter of fact there was no site in the town that could have accommodated our large camp, for the Australian observers had pitched their tents on the only large site cleared.

For some time I had had in my mind as a possible site a spot marked on the chart as a good position for an Admiralty coalingstation, about a mile and a half up the harbour. The next day this was inspected, and it seemed so admirable in every way that it was at once decided to utilize it and start work.

H.M.S. "Encounter" changed her position and anchored off the site the same afternoon.

I may add here that Father Cortie and his party had intended to set up their instruments at the Roman Catholic mission grounds, but he finally decided to camp alongside us and utilize our time signals.

Work was commenced at once to clear the ground for the eclipse and living camps, to cut paths and to erect landing-stages and steps. The various groups of volunteers confined themselves to their several duties, and general working parties were formed for fetching dead coral, sand and water for the concrete pillars, for building the pillars and putting together and covering the instrument-huts and dark room.

The rest of my party, which consisted of Messrs. Brooks, Winkelmann, and Raymond arrived by the s.s. "Atua" from Sydney on April 4, so that the camp became a locus of great activity.

The weather experienced for the first week was all that could be desired in the way of absence of rain, but the high humidity, coupled with a high temperature and the presence of millions of flies and thousands of mosquitoes, rendered the work of the camp formation extremely arduous. In the water we had other enemies in the form of sharks and sea-snakes.

At a later stage a rainy type of weather set in, and it was the exception to have a day free from it. Tropical downpours were very frequent, and special precautions had to be taken to have efficient coverings for every instrument and to trench the small compounds in which each instrument was enclosed.

By April 20, arrangements were sufficiently advanced to warrant the commencement of rehearsals, and on six days before the eclipse these rehearsals took place (Fig. 2).

It is not necessary here to give in detail either the dimensions of the different instruments or the various programmes of work which were intended to be carried out. These can be obtained, if desired, from a report which is in progress of publication by the Board of Education. Fig. 3 shows the general arrangement of one of the instruments, namely the 6-inch prismatic camera and staff.

During the period occupied in preparing the instruments for the eclipse, transit observations were being conducted for time and position, meteorological observations were being made at fixed times, and continuous records were being secured by a barograph, thermograph, and hygrograph.

In order to eliminate any doubt as to the possible inaccuracy of the computed times of the contacts of the limbs of the sun and moon, and also to give certain prescribed signals to those observers whose programmes necessitated them, a special telescope was set up (in conjunction with the siderostat of the six-inch prismatic camera) to

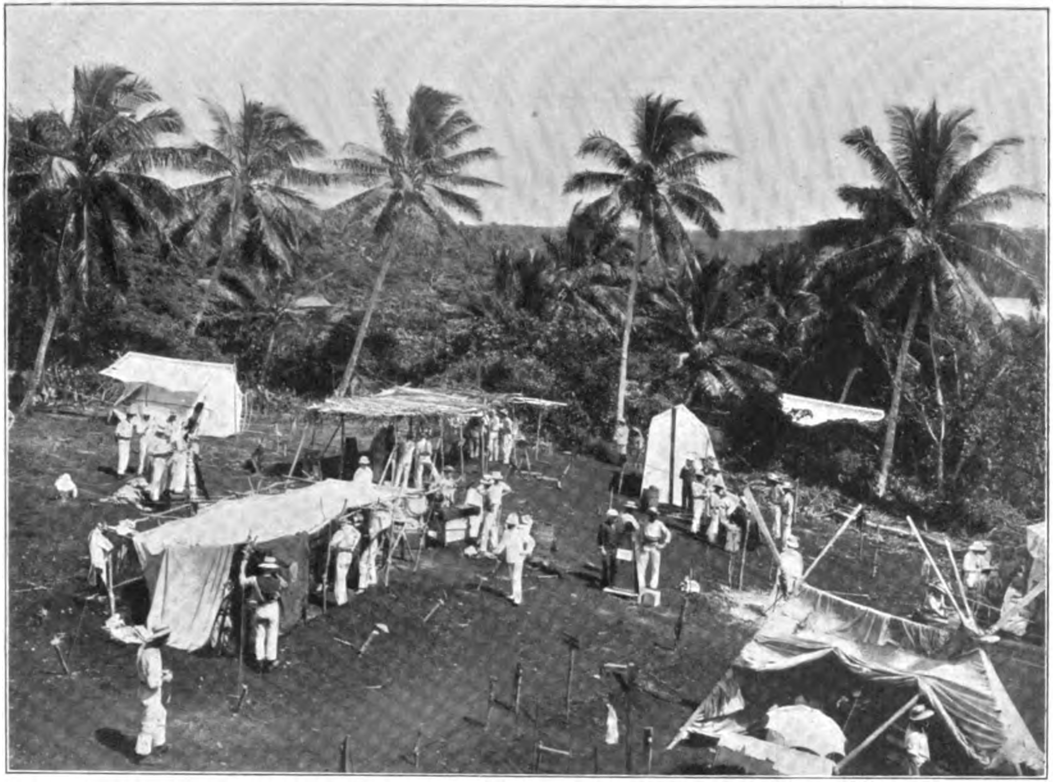

FIG. 2.—THE ECLIPSE CAMP AT VAVAU DURING A REHEARSAL BY THE SOLAR PHYSICS OBSERVATORY'S EXPEDITION.

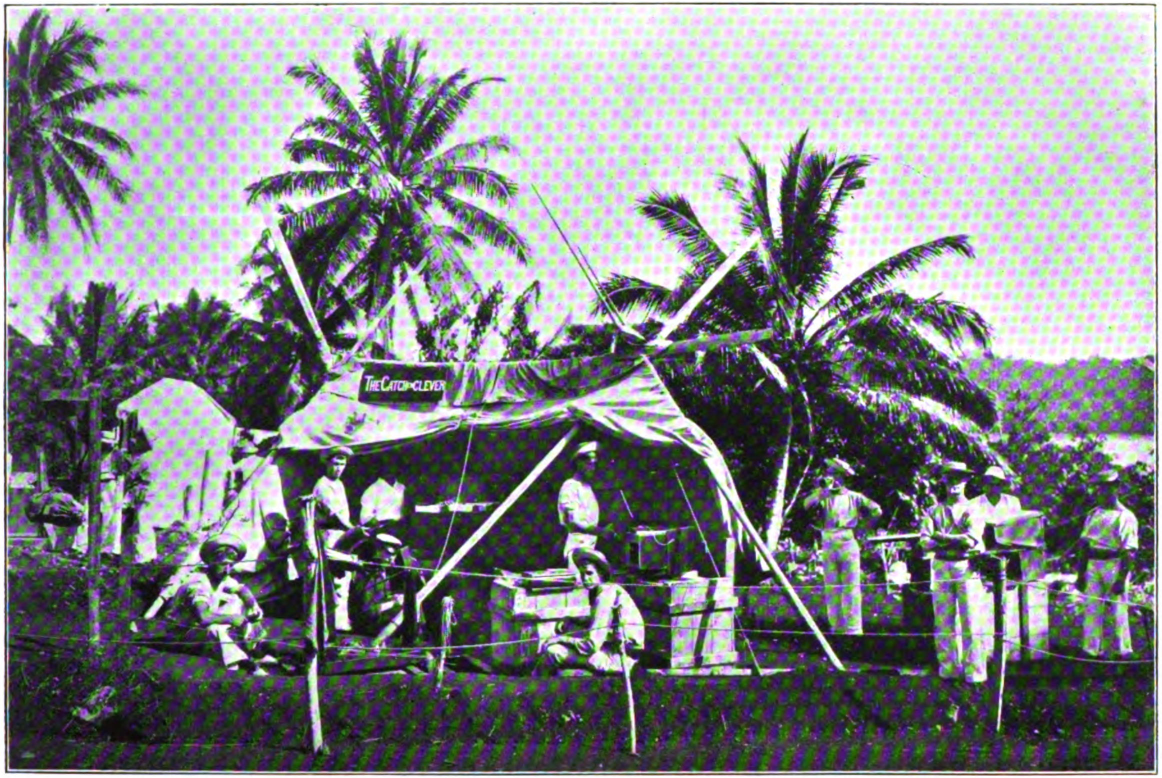

FIG. 3.—THE 6-INCH FOUR-PRISM PRISMATIC CAMERA AND STAFF, ALSO (ON THE RIGHT) THE CUSP-TELESCOPE AND STAFF FOR GIVING THE TIME SIGNALS. SOLAR PHYSICS OBSERVATORY EXPEDITION.

throw an image of the sun on a previously-marked disk (see Fig. 3, right-hand side). The face of this disk was so graduated as to enable the observer to estimate the angle subtended at the centre of the dark moon by the remaining bright crescent of the uneclipsed portion of the sun; previous calculations had shown that when the crescent subtended angles of 90°, 45° and 30°, there remained 42, 9 and 4 seconds respectively before totality began. This method only holds good when clouds do not obstruct the view of the sun.

In order to allow for the contingency of second contact not being seen on account of clouds, the observer was furnished with a deck-watch to give all the necessary signals at their computed times. For the actual calling out of the time during the eclipse, a special "eclipse clock" was taken out. This clock has a seconds pendulum, and a special dial is made for each eclipse. At the commencement of totality the clock is set going by releasing the pendulum held by a thread, and the hand (there is only one) makes one revolution a minute. The dial is marked in the form of a spiral, and each second of the duration of totality is marked on it. The time called out by the timekeepers is always the number of seconds remainng before third contact, or the end of totality. On the present occasion totality was computed to last for 217 seconds, so that the timekeepers called out "217," "215," "210," "205," and so on until 10 was reached, when they called out each second. The duty of the observer at the cusp telescope was to be responsible for giving all the time signals up to the beginning of totality, and at his signal "go," the time callers took up the duty of calling out the time. Two men were employed at the clock, one to call the first half and the other to call the second half, so that each man might have an opportunity of seeing the eclipse. The actual code of signals was as follows :—

10 minutes before totality (wind clocks, caps off, lamps lit)	Bugle	"Rouse up"
5 minutes before totality	Bugle	"Alert"
42 seconds ,, ,, ...	3 blasts (whistle)	
9 ,, ,, ,, ...	2 ,, ,,	
4 ,, ,, ,, ...	1 ,, ,,	
Totality begins, "217" on eclipse clock	Voice	"Go"
Totality ends, "0" on eclipse clock .	Voice	"Stop"

Eclipse day, April 28th, or, as we had not altered our date, since we crossed the "date line," April 29th dawned. It was a cloudy morning, there was a considerable amount of roll cirrus and cumulo-nimbi were not absent. In fact it was much too cloudy to make one believe that it would clear up for the eclipse.

The order of the day was for all parties to be at their stations early, for first contact would occur at 8h. 46m. 37s. a.m., and totality would commence at 9h. 37m. 1·7s. a.m., and there were certain preparations still to be made. These stations were four in number, namely (1) on board, (2) eclipse camp, (3) Muikilekila Hill and (4) Talau Hill.

The moment of first contact was seen between breaks in the clouds. After this the cirrus cloud not only seemed to grow more dense, but to cover a larger area of the sky. To the south (the eclipse was in the north-east) the sky was perfectly clear and blue, not a cloud to be seen there. The crescent sun could still be easily observed through the cirrus, but a dense black billowy cumulo-nimbus was working its way from the east. Just before second contact its outliers had already passed in front of the crescent, and the main cloud soon blotted out the whole region of the sun's position. For the first portion of totality I was busy with my instrument, but during an exposure of 78 seconds for one of my plates, when I went outside my tent, I could not even see any sign of the eclipsed sun. It was only towards the end of totality, when I was exposing a plate for 63 seconds, that the cloud was sufficiently thin to allow one to catch a glimpse of the greater portion of the corona and make a sketch of it. I knew that under these circumstances little, if anything, would be gleaned by our large-scale instruments, and nothing very useful by those of smaller aperture.

I may explain here that as it seemed very probable that a heavy downfall of rain would occur, I did not take the cover off my tent for the eclipse as I usually do on such occasions. As my programme of work necessitated me making some exposures for the chromosphere at second and third contacts, I could not look at the eclipsed sun about those times. After the eclipse was over, however, I soon became acquainted with the facts that totality began and ended before the actual calculated times. The following table shows the observed and calculated times, and it will be seen that second contact occurred about 23 seconds before the expected time, and the duration of totality was nearly five seconds shorter than was anticipated.

—	Contacts		Duration
	Second H. M. S.	Third H. M. S.	M. S.
Calculated (Downing)	9 37 1·7	9 40 38·5	3 36·8
Observed (Lockyer)	9 36 38·6	9 40 10·7	3 32·1
Difference .	23·1	27·8	4·7

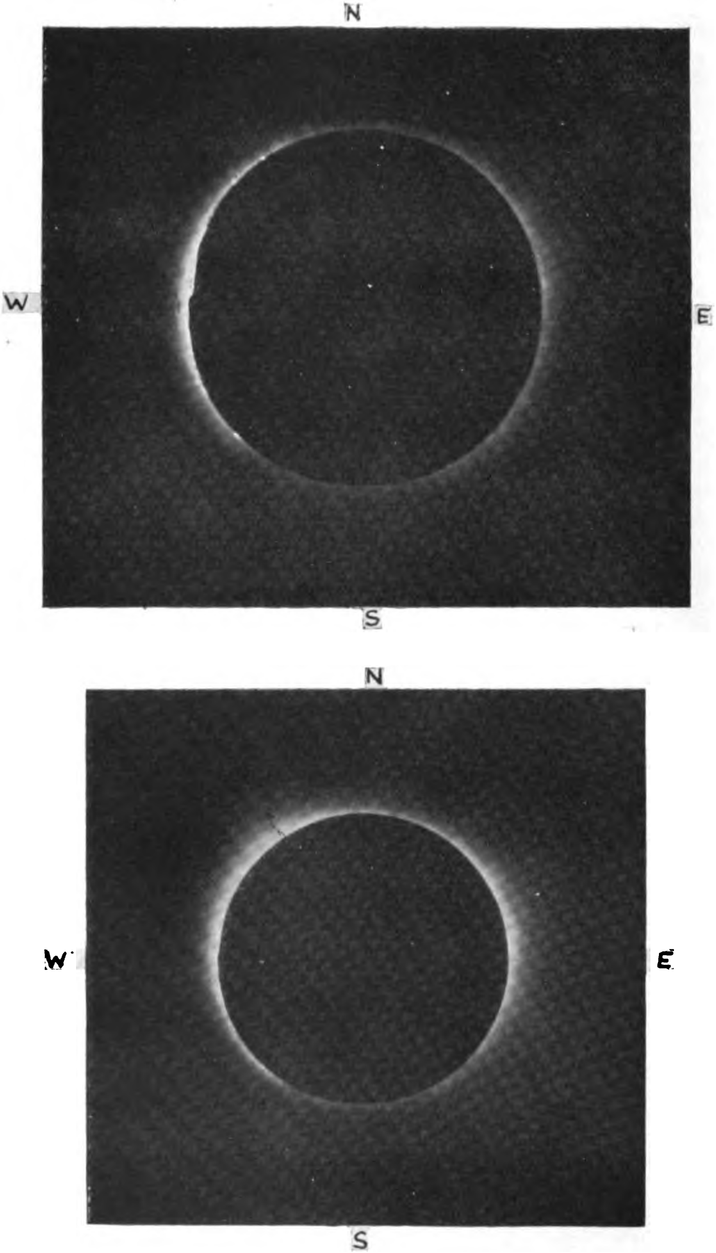

Fig. 4.—Photographs of the Lower Corona, taken with the 4-inch (aperture), 16 Feet Focal Length Coronagraph (upper) and a 4⅝ De la Rue Coronagraph of 8 Feet Focal Length (lower) of the Solar Physics Observatory Expedition.

Large differences of time were experienced also by the Australian observers, whose time arrangements were quite independent of those of my party.

After the eclipse was over, and all the exposed dark slides had been dispatched to the dark room for subsequent development in the evening, the dismantling and repacking of all the instruments were commenced in earnest; before the working parties returned to the ship in the late afternoon, most of the instruments were safely again in their cases and placed on rafters in the instrument tents. It was very fortunate that so much of this packing up had been accomplished, for that night a heavy tropical downpour commenced, and it continued almost unceasingly the whole of the next day and intermittently during the following days.

There is little doubt that the gradual fall of temperature during the eclipse, which subsequent examination was found to be 5° F., favoured the conditions of cloud formation in such a humid atmosphere, and thus prevented us from making satisfactory observations.

The work of the development of the exposed plates, which was proceeded with and completed under these trying circumstances, soon permitted us to gain some idea of the photographic results obtained, and these, as was anticipated, were very meagre.

With the spectroscopic cameras practically no results of any value were secured, while in the case of the coronagraphs nearly all the negatives displayed strong images of the clouds which marred the coronal streamers. Only two of the large number of plates exposed are of value, and these are restricted to the structure of the lower corona, Fig. 4.

The work of all the other groups, such as those for sketching the corona, for the observation of the shadow bands, shadow phenomena, etc., was all for the main part spoilt by the presence of the clouds, in spite of the care taken in widely distributing the parties.

The only parties the observations of which could not be interfered with by the eclipse being a cloudy one were those who were told off to observe closely the effect of the eclipse on animals, birds, etc.

The rapidly increasing darkness as totality drew nigh suggested all the appearances of nightfall, and the animal world evidently took for granted that such was the case.

The animal observers at *Talau Hill* reported that while two horses did not seem to notice the darkening, fowls ran home as if to roost, flowers were observed to close up, and the insects sang as if at night.

From *Muikilekila Hill* the report was that birds and animals went to sleep. Near the *Observatory site*, while horses remained standing, fowls went to roost, pigs lay down, and insects chirped. On board *H.M.S.* "*Encounter*" the chirping of insects was plainly heard, and this was apparently even louder than at night.

Perhaps I might relate here my own experience regarding the noise of the insects. Every evening at the time of sunset, the chirping of insects would commence, at first by a few here and there, and then in about 20 seconds by their whole population, the noise gradually crescendoing until they were all in full song: this chirping continued all night. In fact, I believe that if one were blindfolded a good determination of the time of sunset could be made.

On eclipse day all was silent, even close up to the time of second contact, when suddenly, and I suppose this must have occurred at the moment of totality, this chirping burst forth in all its vigour from the whole region around, and continued until totality was over. The noise, in contrast with the absolute stillness of the camp, where every man was more or less strung up with the excitement of the eclipse, was most impressive, and will remain in my memory as a marked feature of that occasion.

While my party, together with that of Father Cortie, fared very badly, the Australian observers were more fortunate, and Mr. Worthington and his staff more fortunate still. With regard to the results secured by these parties I can only show you, in the case of the Australian observers, a combined sketch carefully made from Mr. Dodwell's negatives and visual observations. This shows clearly the extensive equatorial streamers and the open spaces at the solar poles filled with the beautiful forms of the polar plumes. This corona is undoubtedly one of the "minimum" type, representing the wind-vane form. When this was compared with the sketch I made at the time of the eclipse, it was seen that both were in fair agreement in most of the main features.

Through the kindness of Mr. Worthington, I have been allowed to show you some of the beautiful results which he secured. Unfortunately, like us, he did not obtain any spectra, although he was equipped with a very fine instrument, specially adapted for the ultra-violet region of the spectrum, but unlike us he was compensated by success on other lines. Both the long and short exposures with his coronagraphs met with success. Beautiful structure is displayed in the regions of the solar poles, and the equatorial streamers are extensive and full of detail. These photographs also exhibit a "minimum" type of corona, corroborating the observations of the other parties; they are of considerable value as records and for future study, and form the main contribution to solar physics which this eclipse has afforded.

While the astronomical results of my party were chiefly negative, we managed to get together at odd moments a collection of specimens for the Natural History Museum at South Kensington, the Botanical Gardens at Kew, and the Physic Garden at Chelsea. Captain Colomb set the good example of undertaking the butterfly, moth and beetle department, and the other officers eagerly took up the other branches. All the collections have been handed over to the Institutions above mentioned, and the acknowledgments received have shown that the work was not done in vain.

In concluding this account I should like to place on record in this Institution the fine way in which the volunteer observers of my party worked under sometimes very trying circumstances; the magnificent assistance rendered by the Captain, officers and men of H.M.S. "Encounter"; the great liberality of the Orient Steam Navigation Company in again transporting out and home all our instruments, baggage, etc., free of charge; and lastly, the assistance of many individuals who at various stages of our journey made matters as easy as possible for us.

[W. J. S. L.]

GENERAL MONTHLY MEETING,

Monday, March 4, 1912.

His Grace The Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Douglas Heriot Baird, Esq.
Miss Anne E. A. Baker,
Commander Virgoe Buckland, R.N.R.
William Thomas Burgess, Esq., F.I.C.
Andrew Jameson Matthews Duncan, Esq.
Hardman Arthur Earle, Esq., M.Inst.M.E. M.Inst.E.E.
Miss Cicely H. Farmer,
Percy G. C. Forster, Esq.
John Allen Harker, Esq., D.Sc. F.R.S.
The Hon. Sir Charles Montagu Lush, K.C.
Mrs. Therese Said-Ruete,
C. F. Smith, Esq.
Miss Catherine M. Stainton,
W. A. Tait, Esq., M.Inst.C.E. F.R.S.E.
Basil T. Timotheieff, Esq., B.D. M.I.A.I.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to J. C. Simpson, Esq., for presenting to the Institution the following original letter from Mr. G. Riebau, the Bookbinder to whom Faraday was apprenticed:—

Copy. .

Sir,

I trust the following may be of some service to your numerous readers particular the younger class to inspire them with emulation, as it points out the reward of industry, unexpected by the Object itself.

It is an account of the progress of genius in an Apprentice, whose name I am fully persuaded will be well known in a few years hence, should Providence please to grant his safe return to His Parent, relatives and Friends in London.

Michael Faraday, the second Son of an industrious working Blacksmith of Weymouth Street, Marylebone, (And where his Mother now lives in a respectable situation; his Father died a little before he was out of his time) was bound Apprentice Oct. 7, 1805, to learn the art of Bookbinding with Stationary and Bookselling. In the early part of his time, after the regular hours of business, he was chiefly employed in drawing and copying from the Artists Repository, a work published in numbers which he took in weekly—also Electrical Machines from the Dictys. of Arts and Sciences and other works which came in to bind, this led him to make one himself and succeeding in that, he attended Mr. Tatum's Lectures in Salisbury Court, Fleet Street, and on my advising him to take down in Short Notes (for he was not acquainted with Shorthand) the heads of the Lecture and when he came home to commit their recollections to paper, the consequence of which he was soon able to write the whole lecture and drew the various Apparatus used by the Lecturer, at other times he would make the *Philosophers tree* in a large Vial, with Zink and Sugar of Lead—then turn to the blow pipe and would meal or melt Glass to any form he wanted for his purpose, converting in a very ingenious manner some broken things to use—he also bought some volumes of Enfields Encyclopedea occasionally making drawings of Engines, and figures of Machines to illustrate the subjects treated of. Dr. Watt's Improvement of the Mind, was then read and frequent took in his pocket, when he went an early walk in the morning, visiting always some works of art, or searching for some Mineral or Vegetable curiosity—Holloway Water Works, Highgate Archway, W. Middlesex Water Works, Strand Bridge, Junction Water Works &c &c sketching the Machinery, calculating the force of Steam Engines &c his mind ever engaged, besides attending to Bookbinding which he executed in a proper manner.

Galvanism, Optics, Perspective, occasionally taking a portion of his time, he now added the Vials, lining them with tin foil and cutting curious devices, making an Electric Battery and other pleasing and useful amusements, the ornaments of Head and Tail piece in French and other books which were of novel kind he would copy in a very neat manner with pen and ink as neat as the original on copper plate—his mode of living temperate, seldom drinking any other than pure clean water, and when done his day's work, would set himself down in the workshop regardless of his fellow Prentices or the Servants in adjoining kitchen. If I had any curious book from my customers to bind, with Plates, he would copy such as he thought singular or clever, which I advised him to keep by him. Ireland's Hogarth, and other graphic works, he much admired. Chemistry in 4 Vols he bought and interleaved great part of it occasionally adding Notes with Drawings and Observations. At Mr. Tatums he fell in company with some other young men and occasionally corresponded with them on subjects of Chemistry several letters to and fro he showed me, he had now written Four quarto vols of lectures with Drawings &c on different subjects, these I occasionally showed to my Friends and Customers. I happened one evening to show them to Mr Dance, Junr. of Manchester St. who thought them very clever—and who in a short time returned and requested to let him show them to his Father. I did so, and the next day Mr Dance very kindly gave him an admission ticket to the Royal Institution, Albemarle Street. He attended and took down Sir Humphry Davy's Lecture which he afterwards wrote out and making drawings of the different apparatus used—this he took also to the above Gentleman who was well pleased. His Apprenticeship being now nearly expired he bound up his books with a Portfolio of extra Drawings in October 1812.

He then commenced Journeyman and received a Guinea and half per week, which I think very fair wages for a young man just out of his time—he would occasionally call on me and expressing a wish to be introduced to Sir H. Davy, I advised him to write a letter and take his Manuscript Books and Drawings, and leave them for Sir H. Davy to examine, he did so, and next morning the footman brought a note requesting to see him, he attended. Sir Humphry enquired into his circumstances and told him to attend to the Bookbinding, and if any opportunity occured he would think of him. Soon after this Sir Humphry met with an accident from the bursting of some glass, part of which flew into his eye, he sent for M. Faraday who transacted some business to his satisfaction, after Sir Humphry's return from Woburn, a vacancy happening in the Royal Institution it was offered to young Faraday, he accepted it in March, during his stay he received a valuable present in books. Sir Humphry resigning the Lectureship, Mr Brand began and Faraday attending on him till Sept. about the end of which Sir Humphry returned from a tour to Scotland—and engaged him as his Secretary at a suitable salary to attend him in his travels, thus was this young man at the end of one year from his Apprenticeship placed in a situation of correspondence with the Learned Societies of Europe, and gaining that experimental knowledge which joined to his natural industry will, I think, bid fair to place him in a very exalted situation on his return.

I should observe the Gentlemen of the Institution made him a very liberal present independent of his Salary on his taking leave of them.

On the 12 October 1813—Sir H. Davy, his Lady—Michael Faraday—her Maid—and a footman left Berkeley Square for Portsmouth from whence they embarked in the Collingwood Cartel with their Traveling Carriage and Four—for Morlaix—he is expected to go to Paris—Italy—Greece—Egypt—Constantinople—and perhaps return in five or six years.

I am, Sir, Yrs sincerely
G. RIEBAU. 2 Blandford St.

The Honorary Secretary announced the decease of the Right Hon. Lord Lister, O.M. P.C. D.C.L. LL.D. F.R.S., on the 4th of December last.

The Members of the Royal Institution, of which he was for twenty years a Member, and for several years a Manager, desire to join with the whole civilized world in expressing their sorrow for the death of that illustrious surgeon and man of science, Lord Lister, and in recording their profound sense of the vast and incalculable services he has rendered to humanity in the saving of life and in the alleviation of suffering. Guided by the results of physical and chemical investigation, Lord Lister inaugurated more than half a century ago a new method of the treatment of wounds, which, against strenuous opposition, with modest persistence, he defended and advanced, which has now become the one sure foundation of modern surgery, and the influence of which is felt in every department of medical practice, and in the domestic habits of the people.

Lord Lister's achievements as the apostle of scientific cleanliness have made a new epoch in the hygienic history of mankind, and his memory will be for ever revered as a benefactor of the human race. Endowed with true scientific genius, Lord Lister brought unwearied patience to bear on his own researches, and at the same time maintained a wide survey over the whole field of science, and was thus able to fill with singular distinction the office of President of the Royal Society. The first medical Peer, he added a new lustre to the Upper Chamber of the Legislature, while his personal charm of manner endeared him to all with whom he was brought into contact. Nowhere can Lord Lister's life work be more highly appreciated than in the Royal Institution, and the Members, while offering their condolence to his family on his passing away, full of years and honours, would register their homage to his discoveries and achievements.

The following After Easter Lecture Arrangements were announced :—

EDMUND GOSSE, Esq., LL.D., Librarian to the House of Lords. Two Lectures on ALGERNON SWINBURNE: HIS LIFE AND WORK. On *Tuesdays*, April 16, 23.

FRANK BALFOUR BROWNE, Esq., M.A. Two Lectures on INSECT DISTRIBUTION, WITH SPECIAL REFERENCE TO THE BRITISH ISLANDS. On *Tuesdays*, April 30, May 7.

PROFESSOR WILLIAM BATESON, M.A. F.R.S., Fullerian Professor of Physiology, Royal Institution. Two Lectures on THE STUDY OF GENETICS. On *Tuesdays*, May 14, 21.

PROFESSOR W. M. FLINDERS PETRIE, D.C.L. Litt.D. LL.D. F.R.S. F.B.A., Edwards Professor of Egyptology, University College, London. Two Lectures on THE FORMATION OF THE ALPHABET. On *Tuesdays*, May 28, June 4.

PROFESSOR ARTHUR W. CROSSLEY, D.Sc. F.R.S. Sec. C.S., Professor of Chemistry, Pharmaceutical Society of Great Britain. Two Lectures on SYNTHETIC AMMONIA AND NITRIC ACID FROM THE ATMOSPHERE. (*Illustrated by Experiments and Views of the Norwegian Works.*) On *Thursdays*, April 18, 25.

PROFESSOR J. NORMAN COLLIE, Ph.D. LL.D. F.R.S., Professor of Organic Chemistry, University College, London; Author of "Climbs and Exploration in the Canadian Rockies." Two Lectures on RECENT EXPLORATIONS IN THE CANADIAN ROCKY MOUNTAINS. On *Thursdays*, May 2, 9.

PROFESSOR HOWARD T. BARNES, M.A. D.Sc. F.R.S.(Canada), Macdonald Professor of Physics, McGill University, Montreal. Two Lectures on ICE FORMATION IN CANADA: 1. THE PHYSICAL ASPECT; 2. THE ECONOMIC ASPECT. (*Illustrated.*) (*The Tyndall Lectures.*) On *Thursdays*, May 16, 23.

PROFESSOR J. H. POYNTING, Sc.D. F.R.S., Professor of Physics, University of Birmingham. Two Lectures on THE PRESSURE OF WAVES. (*Experimentally Illustrated.*) On *Thursdays*, May 30, June 6.

REGINALD BLOMFIELD, Esq., A.R.A. M.A. F.S.A. Three Lectures on THE ARCHITECTURE OF THE RENAISSANCE IN FRANCE, 1494–1661: 1. 1494–1547, THE AMATEURS; 2. 1547–1594, ARCHITECTURE AND THE COURT; 3. 1594–1661, ARCHITECTURE AND FRANCE. On *Saturdays*, April 20, 27, May 4.

H. PLUNKET GREENE, Esq. Three Lectures on INTERPRETATION IN SONG: 1. EQUIPMENT; 2. RULES; 3. SONGS AND THEIR CLASSIFICATION. (*With Vocal Illustrations.*) Accompanist, Mr. S. Liddle. On *Saturdays*, May 11, 18, 25.

WILLIS L. MOORE, Esq., Chief United States Weather Bureau, United States Department of Agriculture. Two Lectures on 1. THE DEVELOPMENT OF METEOROLOGICAL SCIENCE; 2. THE UTILITIES OF METEOROLOGICAL SCIENCE. On *Saturdays*, June 1, 8.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Report of the Agricultural Research Institute, Pusa, 1910–11. 8vo. 1912.
Memoirs of Department of Agriculture: Botanical Series, Vol. IV. No. 3. Chemical Series, Vol. II. No. 2. Entomological Series, Vol. II. No. 10 Vol. III. 8vo. 1911–12.
Catalogue of Books registered in the Punjab. Sept. 1911. fol.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXI. 1° Semestre, Fasc. 1–3. Classe di Scienze Morali, Vol. XX. Fasc. 7–10. 8vo. 1911–2.
American Academy of Arts and Sciences—Proceedings, Vol. XLVII. Nos. 10–12. 8vo. 1912.
American Geographical Society—Bulletin, Vol. XLIV. No. 1. 8vo. 1912.
American Philosophical Society—Proceedings, Vol. L. No. 202. 8vo. 1911.
Astronomical Society, Royal—Monthly Notices, Vol. LXXII. No. 3. 8vo. 1912.
Bankers, Institute of—Journal, Vol. XXXIII. Part 3. 8vo. 1912.
Basel, Naturforschenden Gesellschaft—Verhandlungen, Band XXII. 1911. 8vo.
Belgium, Royal Academy of Sciences—Annuaire, 1912. 8vo.
Boston Public Library, The Trustees—The Public Library of the City of Boston: A History, by H. G. Wadlin. 8vo. 1911.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIX. Nos. 7–8. 4to. 1912.
British Astronomical Association—Journal, Vol. XXII. No. 4. 8vo. 1912.
Memoirs, Vol. XVIII. 8vo. 1912.
Chemical Industry, Society of—Journal, Vol. XXXI. Nos. 3–4. 8vo. 1912.
Chemical Society—Proceedings, Vol. XXVIII. Nos. 396–397. 8vo. 1912.
Journal for Feb. 1912. 8vo.
Cracovie, Academy of Sciences—Bulletin, 1911: Matt.-Phys. Klass A, No. 10; B, Nos. 9–10. 8vo.
De Kantzow, Admiral H. B., M.R.I.—Cairo, Jerusalem and Damascus. By D. S. Margoliouth. 8vo. 1907.
From Damascus to Palmyra. By J. Kelman. 8vo. 1908.
Dent, Messrs. J. M. & Sons (the Publishers)—English Philosophers and Schools of Philosophy. By J. Seth. 8vo. 1912.
Editors—Aeronautical Journal for Jan. 1912. 8vo.
Agricultural Economist for Feb. 1912. 8vo.
American Journal of Science for Feb. 1912. 8vo.
Athenæum for Feb. 1912. 4to.
Author for March, 1912. 8vo.
Canada for Feb. 1912. 8vo.
Chemical News for Feb. 1912. 4to.
Chemist and Druggist for Feb. 1912. 8vo.
Concrete for Feb. 1912. 8vo.
Dyer and Calico Printer for Feb. 1912. 4to.
Electrical Engineering for Feb. 1912. 4to.
Electrical Industries for Feb. 1912. 4to.
Electrical Review for Feb. 1912. 4to.
Electrical Times for Feb. 1912. 4to.
Electricity for Feb. 1912. 8vo.
Engineer for Feb. 1912. fol.
Engineer-in-Charge for Feb. 1912. 8vo.
Engineering for Feb. 1912. fol.
Horological Journal for Feb. 1912. 8vo.
Illuminating Engineer for Feb. 1912. 8vo.
Journal of the British Dental Association for Feb. 1912. 8vo.
Journal of Physical Chemistry for Jan.-Feb. 1912. 8vo.
Law Journal for Feb. 1912. 4to.
London University Gazette for Feb. 1912. 4to.

Editors—cont.
Marconigraph for Feb.-March, 1912. 8vo.
Model Engineer for Feb. 1912. 8vo.
Motor Car Journal for Feb. 1912. 8vo.
Musical Times for Feb. 1912. 8vo.
Nature for Feb. 1912. 4to.
New Church Magazine for March, 1912. 8vo.
Nuovo Cimento for Jan. 1912. 8vo.
Page's Weekly for Feb. 1912. 8vo.
Physical Review for Jan. 1912. 8vo.
Science Abstracts for Jan. 1912. 8vo.
Terrestrial Magnetism for Dec. 1911. 8vo.
Zoophilist for Feb. 1912. 8vo.
Electrical Engineers, Institution of—Journal, Vol. XLVIII. No. 211. 8vo. 1912.
Franklin Institute—Journal, Vol. CLXXIII. No. 2. 8vo. 1912.
Geological Society—Abstracts of Proceedings, No. 919. 8vo. 1912.
Harman, E. A., Esq., M.Inst.C.E. (the Author)—Coal Storage. 8vo. 1912.
Institute of Chemistry—Official Chemical Appointments. Fourth Edition. 8vo. 1912.
Proceedings, 1912, Part 1. 8vo.
Kyōto Imperial University—Memoirs of the College of Science, Vol. III. Nos. 7–8. 8vo. 1912.
Life-Boat Institution, Royal National—The Life-Boat for Feb. 1912. 8vo.
Linnean Society—Journal: Botany, Vol. XL. No. 276. 8vo. 1912.
London County Council—Gazette for Feb. 1912. 4to.
Indication of Houses of Historical Interest, Part 35. 8vo. 1912.
Madrid, Real Academia de Ciencias—Revista, Tom. X. Nos. 1–3. 8vo. 1911.
Anuario, 1912. 16mo.
Meteorological Office—Report of Ninth Meeting of International Meteorological Committee (Berlin), 1910. 8vo. 1912.
Meteorological Society, Royal—Quarterly Journal, Vol. XXXVIII. No 161. Jan. 1912. 8vo.
Microscopical Society, Royal—Journal, 1912, Part 1. 8vo.
Mitchell, Messrs. C. & Co. (the Publishers)—Newspaper Press Directory, 1912. 8vo.
Monaco, Institut Océanographique—Bulletin, Nos. 220–223. 8vo. 1912.
Mond, Mrs. Ludwig, M.R.I.—Nachbildung Eigenhandigen Briefe des Bendictus de Spinoza. fol. 1903.
National Church League—Church Gazette for Feb. 1912. 8vo.
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New York, Society for Experimental Biology—Proceedings, Vol. IX. No. 2. 8vo. 1911.
New Zealand, The High Commissioner—Statistics of the Dominion, 1910. 4to. 1911.
Mines Reports, 1909. 4to. 1911.
Crown Lands Guides, 1911. 4to.
Onnes, Dr. H. K.—Communications from the Physical Laboratory of the University of Leiden, Nos. 124–125. 8vo. 1912.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for Jan. 1912. 4to.
Pharmaceutical Society of Great Britain—Journal for Feb. 1912. 8vo.
The Calendar, 1912. 8vo.
Photographic Society, Royal—Journal, Vol. LII. No. 2. 8vo. 1912.
Physical Society of London—Proceedings, Vol. XXIV. Part 2. 8vo 1912.
Radcliffe Library, Oxford—Catalogue of Books, 1911. 8vo. 1912.
Royal Colonial Institute—United Empire, Vol. III. No. 2. 8vo. 1912.
Royal Engineers' Institute—Journal, Vol. XV. No. 3. 8vo. 1912.
Royal Society of Arts—Journal for Feb. 1912. 8vo.

Royal Society of London—Proceedings, A, Vol. LXXXVI. No. 585; B, Vol. LXXXIV. No. 574. 8vo. 1912.

Philosophical Transactions, B, Vol. CCII. No. 288. 4to. 1912.

St. Bartholomew's Hospital—Reports, Vol. XLVII. 8vo. 1912.

St. Petersburg, Imperial Academy of Sciences—Bulletin, 1912, Nos. 2–3. 8vo.

Sanitary Institute, Royal—Journal, Vol. XXXIII. No. 2. 8vo. 1912.

Selborne Society—Selborne Magazine for March, 1912. 8vo.

Smith, B. Leigh, Esq., M.R.I.—The Scottish Geographical Magazine, Vol. XXVIII. No. 3. 8vo. 1912.

Societa degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. I. Disp. 1. 4to. 1912.

South Africa, Union of—Agricultural Journal for Jan. 1912. 8vo.

South African Association for the Advancement of Science—Journal, Vol. VIII. No. 7. 8vo. 1912.

Statistical Society—Journal, Vol. LXXV. Part 3. 8vo. 1912.

Tôhoku Imperial University, Sendai, Japan—Science Reports, Vol. I. No. 1. 8vo. 1912.

United Service Institution, Royal—Journal for Feb. 1912. 8vo.

United States Department of Commerce and Labor—Distribution of Magnetic Declination in U.S.A., Jan. 1, 1910. 4to. 1911.

Bulletin of the Bureau of Standards, Vol. VII. No. 4. 8vo. 1912.

Upsala, Observatoire Meteorologique—Bulletin, Vol. XLIII. 1911. 4to. 1912.

United States Patent Office—Gazette, Vol. CLXXV. Nos. 1–3. 8vo. 1912.

Warsaw Academy of Sciences—Comptes Rendus, 1911, Part 8. 8vo.

Wellcome Chemical Research Laboratories—Publications, Nos. 126–134. 8vo. 1911.

Zurich Naturforschenden Gesellschaft—Vierteljahrrsschrift, 1911, Heft 3. 8vo. 1912.

WEEKLY EVENING MEETING,

Friday, March 8, 1912.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

A. W. Ward, Esq., Litt.D. LL.D., Master of Peterhouse, Cambridge.

The Effects of the Thirty Years' War.

When we speak of the Thirty Years' War, and occupy ourselves with discussing the causes, the characteristics or (as I propose, in an avowedly partial and incomplete fashion, to do to-night) the effects of the long struggle called as a whole by that dread name—are we at bottom only using it as an historical expression? Let us feel quite sure of our ground at the outset; for the historical critics of our own day, whose comments have suggested to me that it might be worth while to ask you to go once more over some of the ground that must be already familiar to most of you, are within their right in demanding that no statement, and no form of words that implies a statement, should be taken at more than its actual value. Gustavus Adolphus, we know, described the Thirty Years' War as the whole of the wars of Europe grown into one; but this was a saying rather than a definition, and one which, from the very nature of the case, could hardly be applied to the earlier part of the struggle—the twelve years, let us say, which preceded his own intervention in the affairs of the Empire. The Bohemian, the Palatinate, and the Lower-Saxon or Danish War, were, all of them, still more or less localized; and to the last there were certain portions of the Empire—notably some of the Austrian crown lands in the south—which never suffered from the actual presence of war. Still, these exceptions amount to relatively little; and the localization of which I have spoken was not—unless it were by King James I.—expected to endure. Thus the continuity of place, though imperfect, was enough to allow us to speak of the Thirty Years' War as *one* war; and I think the same may be said of another continuity—the cohesion of the successive stages of a struggle which lasted for the whole of what we reckon as a human generation. At first sight, indeed, the defenestration at Prague—the incident from which it has always been customary to date the beginning of the Thirty Years' War—might seem far removed from the solemn gathering in the two Westphalian towns of European diplomacy prepared for its long-protracted labours—amidst pomp and circumstance scarcely inferior to those in which, a century and a half later, it assembled at Vienna to bury the French Revolution and all its works. There were few to cast a thought back, from the solemn formalities ushering in the balanced deliberations of the Westphalian Congress, to the arrogant pertinacity with which the proud Ultraquist nobles had defied what they deemed the decadent authority of the House of Austria, and the revived activity of the hated Roman *curia* with whose interests that House was once more identifying its own, and had given the signal of irreconcilable war by hurling the official servitors of the Imperial policy out of the windows of the Hradschin into the gaping foss below. Thirty years had well-nigh passed; Bohemian Protestantism seemed to have gone the way of Austrian; even of the German evangelical world only a fraction still stood in arms against the House of Habsburg; and the problem to be settled concerned not the briefed rights of a kingdom beneath its sway, but the "satisfaction" of the claims of two great foreign Powers, neither of which had visibly so much as cast a shadow over the situation of things in which the outbreak of the conflict had intervened.

Yet the continuity of the Great War, which, by prolonging it without a break, gave impetus to the rolling flood, and volume to the effects—political, social, moral—which it produced, remains incontestable. Indeed, as it would not take long to show, it exemplifies, with almost unsurpassed fullness and variety, what to me has always seemed one of the most appalling features of all great wars—and by no means only of those of the obsolete, long-drawn-out, Thirty Years' War sort, of which nowadays military historians can hardly write, or those who have seen what we have seen, read or think, with patience. For what could be more appalling than that the bloody arbitrament of war should be invoked, and that the whole Iliad of woes which must follow in its train should be brought down upon a nation, perhaps upon more than one nation, for a stated purpose, and that, this purpose having been either achieved or missed, the conflict, with all its attendant evils, should continue, as it were automatically, for some new end—be it some heightened ambition, or some increased gain, or the settlement, hitherto unapproachable, of some long-lived national or international problem, or, too often, because the deadly brood of passions once let loose, cannot, without inducements undreamt of at the outset, be called into leash again!

But, without drawing between this Thirty Years' War and later wars parallels which the case would not bear—and indeed I for one think the seductive game of so-called historical parallels very rarely worth playing—think for a moment, if only by way of recalling to yourselves the breadth and variety of the ground from which we are to choose some points or passages in illustration of the bearings of our theme—think for a moment of the awful chain, if I may so call it, which made up the totality of the struggle. The bold attempt of the Bohemian leaders to determine the destinies of their own and the dependent countries, and to secure a free enjoyment of their faith to those in sympathy with them in the Austrian home duchies, by means of a raid upon Vienna, having come to nothing, an offensive shrinks back into a defensive war, and the Bohemian crown becomes the prize for which militant Calvinism contends with aggressive Catholicism in the persons of the Elector of Palatine, Frederick V., the representative agent of the great plan against Habsburg, and the Emperor Ferdinand II., the pupil of the Jesuits. That question having been settled on the White Hill, and a peace patched up with the Transylvanian ally of the Bohemian revolt, the Winter King's own patrimony, a land of fruits and wine like no other in the Empire, but destined to suffer more terribly than almost any of them from the scourge of this war, lies, like the gladiator's expectant victim, panting in the arena. After a medley, in which even Spaniards and Englishmen have met, though on dry land, as natural foes, the chief captains of the soldiery on both sides, Mansfeld and his fellows, and Wallenstein, already asserting himself as the master of mercenary war, shift the main theatre of the struggle to the north-west. It is then that the inflated ambition of Christian IV. of Denmark, lacking alike in material strength and in intellectual grasp, impels him to essay the first foreign intervention of importance on behalf of the liberties of the Protestant estates endangered by the reaction with the aid of League and Emperor; and its failure, after the whole of Jutland has been flooded by Imperial troops, gives their victorious master his great opportunity. But the twofold scheme of restoring to the Church of Rome her former landed wealth by means of the Edict of Restitution, and permanently extending the power of the Emperor by sea as well as by land to the shores of Baltic and North Sea, breaks down, in part or altogether; and, after daring Heaven to withstand the advance of the Imperial arms, Wallenstein, the new Duke of Mecklenburg, has, in deference to the jealousy of the princes of the League, to lay down his military command.

Then there appears on the scene, at last, the great soldier-statesman, who entered into the war with the definite purpose—this also twofold—of safeguarding the security and power of his own Scandinavian kingdom, and of rescuing German Protestantism, of which it is futile to suppose self-interest only to have prompted him to become the champion, in the process. But note how, even in his case, a few months of brilliant though not unbroken success, induce this great strategist and far-sighted politician to change his design into something widely different. So, the head of the whole *corpus evangelicorum* now means to become master of the whole of Germany, under whatever form may prove most convenient. The purpose to which Gustavus Adolphus, after his victorious but carefully planned march from Oder to Maine, now addressed himself, he seemed likely to accomplish; but, he no more, it must at once become a phantasm. He falls at Lützen, and by that single catastrophe the whole conditions of the struggle are fundamentally changed. And with them the character of the conflict itself seems to change. The direct religious issue seems to grow dull and dim with the removal of the Protestant champion ; the personalities that carry on the struggle seem to become less vivid to the student of it, as they did to its contemporaries ; it has been pointed out how the songs of the Thirty Years' War, of which we have a plentiful and interesting crop for its earlier half, seem to become extinct after the death of Gustavus.

Meanwhile, Wallenstein has reassumed the command ; and, with an outspokenness unprecedented in the history of modern warfare, the system nakedly proclaims itself of making a great organized mercenary force, not the servitor of nations or other political authorities, but the arbiter between them. The great captain concentrates in his native Bohemia the vast armada, by which his master had hoped to restore both the fullness of the Imperial sway and the fixity of Catholic ecclesiastical control—but his own mind is intent upon peace. And, though his schemes are dark and interfused with personal ambition, there can be no doubt that the peace at which he aimed was to have been brought about by sacrifices less exacting than those to which the Empire and Germany had actually to submit in the Treaties of Westphalia. After his violent death, the conflict enters on a stage in which Sweden, after at first heading a compact but not Protestant alliance, falls back, when defeated at Nördlingen, upon the more limited task of securing what she can of the territorial acquisitions on which she had laid hands so soon as might be after the coming of her King. In her place, France assumes the real control of those princes who have not turned their thoughts once more to a reconciliation with the Emperor, and who finally conclude their separate peace with him at Prague. Half as an auxiliary, half as a paid officer of France, Bernard of Weimar ends his heroic career. But it is the divided purpose, the particular ambitions, of Sweden and France, whose alliance might have found a very different end had the great King lived a few years longer who had first concluded it with the far-sighted Cardinal—it is their self-centred ambitions seeking to control the policy and the relations to the Emperor of the German princes—even of the Catholic Bavarian—and it is, on the other hand, the close co-operation between the Emperor and Spain, and the consequent inclusion of the revived struggle between Spain and the United Provinces, in the war, which give to its latter half its characteristic aspect. That aspect is one of purposes and policies crossing one another, as the marches of the armies pass from Silesia to Westphalia, and far out to the Baltic shores and to the borderlands of Alsace and Lorraine. The very armies have changed their character, for the system of mercenary armies of the earlier type had given way to that of standing armies organized by princes for the territorial defence ; but the character and conduct of the soldiery had not improved in consequence, and the discipline had sunk from the days of Tilly and Gustavus, and, not less distinctly, of Wallenstein. But this, by the way; together with a hint that the Swedish atrocities, which are traditionally connected with the later years of the War, were not very Swedish—in the year 1639, the Swedish armies, which harried so many regions of the Empire, are said to have not contained more than 500 Swedes.

Thus there was added to the horrors and sufferings of war the unutterable weariness inseparable from a conflict to which there seems no end, because the objects for which it is carried on are unrecognizably disguised or hopelessly confused. When peace came at last, and when the reckoning into some of the details, of which we are about to enquire, had to be cast up, the one clear gain for the Empire at large, ascribable to the protracted prosecution of this ever-shifting War, was the establishment of the principle of parity between the three great Christian confessions into which it was divided. This gain, the announcement of which found a responsive echo even among the political and religious contentions of distracted England, could not be, and was not, again lost to Germany; but, for the present, a dark shadow was cast over it by the great exception on which the Emperor had successfully insisted. Protestant worship of either kind was excluded from all the dominions of the House of Austria; and this exception covered that very Bohemian kingdom where the violent resistance to the Catholic reaction had provoked the outbreak of the Great War. *Sic vos non vobis*—could a more signal instance be suggested of the paradoxical results of war?

Among the effects which this War left behind it, and at some of which I propose to invite you to glance in turn, the very first, as the most obvious of all, is the numerical effect upon the population of the Empire. The German economists and political philosophers of the generation which followed upon that of the Thirty Years' War were remarkably alive, as well they might be, to the primary importance for the welfare of a state of a numerous population living under conditions suitable for its due support; but the science of statistics was still unborn, and estimates of the advance or decline of population even in quiet times can often only be accepted with the aid of a good deal of faith. More recent criticism cannot therefore be blamed for treating sceptically such traditions as those which have been confidently handed on from generation to generation, and according to which the War and its immediate effects entailed upon Germany a loss of something like 12 millions out of a population of 18 or 20 millions. This calculation should not, in my opinion, be contemptuously rejected as altogether wide of the mark; but it must be allowed to rest on a basis not satisfactory enough to shut out the possibility of very great exaggeration. (The case stands otherwise, as you know, with the estimate which concludes the Black Death to have swept away more than half the population of England in its visitations about the middle of the fourteenth century.) The estimated loss of population in the course of the Thirty Years' War covers, as has been pointed out, together with an actual decrease, the failure of the ordinary increase of population, even if this be taken at a very low rate indeed. It covers, as a matter of course, the loss of female as well as of male population, or rather it makes no distinction between them —a rather curious omission, since, besides the number (actually calculated) of the men who fell arms in hand, one would have expected the number of men whom the War struck down to have been greater than that of women—unless indeed Disease and its parent Want acted with compensating intensity in the case of the weaker and probably worse-fed sex. This points at what in fact is one of the chief difficulties against forming an even approximately correct estimate of the loss of population. But, in conditions such as those of a destructive war which disturbs and dislocates all order and system, the statistics of disease are usually beyond reach, while those of deaths under arms or from the conquerors of battle are proverbially doubtful, and especially so in the case of armies lined and organized (sometimes in half-bogus companies or regiments) like the mercenary hosts of the earlier part of the Great War. Yet the attempt, as observed, was made even at the time to arrive at results which at least seemed accurate; and, while one daring—but, at the same time, temperate—statistician in 1631 reckoned the Emperor's military losses during the first nine years of the war at 51,011 men, he stated those of the chief leaders on the Protestant side (exclusive it would seem of the Bohemians in the first phase of the war) during the same period to have reached 57,686. This arouses no confidence, though, curiously enough, we have the author of a broadside, printed in the last year of the War, extending the number of those who were killed during its course as "at least" 325,000—a number not much out of proportion to the earlier calculation. Another difficulty, at which I have also already hinted, is that of the varying incidence of the inflictions due to the War upon the different parts of the Empire, and the consequent dangerousness of generalizing from particular data. The law of average could never have been more severely tried. On the other hand, a large number of statements as to actual loss of population from divers causes are at the same time so overpowering in their magnitude, and so reasonably well authenticated, proceeding as they do from actually official sources—administrative statistics, ecclesiastical registers, municipal records and the like—that it is futile either to seek to discredit them one and all, or to treat them as mere isolated phenomena from which no general conclusions are to be drawn. Such, for instance, are many of the detailed items supporting the total figures which are given as to the decrease in the population of the sorely-tried Palatinate—a decrease from something like half a million to less than a tithe of that number; or in that of electoral Saxony, where, in the critical years 1631 and 1632, when it was successively occupied by the Imperial and Liguistic forces, more than 900,000 lives are said to have succumbed to the sword or sickness (how we used to be taught to detest the craven policy of the Elector John George, who was all for peace!); or of Franconia, Gustavus's own chosen prize, where the population sank so low that monastic vows before the age of 60 had to be prohibited, the marriage of clerics was stopped, and laymen were allowed to take two wives each. I confess that I am not impressed by the suggestion that elsewhere there may have been a compensatory increase of population. Of course, we need not assume that in all the endless cases of the depopulation of villages, which in a very large proportion of them actually amounted to utter destruction, the population which their place of habitation knew no more was literally annihilated—and, in the case of the towns, we know that even in Magdeburg there was not an end of all things. Many peasants must have shifted their dwelling-place, and we know in what large numbers they took up their refuge in the towns—like the people of Attica within the walls of Athens—so that in Weimar, for instance, we read that towards the end of the war the number of immigrants was double that of the natives. Many men, women and children, too, must have been absorbed into that floating element of population, which, after being unhoused and unsettled by the tide of the War, followed the endless marches of the armies as an untold and untellable contingent of beggars and brigands. When peace returned, this vagabond part of the population was not extinguished, but waited to be revived in later days of warfare, as indeed it was very notably in the early wars of the French Revolution. The vagabonds who represented the homeless element in the population, including swarms of disbanded soldiers, coalesced with the gipsies—a class of nomads long regarded as standing outside the social community, but which, being thus reinforced, remained in Germany a more dangerous and in some respects a more important element in the population than elsewhere. My immediate point, however, is the decrease of the calculable in favour of the incalculable element in the population, by the wholesome uprooting of peasants whose expulsion from their homes was a direct effect of the War.

Altogether, after room has been found for inevitable exaggeration, and after all deductions have been allowed which ought reasonably to be made from the points of view indicated, we shall probably be willing to subscribe to the conclusion with which few economic historians of the present day and few statisticians seem prepared to quarrel, that during the Thirty Years' War the population of Germany had sunk to one-half its previous total, or perhaps between one-half and two-thirds. This is the conclusion of Schmoller, the results of whose investigations, especially for north-eastern and eastern Germany are authoritative; and I do not think that any hazard will be run in accepting it.

It was the peasantry, in which of course lay the real strength of the greater portion of the Empire, that beyond all doubt suffered most heavily from the effects of the War. Schiller, whose *Camp of*

Wallenstein offers many notable instances of the insight of the imagination into historical truth, knew this when in his picture of the War the peasant is the figure to whom the attention of the spectator is first directed. With the peasant there was no question of buying off the inroads of the soldiery by power or by payment; it is we who have to pay for everything 'with the skin off our own bones.' Among the agricultural districts of Germany, we have precise governmental information as to part of the rustic population of the Mark Bradenburg; a year or two after the close of the War, their settlements were in number less than half of what they had been at its commencement, and there was at least one county which had lost all its villages but four. This example is attested beyond cavil; but the story was much the same in the midlands—in Thuringia and in Hesse, and again in the south-west, though here the natural fertility of the soil made a speedy resuscitation of village life and activity at least possible where labour could be obtained, instead of, as farther north, leaving wide stretches of land to remain uncultivated for many a long year—the prey of weeds and water, till they turned into all but hopeless morass. How could the disheartened remnant, without capital of any sort, least of all the beasts of the field which the War had driven away, restore the face of the land to the semblance of what it had been? What inducement was there to produce more than this crop yielded by the impoverished soil, when such scant purchases as could be collected would pay only half the old price for either wheat or rye? Even in those districts favoured by nature like the Palatinate or Wurtemberg, or blessed with a capable ruler like the Elector Charles Lewis, whose heart was set upon recalling both landlords and peasants to the land whither he had himself returned with the Peace, labour was almost impossible to obtain, agricultural wages having risen to four or five times their former height; and the peasant had long to manage as he could without labourers—in other words, to limit his production to what was necessary for the bare subsistence of himself and his family. During this cruel war, the peasant, even if he remained in his cottage on the soil, instead of being hounded out of it or burnt down with it, had not merely to toil at the desperate task of making a livelihood out of his imperilled land. Besides the taxes and dues imposed upon him by native or foreign governments, he was subjected to personal services (*Fronen*) which so far from being extinguished by the War, frequently rose to an unprecedented height during its course, in some places to such a height as to convert the position of the free peasant into that of serfdom, while elsewhere they became so intolerable as to empty the land of its peasantry. Moreover, in times of unrestrained violence and licensed illegality, the instances were numerous of the actual expulsion of peasants from the lands which they held by the landlords, in order that these might possess themselves directly of the vacated holdings. This is the notorious practice of *Bauernlegen*—a rough and ready way of creating large estates which was to exercise a lasting influence in parts of Germany, notably in Mecklenburg. There was nothing to stop this practice but the consequent want of labour in the depopulated estates; but it contributed very distinctly to the decrease of a starving peasantry, while the landlords, who were left, if there was truth in the song, had to fight it out with the soil themselves:

> "Of the German War what is the gain?
> Many counts, barons and noblemen.
> German blood's very noble at present,
> Because of the weakness of the peasant." *

It needs no further illustration for us to understand how in the open country, even in the more fertile parts of Germany, the peasantry very slowly recovered its numbers, and how in others the country very long remained the desert, to which the war had reduced it. The assertion that particular cultures were destroyed by the War, on the other hand, seems to require more careful consideration here, but I may direct attention to the probability that the supersession of the culture of woad (due to the importation of indigo) was probably only completed by the War; whereas, the culture of the vine was undoubtedly largely reduced and confined to those nobler sorts, which can only gladden the hearts of a limited number of purchasers.

In the general bankruptcy which prevailed after the War, when the armies had to be disbanded, and the payments and compensations of various sorts settled—though signs of financial collapse were perceptible both during the War and even before its outbreak—it was therefore not astonishing that the peasant should be the worst sufferer. And this, to whatever section of his class he belonged: whether to that bound to the domains of the several princes, or to the estates of the nobility, or to the free peasants in their own holdings. The peasants' holdings were largely mortgaged like the lands of the nobles themselves, the mortgagees being for the most part the capitalists, large or small, in the towns; and now there was a general stoppage of payment and fear of foreclosing, because of the profitless condition of husbandry, on which I have already dwelt. Hence, a general state of hopeless indebtedness, in which the peasant, unable any longer to obtain the slightest advance of either money or materials, was the earliest and most certain to go under. Within half-a-dozen years from the Peace of Westphalia the problem had assumed such dimensions that it was brought, as a matter of imperial interest, before the Diet of Ratisbon. A sort of *tabulæ novæ* was proclaimed, a promise being held out to creditors who had made loans to agriculturists of various classes of repayment of their capital within ten years, and in return the debtors were relieved of three-quarters of the interest due from them since the troubles of the War began, while for the payment of the remaining quarter they were to be allowed a ten years' respite. But, like many another decree of the Diet, this decree, which does not seem to have stood on any firm footing, when it was not either anticipated or superseded by the action of particular governments, remained ineffectual.

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* The *Reichsritterschaft*—once a very respectable body so far as its possession went—dwindled away during the War; and the poverty in which it left a few reigning houses forms a pitiable chapter in the history of the petty principalities of Germany.

Although, then, it may be conceded that the intolerable burden of debt placed upon the land was not wholly due to the War, but began to weigh down the cultivators even before the outbreak of hostilities, yet it was enormously increased by the conflict, which thus crippled, and in many parts of the Empire paralysed, its most important and widespread industry, and with it the vitality of the greater part of its population.

Let us turn from the country to the towns. Here, again, it would be futile not to allow that some of the causes which contributed to the all but general downfall of the commercial prosperity, and hence of the political influence, of the German towns were in operation already before the outbreak of the War. For many a decade it was only by holding together at home against the encroachments of the territorial sovereigns that the fifty-one free towns of the Empire had preserved their autonomy behind their ancient walls; and their prosperity had been sapped at its base ever since the change in the great trade routes of the world had set in in the 15th century, and since in the 16th the nations who followed the Spaniards and Portuguese as the leaders of Oceanic intercourse—the French, the Dutch, and the English—had begun to distance German maritime trade. The day of the Hanseatic merchants seemed gradually drawing to a close even in what they had come to regard as German waters; the English merchant adventurers were busy in the ports of both North Sea and Baltic, and the surviving representatives of the Great League were subjected to Danish dues in a land which that League had once regarded as subject to its irresistible control. The Thirty Years' War, however, put an absolute end to the corporate activity of the Hanseatic League, and this, not only because of foreign competition, but, primarily, because the pressure of the War on the inland towns belonging to the League prevented them from helping to maintain its organization by their contributions.

When, at the height of the Imperial ascendancy in the period after the Danish War, the ambition of the House of Habsburg, urged on by the brooding genius of Wallenstein, sought to win over the decadent League to an alliance which would have placed its mercantile and naval resources at the disposal—or, why not say at the mercy—of the Imperial power, the offer was declined. In lieu of it, the Emperor was besought to allow the members of the League to continue a neutrality which had hitherto proved so advantageous—though, as they might have added, the advantage was theirs rather than the Empire's. This candid request indicates the attitude which the great maritime members of the Hanseatic League continued to observe during the War. As a matter of fact, two out of the three great maritime towns which after nominally renewing the decrepit League towards the end of the War, preserved down to our own day the ancient name which was all that was left of it—Hamburg and Bremen—suffered perhaps less than any other of the more important German towns during the course of the conflict; and one of them, Hamburg, which from the beginning of the 17th century onwards, had facilitated the advance through its portals of English trade into Germany, turned the actual state of things to its own account with remarkable skill. Lübeck, the venerable head of the Hansa, was necessarily less favoured by fortune; for the command of the Baltic was one of the main ends to compass which first Denmark and then Sweden entered into the War, and the ultimate ambition of the Scandinavian Powers contemplated nothing short of the extinction of German navigation in its waters. Lübeck, instead of any share in the rule of the blue sea over which she had once been mistress, had to guard her ancient gateways against horsemen and pikemen; and even before the conclusion of the peace called by her name at the end of the Danish War her citizens are found complaining of the diminution of her fleet, ship after ship, ill made up for by the unavoidable increase of her military trained bands. The credit of her great merchant houses was beginning to give way, and a decline was setting in to which there has hardly been a turn till the last quarter of the nineteenth century. On the other German Baltic ports, in so far as they were within the orbit of the War, nearly all seem to have followed her example, sharing the fortunes of the territories in which they were situate or in which the terms of the War incorporated them. Thus, Wismar and Rostock, the twin ports of the ill-starred Mecklenburg duchies, were stricken down by the rush of the conflict which made them for a time the spoils of the great Imperial captain; from the former comes the complaint that in six years not a single vessel had weighed anchor in her harbour, and that her citizens capable of bearing arms had dwindled from 3000 to one-tenth of that number, while out of this handful of men had in the same period been extorted contributions amounting to 200,000 dollars. Pomerania, we know, was in Swedish hands; but further east, and beyond the immediate range of the War, Danzig, long famed for the skill with which, like another Ephesus, she had managed to preserve her neutrality in the midst of armed conflicts around her, was little better off than the towns of the Baltic south-west. This was partly due to the consequences of the Polish Wars which paralysed her main trade—partly to awful ravages of the plague in the first decade of the war.

In central Germany, the statistics of taxation, which here we fortunately have at hand, speak very distinctly of a general decline of taxing-power, bearing out the general conclusions as to depopulation, which I have already discussed; but I am now specially concerned with the towns, among which our attention is arrested by the very exceptional case of Leipzig. As, so to speak, the centre of the centre, and the natural meeting place of the streams of commerce converging from east and west, north and south, Leipzig had suffered all but incessantly from the first approach and advent of the Swedish invaders onwards, and had witnessed a succession of sieges, occupations and great pitched battles on its vast plain, to which it would be difficult to find a parallel. However, after the bloody second battle of Breitenfield (1642), it remained occupied by a Swedish garrison for a total of eight years (extending even over the actual conclusion of peace); and this circumstance gave the sorely tried town a position of certain stability, the Swedish commander Torstensson, who was a shrewd personage and fully alive to his own interest, proclaiming the Leipzig Fairs to be under his protection, as an institution, without which neither Leipzig nor the world at large could exist. Thus the good town after all weathered the storm, and even while that storm endured had maintained its honourable reputation as a shelter for sick and poor, and a refuge for fugitives and exiles. Such a reputation may well be looked back upon with pride, and proves the beneficent part which even in the worst of times commerce may play on behalf of civilization. What a contrast the experience of Leipzig in the first War offers to that of the other great midland city, the bulwark of Protestantism, which had proved unequal to save itself, and which Gustavus had been unable to save because of the laws of strategy! It had crumbled into a heap of ashes, from which at first there seemed little hope of recovery. Magdeburg had before the War numbered something like 40,000 inhabitants—not very far off the population of Nürnberg, larger than that of Strassburg, more than twice that of Leipzig or Berlin. After the catastrophe to which the city's unsupported steadfastness had doomed her, the population, huddled together in a handful (100–150) of small houses or huts, had sunk into a miserable remnant, which not until a whole generation after the peace (1680) had again risen to 7000 or 8000 souls. The trade of the Elbe had irrecoverably gone to the great town at its mouth. Erfurt, formerly not only a famous University but the flourishing commercial capital of busy Thuringia, was entirely shorn of her prosperity; nor was the condition of the Brunswick and Westphalian towns much better, owing to a chronic pressure of calamity, which no administrative care could withstand, and to the drying-up of the industries, which in these regions had been most carefully and continuously fostered—thus, in particular, the mining industry in Brunswick, and in Westphalia the cloth industry, which had now passed altogether into the hands of English and Dutch.

On the Rhine, the ancient imperial cities of Cologne and Aachen had fallen from their former prosperity, and for a long time German trade could in these no longer hold up its head against that of the Dutch. The competition was not so ruinous higher up, on the Maine. Frankfort, which had suffered terribly from pestilence and famine in the earlier part of the War, afterwards, though not in the same measure as Leipzig, showed recuperative power. Very different was, in its results at the end of the War, the experience of the great Franconian towns, Nürnberg and Augsburg, across the history of whose many centuries of vigorous and honourable prosperity the War seemed to have drawn a bar which it would take long to remove. The number of the inhabitants of Augsburg had in the earlier half of the War sunk from 80,000 to 17,000 or less—and of the weavers and their families who before its outbreak numbered nearly what was now the sum total of the entire population, only a few hundred were left. Thus, in this case, almost an entire industry, which occupied or fed not far short of a quarter of the population, was virtually extinct. Within the ancient walls of Nürnberg, in face of which in the middle of the War two of its most formidable commanders and their armies had met, to separate again as after a drawn game, famine and disease had raged with similar results. The great Bavarian city of *Ratisbon* had ceased to be a trading town; and as one of our authorities chooses to put it, had to console itself with being the established seat of the Diet—a very wordy and windy consolation.

The decay of industry, as already observed, is even more striking than that of trade, though the prosperity of both was of course inseparably bound together. The former was not due to any falling off in the aptitude of the Germans as technical workmen, or to a more than ordinary unwillingness, fostered by the continued endurance of the guild-system, to make use of new inventions or improvements, especially in the direction of machinery; but it was intensified by the continuous competition of other countries not similarly hampered by a growing deficiency of labour. For the cloth looms of Westphalia and the potteries of Hesse could no more than the vineyards and orchards of the Palatinate be worked without labourers; in Bavaria too, where the cloth and linen manufactures had attained to considerable prosperity, a complete and lasting stagnation had set in throughout these widespread industries. In Saxony the recovery seems to have been quicker—hastened, no doubt, by qualities which have always distinguished her population—intelligence and frugality. But I must not carry on the dreary catalogue further, or point out how the War ruined for a long time the nascent commercial and industrial prosperity of Brandenburg, which after its close had to be nursed up again by sustained administrative efforts, which call for our admiration as fully as does the military prowess generally held to have laid the foundations of the greatness of the Prussian State—or, on the other hand, how those of the Habsburg lands which had not been spared the infliction of the presence of the War—Bohemia, Moravia and Silesia—had suffered from a depopulation, to the causes of which was here added the ruthlessness of religious intolerance. I therefore conclude this part of my remarks by reminding you once more of the hopelessness in which the War left the economical condition of the Empire at large—not always bringing new evils, but always, or almost always, intensifying those already in operation. There are, I for one have no scruple in allowing, two sides to the question whether the established disunion of Germany—which affected its social and economic interests as well as its political life—was not an enduring evil mitigated in some measure by the variety of interests and ideas engendered by a large number of territorial centres; but of the calamitous effects of this division, which the War confirmed upon the economic, the commercial and industrial condition of the country there can be no doubt; and one illustration of this truth may suffice. How the evils which beset the economic condition of the country were aggravated by the War, even when their origin lies beyond the date of its outbreak one instance may show, in lieu of many, because it touched the very nerve of those relations of which I speak.

It is quite true that the financial condition of many German principalities was extremely bad in the period immediately preceding that of the War, and that this fact should not be overlooked in judging of its general effects upon German finance; it is true, for instance, that the extravagance of many of the princes in the early years of the century had reached an unprecedented height, and that the Emperor Rudolf II. had set an evil example of an expenditure, equally reckless in peace and in war, which some of the petty princes did their best to imitate; while the towns kept pace with the Courts, and ran up municipal debts by a profuse expenditure upon building and, in accordance with the taste of the times, upon the ornamental arts which decorate an opulence that not unfrequently means decay. But the particular evil of which I am about to speak after all had its root in the political divisions of the Empire themselves. They caused an abuse from which other countries besides Germany—England and the Netherlands, for instance—were to suffer in the days of the great monetary crisis of 1621—to disturb the economic and social life of Germany with unequalled violence. For, as was pointed out by my friend Dr. Shaw in a valuable paper read by him before the Royal Historical Society some years ago, in Germany there was no central authority which could command general respect and obedience enough to warrant its intervening in the matter of the *coinage*. Thus, while in other countries the secret traffic in coins was perpetually putting in circulation those of smaller value—debased money, in a word—in place of the better, they could authoritatively fix from time to time monetary standards, and thus again produce order out of disorder. But in Germany this was impossible; and the only substitute was a fluid system (if system it can be called) of minting unions formed among the Circles of the Empire, or among princes and cities whose territories were mutually adjacent. What was the meaning of this disunion, and what were the opportunities of mischief thus supplied for the debasing and circulation of base coin to the *Kippers and Wippers*, as they were called—a fraternity which, as a strict matter of fact, included the highest minting authorities from the Emperor down to the, to whom the name was applied, very humbler practitioners of the singularly easy process—may be gathered from the list given by Dr. Shaw of the independent mints in existence in one or two of the Circles of the Empire, by way of example. In the Lower Rhenish Circle, if I have counted right, they amounted to 66—including, besides temporal princes and cities, 13 counts and 7 barons, and not less than 8 bishops and 7 abbots. The facilities for sharp practice and fraud were increased by every one of these mints, inasmuch as it was precisely by means of their multiplicity that the *Kippers and Wippers* made their profits, systematically buying up the better coins, circulating more of the same nominal value coined out of the reminted silver, and making their profit out of the balance. (It is only fortunate that while the evil was at its height in the earlier part of the War coining was still a comparatively expensive process, till the use of machinery was introduced into it about the middle of the century—or this profit would have been still more tempting.) The value attached to a coin in commercial dealings came to depend upon the individual who tendered or accepted it in payment; an almost universal mutual distrust ensued, and the very basis of all business dealings between man and man, as well as between authority and authority, and community and community, was cut away. To the uncertainty which war in all its forms inevitably produces, and which with a shifting war like this could not be otherwise than constant, the condition of the money market, which was rather a money scramble or a money chaos, and which helped to send what good money there was in the country out of it in a steady current, thus formed a signal contribution. The condition of things was one in which trade and industry could *not* pick up, and in which profits could be made only by those whose combined resources of capital were peculiar to themselves; but it would take me too far to go into the special subject of the fortunes of the *Jews* in the course of the Great War.

Enough has been said, though much more might have been added, as to the material disadvantages to the Empire which the War inflicted upon it or intensified. It is obvious that these could not but be accompanied by experiences in the intellectual and moral life of the nation not less grave and disheartening, and enduringly associated in its traditions with the memories of its long day of trouble.

Beyond a doubt, the decay of the *Universities*, which had until recently stood for so much in the progress of the intellectual life of the people, and upon whose recovery of their former position the revival of that life must largely depend, has to be reckoned among the most disastrous effects of the Great War. Here again, the shocks of the War fell on ground ill prepared for offering resistance. The great impetus had nearly spent itself which had been given to the foundation of universities in Germany by the great humanistic movement of the 15th and 16th centuries; and, though it was not altogether stopped in the 17th (the German University, that of Altdorf in Bavaria, where Wallenstein, we remember, was as a student subjected to detention in the *carcer*, was actually raised to that rank after the actual outbreak of the War), it could only continue if the spirit from which it rose were renewed. But the Lutheran Reformation, speaking broadly, had failed to develop in the atmosphere of true academic freedom the growth of the seed which it had sown; and while, in the latter part of the 16th and earlier years of the 17th century, the study of theology had overshadowed all others, it had arrested that liberal expansion of the pursuit of letters and science to which the age of Erasmus, and at one time at least Erasmus himself, had looked forward. In the days of *summæ theologiae* and of *formulae concordiæ*, drawn up so as to leave no escape from the network of the absolute, the German universities, whether Protestant or Catholic, revived *methods* of study which seemed to bring to life again the scholasticism which the Renascence had striven to cast out; and the weight laid upon the intellectual aspirations of studious youth—especially as it came to be drawn in increasing proportions from the classes which count the enjoyment of life as part of their inheritance—was greater than it could bear. What wonder, then, that German studentdom took its revenge in a licence which, as of its nature local and temporary, was readily tolerated by authority, and of which in most of the German universities, even after it had been repressed, the traces were not altogether extinguished. Professor Taubmann, of Wittenberg, who lived almost to the outbreak of the War, and who doubled his office with that of court fool to the Elector of Saxony, was on a visit to the palace at Dresden asked by the latter what the students at Wittenberg were doing. Whereupon the Professor at once descended into the courtyard of the palace, and digging up some of the cobbles with his sword, proceeded to throw them at the Elector's windows, shouting, "Down with you, you blackguard, you pennal!" The Elector was answered, so far as the great Lutheran university was concerned, but it was very far from being the worst offender. So widespread and palpable were the evils which were produced by this period of academic licence, and so effectually did it in many places during the War silence all but isolated endeavours at academic study, that the conclusion is justified that but for the War it could not so long have reared its head.

I do not know whether you are acquainted with the terms *pennal* and *pennalism*, which in German universities acquired an historic significance far beyond the intrinsic importance of the follies to which they referred. But the follies of youth may affect the welfare of the community as well as of the individual; and it was *pennalism* which brought German universities of the 17th century into a contempt and alienation from the intellectual and moral progress of the nation far deeper and more enduring than that which, from not dissimilar causes, seemed to threaten the English universities of a rather earlier age. *Pennalism* (the derivation of the word is obscure) was the treatment administered to students during their first year by their seniors. After, amidst tremendous mock solemnities, the *beanus* (*bec-jaune*, or freshman proper) had passed through the purifying ceremony of the *depositio* (common), and transformed by the application of scissors, file, saw and axe, he was depleted in purse by the feast by which he had to pay for his admission into the academical world. His first or *pennal* year then ensued—a year of mingled debauchery and tribulation, under the organized direction of his seniors, and lucky he if he could pass on to the later stages of his university career without a load of debt, a ruined constitution, and perhaps a half-broken heart. There are phases of university life which have all the sordidness without any of the poetry of Villon, and from which even the historic observer may turn with disgust. But if you remember that this was the time when some of the loftiest minds were engaged in speculating on the purposes and methods of education, proposing to themselves at times ideals so high that we are prone to set them down as Utopias—if you remember that this was the age (to mention two names only) of Milton and of Comenius (whose final work was published in the last year of the War)—then you will not wonder that in some of their contemporaries in Germany doubts arose whether the universities could really be looked upon as the true seats of high intellectual culture, and whether the training of the country's youth might not more profitably be carried on elsewhere. Such was the question asked by John Balthasar Schupp, a pamphleteer who should not be overlooked by students of the social history of the War.

Of the German universities, that which had taken the lead in cherishing the new birth of humanistic learning was Erfurt, whose geographical position, as well as its relation to the archiepiscopal See of Mainz, had forced it into the front of the religious conflict, and thus brought about a decay of its academical prosperity which seemed to be completed by the Thirty Years' War. The reason why I mention it in the present connection is, that no less a personage than Gustavus Adolphus made an attempt to restore to Erfurt, which occupied a central position in Germany, not far from that portion of it where he had actually established the seat of his own power as a German prince, its former academical greatness, and promoted a number of reforms which shortly after his death (1634) were formulated in a new code. But the War made it impossible to carry out these attempted changes, nor was it till a generation later—in

1664—that a reorganization, under which Erfurt's prosperity revived, could be carried into effect.

May I detain you for a few moments while I trace the effect of the War in the history of two other German universities—one southern and one northern—which may even more signally illustrate its direct incidence?

The ancient Ruperta Carolina of Heidelberg had, after a long period of fidelity to the Papal traditions of her foundation, in which she rivalled even her rather younger sister Cologne, continued to flourish both under the Lutheran Elector Otto Henry, who left her his library, and whose name still survives with the remains of his castle, and under the Calvinist Frederick the Pius, the magnanimous prince, under whom, owing to the confluence of Calvinist fugitives (Peter Ramus was one) and those of the flower of the Calvinistic youth of Europe, Heidelberg became the first German university to which we may ascribe an international character. After a second interval of Lutheran ascendancy, Heidelberg, under Frederick IV. and Frederick V., the husband of our younger English Elizabeth, once more became the centre of the Calvinistic anti-Habsburg propaganda, until the Great War broke out, and the honest young Elector, the heir of great designs, to the execution of which nature had not made his equal, was drawn to the Bohemian throne, and thence cast forth into a life of homelessness. Thus it came to pass that, in 1622, Heidelberg fell into the avenging clutch of the Catholic League and its general Tilly, and the University, with all its great memories and its greater promise, seemed already to have become a thing of the past. In the four years which ensued upon the capture and partial destruction of Heidelberg castle and town, six students matriculated; and in 1629 it seemed time for the gradual processes of the counter-Reformation to begin. A Catholic university crept into being, with two faculties, a theological and philosophical, each consisting of a single teacher. Within yet a few years, the Swedes were masters of Heidelberg, and once more, though the great Protestant champion lay dead, the university was brought back to its allegiance to the Reformation. But her vicissitudes were not yet over; and a year had barely passed before the great battle of Nördlingen was fought, and the darkest days of the Palatinate set in, when the capital, after being plundered by the worsted Swedes, was subjected afresh to a siege, and after this had been raised through the French, was reconquered by the Imperialists, and subjected during the remainder of war to the Bavarian rule, or rather to an alternation of anarchy and military occupation, during both of which violence and rapine, famine and disease, ran their horrid course.

These were the scenes, I may observe, in which, naturally enough, the imagination of posterity came, perhaps more than in any others, to picture to itself the awful desolation of the Thirty Years' War. I do not wish to enter very closely on this occasion into the comments of modern critics upon the stories of cannibalism—the last horror of barbarous warfare, which Khevenhiller's narrative has located in this neighbourhood; there are other stories of the same sort which came from other parts of Germany—possibly true, possibly false—but on the authenticity of each of which after all very little turns. The sufferings of the Palatinate in population and in prosperity are happily most trustworthily gathered from the detailed accounts of the recovery of the devastated land under Frederick V.'s son Charles Lewis, to whose errors history may be very kind in remembrance of his conception of the duties of a prince. To the university, after he had recalled it into existence some four years after the conclusion of the War, he restored a vigorous life, renewed, let it be said to his further honour, in the spirit of religious tolerance to which this descendant of the Calvinist Electors was one of the earliest witnesses among the princes of Europe. One possession, as you know, he could not restore to the University of Heidelberg—the famous *Palatina*—the library of which had been seized by the Bavarians in 1623, when the ban of the Empire was pronounced upon the Electoral House, and which mules had borne across the Alps to be incorporated in the *Vaticana* at Rome. I cannot tell the curious story of the partial return of the books under Pope Pius VII., in 1815 and 1816, which to them too was the epoch of Restoration. But the fate of the *Palatina* forms a curious episode in the history of the university of which it was long the most cherished treasure.

From Heidelberg and its story of successive rapid changes, we turn for a moment to a university whose name is less familiar on modern lips, but whose memory, like that of one or two other universities, I love to recall in season. The Julius University of Helmstädt had in its day been well endowed by its founder, Duke Julius of Brunswick, and his high-minded son Duke Henry Julius, who, like many princes of their age, regarded their universities as the most precious of their possessions, and (if one may so say) as the best of their investments. At the turn of the 16th and 17th centuries, Helmstädt was, next to Wittenberg and Leipzig, the best "frequented" of the Universities of Northern Germany, and was more especially sought out for their sons (like Göttingen in after days) by the great men of the land. In the years immediately preceding the Great War, the number of its students reached its greatest height, and *George Calixtus* was already a member of its theological faculty. If ever any one teacher has deserved to be called the glory of his university, such was Calixtus, who through great times of evil, till he remained all but the single academical teacher within its deserted halls, held out by her. Then, indeed, he was *professor controversiarum* without anyone to say him nay! For my part, there is no figure in the history of learning that I regard with deeper reverence than that of Calixtus, the personification of the principles of peace and tolerance in the midst of a generation bent upon persecution, fire and sword. Helmstädt was under the rule of a territorial prince, Duke Fredrick Ulric (brother of the redoubtable bishop Christian of Helmstädt), who was *dux pacificus* in his way—that is in the way of seeking in turn to be the friend of all sides; in consequence his land became one of the cockpits of the war, where indigenous and foreign ambition—Danes, Leaguers, Swedes, Imperialists—successively took up their quarters. The university, of course, began gradually to feel the pressure of the War; but so firm a root did it seem to have taken in the soil, that it was not till 1625, when the conflict between Christian IV. of Denmark and Tilly was approaching its crisis that the university was (as we know from one of Calixtus's letters) literally starved out of the town which had long and often very unwillingly harboured it. In 1626, the last of the students had quitted Helmstädt, and Calixtus and a worthy professor of physics, named Gran, alone remained to represent all the arts and sciences. In a while, the *régime* of League and Emperor seeming to have firmly established themselves, some of the teachers found their way back from Brunswick, and under the ægis of Wallenstein this Northern Protestant University began a new period of existence. Fixity of condition seems to have been its chief condition of existence; for when the Swedes came in 1630, and after the death of Frederick Ulric approved a *condominium* of his heirs, the number of students increased and went steadily up till the end of the War, when it reached the respectable total of 462. With the later history of the university (ultimately united with Erlangen) we cannot occupy ourselves.

Now, here we have a university which even during the course of the war, contrived after a period of calamity to accommodate itself to that course, and under more favourable circumstances, kept its head (as one may say) well above water. We are accordingly able in this instance to note the main characteristic of university life as affected by the war, and find them—exceptional influences like that of Calixtus apart, and of these we hear very little—those of intellectual stagnation varied by unbridled academical licence. Ill-paid professors—I spare you the harrowing details as to more pacific ways in which they supplemented their insufficient income—carried on the work of the faculties as best they could; and some members of the teaching-staff were fain from time to time to exchange the gown for the sword. Thus one Eberhard Bering, after lecturing on Oriental languages, at different points of his career enlisted as a trooper under the notorious General Holk and under another Swedish commander (he died peacefully after being in turn professor and schoolmaster). The students at Helmstädt took very largely after this type of professor, and of the students many of them treated the War, in the contending forces of which they took occasional service, as an agreeable vacation change, with the variety of pay, in the midst of occupations nominally more peaceable.

Nominally, I say, for nowhere shall we find the swashbuckler ways and brutal tone of the students of this period more glaringly exhibited than at Helmstädt, where the students were in constant warfare with their authorities, and the university as a whole with the town. The students brought back with them from their summer campaigns, not only the extravagances of military ways and manners (incredible extravagances of costume among the rest), but fire and sword in the most literal sense, to play a part in their winter amusements, and in spreading dismay among the Philistines. I use the word advisedly, because Helmstädt and Jena, which vied with one another in the excesses of their students, alike claimed the honour of having invented the famous designation for their natural enemies, those outside the pale of academic freedom. The academical seal of Helmstädt bore the design of Samson and the lion, which certainly seems suggestive of some traditional foundation for the claim; on the other hand, at Jena a legend ran of a Lutheran pastor having lamented over the corpse of a student done to death by offended burghers: "The Philistines have been upon thee." Be this as it may, we cannot marvel that the time came at last when the patience of outraged authorities was at an end; and two years after the Peace a visitation of the University of Helmstädt took place, which sought to bring order into confusion. As, however, a mutual understanding obtained among the students of practically all the German universities, the evil was of such magnitude that the Imperial authority thought fit to intervene, and in 1662 an Imperial ordinance was issued to put an end to *pennalism*, and perhaps this arrested some of the most intolerable excesses in vogue. I could not refrain from lifting just a corner of the curtain of oblivion which covers the distressful and unpleasing features of German university life in the period of the Thirty Years' War—features which were not to be changed in a moment. Happily, here at last a nobler side was not wanting. I have already spoken of Calixtus, whom not only the wide learning and the spirit of concilation, which he had inherited from his master in divinity, Melanchthon, lifted above party, and who pointed the way to that union which the Christian Church has not yet seen accomplished on the basis of the ethic of the Gospel. But the seed that he sowed through the long dreary years of his academical life was not sown in vain, and no university was more ready to receive and apply the lesson which he taught than the other ancient seat of learning, of which we have spoken—the Palatine University of Heidelberg.

Although the impulse towards intellectual endeavour could never be altogether deadened in Germany, although the universities and with them the grammar schools or gymnasia, recovered in course of time from their forced inactivity, or from the perverted conditions into which their life degenerated, and although, in accordance with the tendencies of the times noticeable elsewhere (in England, e.g.) learned societies, founded for the more intimate prosecution of classical and historical, and more especially scientific studies, helped to stir the current, it remained languid for a long time. The day seemed to have passed, although it was to dawn again, when Germany, instead of barely keeping pace with the literary and scientific advance of other nations, held a position of intellectual leadership, as she did in the days when the currents of Renascence and Reformation joined together. Under the direct influence of the War, which on the one hand accustomed the Germans to a long and benumbing domination of foreign influence, and on the other cut them off from ampler, easier and kindly conditions of life in which princes and cities vied with one another in the endowment of art, learning and research, there began an age of intellectual dependence. Not only do we notice that many Germans—many of them under the pressure of necessity—acquired the scholarship or other learning by which they afterwards gained distinction in the great foreign centres of intellectual activity; but the same was the case in art, where in sculpture, painting and architecture Germany sought its models in France and Italy, and in music, the native art *par excellence*, in which original productivity seemed to have dried up. Most of all was it the case in literature, in which the Germans passed through a century and a half of bondage, first to classical examples, then to Italian, French and Dutch and (in the eighteenth century) to English fashions. And yet, while this state of things continued, and in its intellectual life the country seemed more and more to depend upon what it could borrow and assimilate from others, it was sinking more and more into the narrow ways and ideas of an inland territory, shut up in its own inherited conditions which the War had fastened down upon it, while France, Holland and England were expanding their relations into an ever-widening sphere, and stimulating and heightening in the process the observing and reproducing powers of their sons. It is this effect of the War which is perhaps the most familiar to us of all, and it is this long period of dependence and pettiness in German national life which German historians have justly regarded as a debt, of which a large proportion was due to the memory of that long tale of warfare and of woe.

But while I repeat these truisms, I should add that the association of the results in question with the great war, as distinct from the general course of the national history, may again be carried too far. The condition of dependence upon foreign nations into which Germany fell for a long period of her history, and which she was not to cast off till after experiences such as few other great nations have undergone—this dependence was largely due to the war—to the renewal which it witnessed of an intimate alliance between the Austrian and Spanish branches of the house of Habsburg, to the long protracted intervention of Sweden and the decisive one of France, and to the relations in which those powers were left towards the Empire by the peace that concluded the war. Sweden was now a State in the

Empire; that France was not such was her own choice, and she preferred to trust to the principle now first established that the conclusion of alliances with foreign powers was open to any Germanic State at its own discretion. But though this full measure of dependence was, as I say, due to the War, the splitting-up of the nation into an infinitude of sovereign units, which it was impossible to bring into common action by the excercise of an acknowledged Imperial authority, and which could only be gathered into groups or camps under the name of Leagues or Unions by the special operation of interest, passion or intrigue (though the interest might be that of self-preservation and the passion that of religious dominance)—this was a *damnosa hæriditas* which mounted further back than the War. And the fashion of submitting with predisposed readiness to foreign influence as *eo ipso* irresistible—in a fashion which from political spread into almost every other aspect or branch of the national life, whether concerned with things intellectual or things material—this fashion was an old and inrooted evil which the windiness and want of patriotic self-confidence, fostered by the Great War, could only augment and exaggerate, but which it did not originate or invent.

I had intended to illustrate this observation by tracing, at the hands of Gebauer and other recent authorities, the history of a particular foreign influence which beyond any other has naturally occupied the minds and engaged the researches of modern German historians; but my time is growing short, and I can only ask you to examine for yourselves the earlier, as well as the later, chapters of the story of the French refugees in Germany, of which it is difficult to exaggerate the importance, and which begins with the period of the French Religious Wars and part of the not wholly tolerant reign of Henry IV. himself. Religious and university life were alike animated by the constant contact; trade both export and import flourished, France supplying the means of gratifying the growing love of luxury of all sorts, Germany, through its great banks of the South-west, supplying the French monarchs with ready cash in return for many welcome concessions. A particular trade which specially concerns us, the bookselling trade, with Lyons in this era as a centre perhaps even more important than Paris, was extremely active in advancing French influences of all kinds across the border. Thus, the ground had already been diligently prepared, and the intimate relations between German Calvinism and French Protestantism, French Liberalism and French public policy were steadily encouraging the process, when the Thirty Years' War supervened, and by its ultimate issues secured to France a political ascendancy which reacted upon every other kind of influence.

The influence of French manners, to which I intended to have more especially directed your attention, is traceable with curious completeness, though of course in varying degrees of intensity, during the periods immediately preceding and forming part of that of the Thirty Years' War. After the Religious Wars of France were at last over, the Court of Henry IV. is the seat of gallantry, where the pursuit of pleasure is viewed as the principal object of existence, but where, as is common in periods of reaction after troubled and turbulent times, a love of refinement and desire for restraint keeps at a distance the coarser forms of self-indulgence After the assassination of the good King Henry, a new phase of social form sets in and continues under the Spanish influence of the consort of Louis XIII. A strict etiquette and a deferential acceptance of religious formalism took the place of easier and pleasanter ways, without a corresponding advance in the morality which lies deeper—the earlier age of Louis XIV. was preparing itself, till in its turn it was to be succeeded by another of still more marked formality and decorum which lies beyond our present purview.

The Palatine Court, as was natural from its local situation, was most directly open to French influence; the University numbered several French scholars among its teachers, and the electoral palace itself, Heidelberg Castle, was renovated by architects in the French style. It was here that the young Electress Elizabeth, the grand-daughter of Mary Stewart, was received not long before the War with French allegorical ceremonial, and with French words of welcome. The Court of Hesse-Cassel, second only to the Palatine in political importance as upholding the "system" of the French alliance, was hardly less deeply imbued with French tastes and ways of life. Landgrave Maurice, one of the most far-sighted politicians of his time, was a resolute reformer of manners on the French model, and (in accordance with the change from grosser times) was the founder of a temperance society. In other Courts of the South-west—even in Bavaria—French influence asserted itself in spite of politics; in the North less so, with the exception of Anhalt (the home of Prince Christian, the life and soul of the Calvinist French alliance) and at a rather later date the Brunswick Courts, which bequeathed at least the preference for French speech to our first two Georges. Of all the German Courts that least open to French inroads upon old-accustomed forms and half-mediæval ways of life was the strictly Lutheran court of Dresden, which less than a century later was to outrun all others in its exaggeration of the gallantries of France.

There may have been something left-handed in such a compliment as that which Henry IV. paid to Count Dohna, a member of a family of celebrated diplomatists, and himself a German nobleman of a new school, when he presented him to the Queen with the question, "*Le voilà—le prendriez-vous pour un Allemand?*" But the anecdote supports the view that the Germans—a nation which in the middle ages had presented themselves in their poetic literature as types of a refined courtly way of thought and conduct—were not wholly losers by the operation of influences without which it would not have been easy to reform from within. *Grobianus* was worth extirpating *quo-*

cumque modo; while there was something in the whole history of German civilization which safeguarded it from being permanently corrupted by the new fashion of treating the relations between the sexes. Cyriacus Spangenberg, the author of an *Adelspiegel* (mirror of nobility) published at the beginning of the 17th century, speaks of the place of woman in society in a tone neither that of Grobianism nor of *galanterie*, and this tone, in spite of the coarsening and in spite of the relaxing influences of the War, German life and literature were to recover even before the ascendancy of French influence had passed away.

It would have been amusing had time allowed to observe how French ways and fashions of life progressed in Germany in the period of the Thirty Years' War, under the influence of the intimate relations between the Calvinist princes and France which marked its opening period, and the later political intimacy which culminated in the military compact negotiated by Richelieu with Bernard of Weimar. French example affected the outward forms of social life—the whole code of compliment, of which it seems to have been reserved for the 20th century to cast out the last lingering remnants—the amusements of the upper and middle classes, games and other diversions—matters of food and drink and the whole science of the table—the vast question of dress, in which the French *mode* is to be carefully distinguished from Spanish and partially stands in direct contrast to it. Upon dress the War exercised a controlling influence peculiar to itself, even the dandies of the day thinking it proper to appear in wide flapping hats and top boots, and even the women adopting the quasi-military fashion of head and neck gear, short cut hair and scarf round the waist. Yet these were mere passing whims, the extravagant expenditure on dress was permanent and remained unaffected by the soberer Spanish fashions. As in dress, so in speech, the War introduced, together with some Spanish and Italian words, a considerable element of French into the German vocabulary; the phraseology of the *campaigns* (there is one French word) became that of daily conversation, or *allied* itself (there is another) with it. The *contemptus tonguae Teutonicae* of which Martin Opitz in his book of that name complained just before the War (1611) could not but continue to prevail during its course. I must not, however, pursue the subject further, attractive as it is. Lest, however, it should be assumed that to this period was due the first use of French as the language of diplomacy by the side of Latin, I should perhaps point out that for this the Emperor Charles V., who was accustomed to the regular use of French from his boyhood, is to be held responsible. The attention given to the teaching of French and modern languages could not but be increased by the contact which the War after its fashion promoted; notice, for instance, the advice urged on this head by Clarendon, who had seen so many men and cities in his enforced travels, and notice how often war, for one reason or another, leads to the development, as in self-defence, of this branch of educational activity.

When, at last, the intervention of France had, by the unequal skill of her policy and the success of her great generals, been carried to such a point, that even after she had concluded peace with the Protestant princes of the centre and the north-west, the power of the Emperor could not prevail against it, her weight in the scale could not but determine the conditions of the general pacification, which it gradually became impossible to defer any longer so far as the Empire at large was concerned. France, as we have already noticed, declined to enter the Empire as one of its component states; but its material and moral forces had now been so effectually weakened, and the strength of national sentiment to such an extent impaired—though it is an error to suppose it to have been utterly extinguished—that though peace was signed between France and the Empire, the gates were really flung open to the continuous encroachments of the former Power. The age of Louis XV. was at hand, and all that it meant for Germany—the breaking-up of her Imperial defences in the west, in order that the rival ambitions of her princes might be gratified, the consummation of the rape of Elsass by the long series of reunions and by the surprise of Strassburg, the devastation of the Palatinate in the Orleans War, the demonstration of the national helplessness and the bitter disappointment (as in the case of the barren victory of Brandenburg over the Swedes) of the hopes even of those who greatly daring helped themselves, the closing of the future, as it seemed, which accompanied the present undoing. By the end of the century France was the arch foe, as which she presented herself to the consciousness of the German people for generations—but it was the Great War which made this result possible, and which had shaped with unmistakable definiteness the stages of its achievement.

The political effects proper of the War, though a recital of them is among the commonplaces of modern history, are not on that account the less deserving of careful investigation; but it was not part of my intention to-night to take you once more over this well-known ground. The War had destroyed most of what before its outbreak had remained to recall the earlier and more vigorous life, or substituted for it new and alien formations. So with its military organization, which had been rent in twain. So with its constitutional life, for the Ratisbon Diet, abandoning all thoughts of reconstruction, merely kept giving the machinery without which the Empire would have lacked even the semblance of unity. The Emperor was driven back upon the family policy, which—unless it were in the warding off of the Eastern peril—could no longer even appeal for a common endeavour for national ends. The princes came to him for his alliance—though a vague prestige and the right of conferring favours of rank of place still attached to it—much as they came to any other power; or even resisted him in combination with other powers, whose alliance they had formally been justified in seeking. Historians have found again in the decentralization, which already before the close of the War, writers—such as Hippolithus a Lapide—had proclaimed a legitimate political development, and the great publicists of the next generation, notably Pufendorf, saw in the formation of states with strong absolute governments the true creative process of the new era. The new era came, but it came slowly, and bore the promise of new conflicts in its bosom. Strong territorial or state organization, the reconstruction of the service of each state in its army, in the body of its civil officials, in the functions and responsibilities of its sovereign himself—was the work awaiting the new generation, on whose behalf we may say that the Great War had swept the path clear.

One word more before I hurry to a close. Though the age was not a creative one, in any direction of intellectual effort which in Germany succeeded that of the Thirty Years' War—how could it be such after the blight which had fallen on the land?—its literary and scientific impulses had not been wholly destroyed. Learned poets and simple—Martin Opitz the reformer of the poetic art, Andreas Gryphius the imitator of Shakespeare, Simon Dach who said that he did not like writing poetry in German, but who produced in it two of the most delightful lyrics (each in its way) in the language, besides the religious poet of whom I will speak in closing—were all at work during the course of the War itself. The *Fruchtbringende Gesellschaft*, founded in 1617 for the preservation, among other things, of the purity of German speech, held up its head during the whole course of the War, and continuously grew in numbers, though confining itself to the noble and learned classes. The accumulation of knowledge progressed steadily, and the desire to methodize and systematize it, as we should say, scientifically asserted itself more and more; the study of law made a fresh advance under such teachers as Conring, another Helenstadt name representative of the many-sided, and in his case almost encyclopaedic learning of the age—German's worst age, as one of his biographers calls it; history which had been silenced in part or altogether in some of the Universities during the War reasserted her claim to instruct the present by the experiences of the past—and before another generation had come and gone, the polyhistory of its predecessor had been exchanged for that aspiration towards an advance in all the fields of human knowledge which finds its incarnation in Leibniz.

If this was much to have been saved, there is yet something more to add. The religious life of Germany could not but suffer from the War more than any other side of the national existence. The gain secured by the Peace which concluded it to the cause of religious liberty was, as has been seen, marred by uncertainties and exceptions; while as to the establishment of the several forms of faith it was the *fiat* of the territorial prince, not the choice of his subjects, which, with certain limitations as to foundations that were to be left untouched as from a certain date, was pronounced to be the determining authority. But the religious life which underlay, or lay beyond, divergent confessions of faith or articles of belief could not be established or disestablished, settled or unmoored, by treaties and pacifications, and this life the War had constantly spoiled, broken and outraged. Except, in a measure, in the Bohemian or earliest stage of the War, and perhaps in the first siege of Madgeburg, and on some similar occasions in Silesia, the Palatinate and elsewhere, the War had even in its earliest stages not presented itself to the populations in the light of a religious war so much as in that of an endless series of invasions, occupations, devastations, bringing with it all the blankness as well as all the horrors belonging to a struggle carried on for ends to which those who suffered from it were more or less indifferent, or, worst of all, in that of a war carried on for its own sake. Thus religion, with all the emotions that nourish the religious sense, had been largely thrust out of the issues of the conflict, and by some of its agents frankly ignored. Meanwhile, those principles which are the foundations of the social edifice—respect for purity, sobriety, chastity, for the very sanctity of life itself—had become the playthings of homeless hordes of soldiery; and men (and with them doubtless women and children) had unlearnt the laws which should govern the relations and the conduct of human beings towards God and their neighbours. In their place stalked abroad all the vices of a society unhinged from its ordinary conditions—rank dishonesty in all the dealings of the day, and a shameless disregard of other demands than those of a self-indulgence eager to pluck the fruit of the day before the hour of doom.

Yet the voice of religion made heard its whisper—here and there its clear admonition—after the storms, and in the intervals allowed by darkness and riot; and out of the spiritual anarchy were born not mere passing murmurings or protests, but what were to prove the beginnings of new and far-reaching spiritual movements in the land. It should be remembered that in yet some other ways, not hitherto adverted to in this brief paper, the age of the Thirty Years' War is that in which the spiritual and intellectual life of Germany reached its lowest depths. The superstitious notions and misbelief which the Protestant Reformation had not only been unsuccessful in overcoming, but which it had allowed to continue their advance—at times almost under its protection—the belief in magic and witchcraft, and in the grotesque extravagances of astrology and alchemy—had corrupted all classes, from the highest to the lowest; had invaded the province of science—the strength of the demons, says one theological thinker, is as undeniable as the force of the winds, and Kepler taught that each of the heavenly bodies possessed a soul of its own; had permeated the art and the practice of medicine; and, in the case of the peasantry, had congealed themselves into a mass of tenets which might almost be said to constitute a distinct religion. When tremendous catastrophes, which hardly needed to be heralded by signs from heaven, the burning of towns and the destruction of villages, the slaughter of many thousands on the field of battle, the massacre of innumerable women and children, the uprooting and scattering of whole bodies of population, were witnessed as it were from the house-tops in most parts of the land, and in many districts of it seemed almost to form the regular order of things—the still, small voice of which I have spoken, could of course only be heard with difficulty and at intervals. Yet that it did make itself heard we know from several instances. A notable one is that of the Jesuit poet, Count Frederick Spee, a personality of the Great War which still lacks, so far as I know, its due biographical monument. Trained at Cologne on the lines of orthodoxy, he was serving as priest and preacher in the Minster at Paderborn, when the Bishop of Würzburg requested the Order to which Spee belonged to provide him with a confessor for a number of witches who had been condemned to be burnt to death in his episcopal capital. That such a functionary was needed may appear from the trustworthy statement that, in the year of this summons and the following year, 158 persons were publicly burnt to death at Würzburg on the charge of witchcraft—including three canons of the cathedral, seventy town councillors, a doctor of theology, several young noblemen, a blind girl, and two quite young children.* Spee did his duty until the authorities decided that no Jesuits should any longer be allowed to hear such confessions, which interfered with the due administration of justice. His hair had grown white over his work, and when his missionary activity (this time directed against heresy) led to an attempt against his life, he was allowed to retire for a time into the sylvan solitude of a village near Corvey. There it was that he wrote or put together the series of religious hymns or songs which first appeared in a collected form in the last year of the War; some thirteen years after the death of the author, in the midst of his ministrations at Trier to a population afflicted both by a pestilence and by the ravages of war. The *Trutznachtigall*—I do not like to translate *the Trusty Nightingale*, but it gives the sort of sense—with its simple strains, full of deep religious emotion, intended partly for singing in church but not as regular contributions to the liturgy, cheered many a pious heart in these times of stress and sorrow, and conquered for itself a corner in the national literature.

While it would be difficult to find a more lurid background for the study of a pious life, vocal with religious emotion, an equally true, though more tranquil picture might be drawn of the life of a devoted

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* There is something specially pitiful in the case of these young 'witches.' Elsewhere we read of that pattern of orthodoxy, Elector Max, of Bavaria, being in his youth taken by his tutor to assist at the burning of a young female witch.

Lutheran pastor intent through all the troubles of the times of reaching the inmost recesses of his people's souls. I should not take my example from the well-known figure of Paul Gerhard, the greatest of Lutheran hymn-writers after Luther, whose sufferings are partly legendary and who long survived the Thirty Years' War. I should rather take it from the experiences of Johann Arndt, who has been sometimes called the earliest of the Pietists, and who, whether or not he is entitled to that designation, is remembered as having by two books, which he wrote during the War, carried edification and comfort to innumerable humble homes in the midst of trouble and distress. His *True Christianity*, which exposed him to some tribulation (which indeed he was hardly spared at any time of his life), and his book of prayers in prose and verse called the *Little Paradise*, were cherished in numerous editions as the *Arndtenbuch*, by the evangelical population; and his mantle, there can be no doubt, fell upon the long succession of good pious men of whose names the best known to us are those of Spener and Francke. Thus, it came to pass that the Thirty Years' War was instrumental in bringing about, *through the very pressure* of the evils that followed in its train, a religious reaction against the formalism of contending creeds, and, at the same time, a deep and powerful protest against the rampant irreligion, of which among the great wars of modern times this War, by reason of its widespread, all-absorbing character and long endurance, was the most prolific. Of later German mysticism the most prominent representative, Jacob Boehme, with whom our great modern English mystic William Law was to familarize our own religious public, belongs to the period of the Thirty Years' War; but his, to tell the truth, is an isolated figure on its canvas; and although there is an undeniable inner connection between him and his predecessor Weigel and the great Pietistic and its successor the great Evangelical Movement, on which I will not dwell here, they stand historically apart from it in the period which we have been discussing, for they abstained from any thought or intention of direct influence upon Christian society and the progress of the world, and withdrew themselves into the sphere of cosmic speculation, seeking to harmonize their idea of religion with their conception of the world, its creation and government. The *Pietists* on the other hand, whether we reckon them from Arndt or from his immediate succesors, directly addressed themselves to the practical problems which Christianity—and Protestant Christianity in especial—saw before it, instead of carrying on the perennial controversy as to which is the true Christian Church, and which are its relations to the other Christian churches. Though it was in Calvinist Holland and in the person of Gisbert Voet (the adversary of Descartes, another contemporary of the Great War) that Pietism has been held to have first taken its origin in a period partly coinciding with that of the War, it was nevertheless in the Lutheran body and on German soil that it attained to its first continuous growth—and there we must leave it.

The real founder and father of Pietism—and in saying this we render no injustice to his predecessors and teachers, Arndt or Stoll or others—was Philip Jacob Spener, the fullness of whose activity begins not long before the last quarter of the century. But the men of "true Christianity," who first proclaimed the demand for free Christian life, not as superseding but as betokening that of faith—the *vivi piorum desideriorum*, who, while marking the defects of the cure of souls as at present administered, and the care of the Church as now deemed sufficient, took the new step of indicating the remedies—the men whose strenuous endeavours were decried as *pietismus* or *malum pietisticum*, just as their successors in the days of Wesley were lumped together with the despised and loathed *enthusiasts*—these were bred, at least to a large extent in the evil days of the Great War, amidst its excesses and extravagances, its inhumanity and its godlessness; and they too and their work may in no presumptuous sense be numbered among its effects.

[A.W.W.]

WEEKLY EVENING MEETING,

Friday, March 15, 1912.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

FREDERICK SODDY, Esq., M.A. F.R.S.

The Origin of Radium.

THE philosophical explanation of radioactivity arose out of some very simple chemical observations on the nature of the radioactive process. The apparently permanent and constant radioactivity of radioactive substances in general consists of two parts. One part cannot, as a rule, be separated from the substance, whilst another part is readily separated by many ordinary chemical and physical operations. The latter part is often the larger part of the whole radioactivity, but it is associated frequently with a practically infinitesimal proportion of the material. Naturally, the first explanation to present itself was the ordinary one that the radioactive substance is not homogeneous, but consists of a mixture of more than one radio-element. In this way Mme. Curie accounted for her analysis of the radioactivity of pitch-blende, and the separation from the preponderating radioactive constituent, uranium, of new radio-elements in minute amount, to which the names radium, polonium, and actinium were given. Similarly, Sir William Crookes, who separated by chemical methods the β-activity of uranium compounds, gave the name uranium X to the constituent responsible for this activity.

In a chemical examination of the radioactivity of thorium compounds ten years ago, Professor Rutherford and I found that one portion of the activity was separable from the thorium by chemical methods, and to the constituent responsible for this separable activity the name thorium X was given. Then an astonishing observation was made. Unlike any other chemical separation which had hitherto been carried out, it was found that thorium, after having been deprived of thorium X by simple chemical processes, spontaneously regenerated this constituent with lapse of time. After a month's interval from the separation a new quantity of thorium X could be obtained from the thorium, the activity of which was as great as that obtained in the first separation. In the same interval the activity of the thorium X first separated completely decayed, according to an exponential law. This continuous regeneration of the constituents responsible for the separable portion of the radioactivity was found to occur not only with thorium, but also with radium and with uranium. If a chemist observed that after a substance had been separated from impurities these impurities made their appearance again with the lapse of time, he would scarcely credit his observations. If such observations were confirmed and shown to be the invariable consequence, impurities after chemical separation being regenerated with lapse of time, the view that the elements were the ultimate unchanging constituents of matter would be undermined. The theory of atomic disintegration was founded on facts of this simple chemical nature, and not, as has sometimes been asserted, on any mathematical or physical theories as to the nature of the atoms of matter. The question immediately arose whether over long periods of time the intensely radioactive constituents separated from uranium minerals were being regenerated by the disintegration of one or more of the other elements in these minerals; and this question, as regards radium, forms the subject of the lecture.

The law of radioactive change is the simplest possible expression of what has been termed by chemists Mass Action, and has been familiar to them as " the law of mono-molecular reaction " since the classical researches of Wilhelmy in 1850, and of Harcourt and Esson in 1865. The "velocity" of the change, that is the quantity changing in the unit of time, is always some definite fraction, signified by the symbol λ, and known as the radioactive constant, of the amount remaining unchanged. The period of average life of the changing atom is the reciprocal of this radioactive constant, that is $1/\lambda$. The peculiarities of radioactive changes depend upon the fact that frequently several successive changes occur in series, a substance A changing at a definite speed according to the law stated and forming a product B, which changes according to the same law but at its own peculiar rate, forming the product C, and so on. All through, the number of atoms, of any one member of the series, changing in a given time is the number of atoms of the next product formed, the general differential equation being

$$\frac{d\,Q}{d\,t} = \lambda_P P - \lambda_Q Q$$

where P and Q are the quantities of *any* two successive members in the series and λ_P and λ_Q, their radioactive constants.

The machine exhibited (Fig. 1), of which a full description will be published separately, is designed to illustrate radioactive change, even in some of its more complicated cases, and to draw the graphs connecting the quantity of the radioactive substance with the lapse of time, when the substance in question is the first, second, third, or, in certain cases, the fourth product of the successive changes. It consists of three separate units, one of which is sketched in Fig. 2. The height of the nut on its screw represents the quantity of the radio-

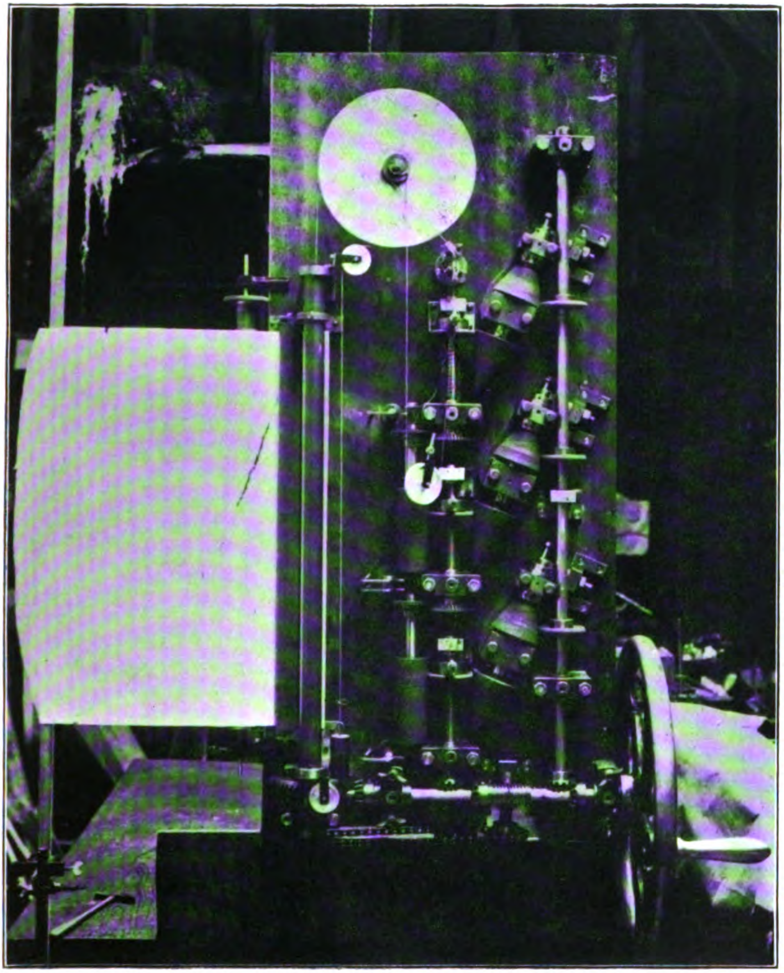

FIG. 1.

active substance in question. This height is magnified and communicated by pulleys and cords to a crayon-holder moving vertically over

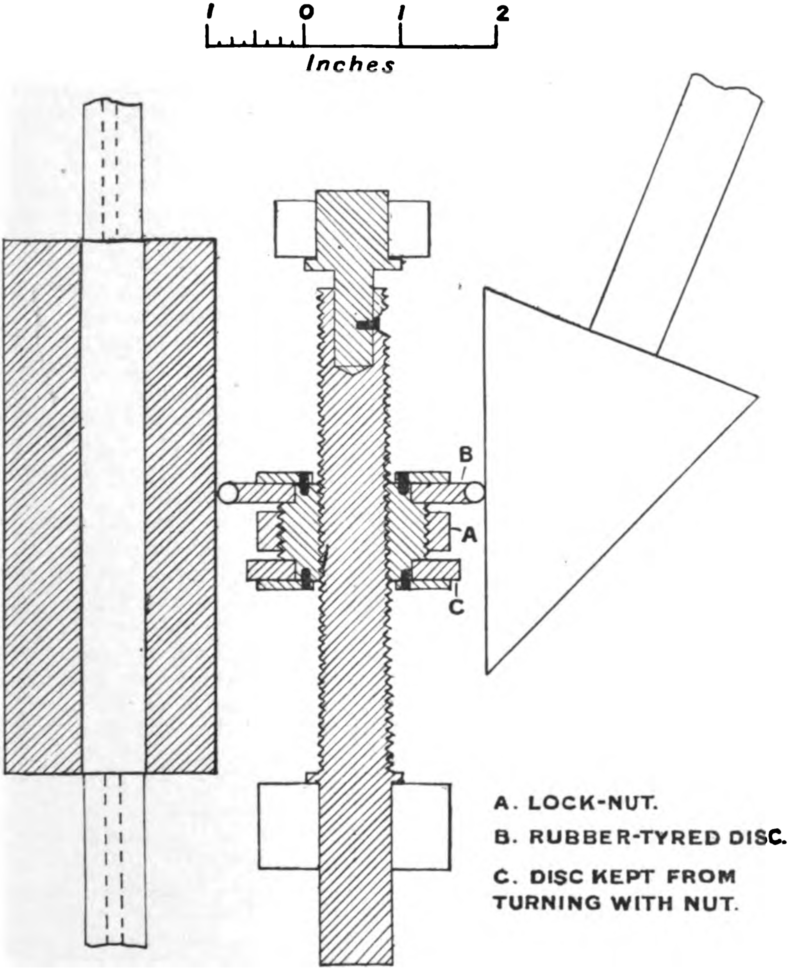

FIG. 2.

a paper roller geared to the main shaft, the paper moving horizontally in a vertical plane. The rubber-tyred nut is constantly being turned down on the screw by a rough cone, driven at constant speed from the main shaft, which presses against it. The rate at which it moves downward is always proportional to the height of the nut above the point of the cone, that is to the quantity of substance remaining unchanged. If the screw does not turn, the nut descends exponentially with the time, illustrating the exponential decay of activity of a single radioactive substance when left to itself. On the other hand if the *screw* is now turned at constant speed in the direction to screw the nut up, the nut mounts according to the usual law of the regeneration of a radioactive constituent after it has been separated, to the height such that it is turned down by the cone as fast as it is turned up by the screw. At this point it is in equilibrium, precisely analogous to radioactive equilibrium, and does not move however long the machine is driven. The rubber-tyred *nut* communicates its turns by means of a roller and gear-wheels to the *screw* of the next unit above, in accordance with the requirement that the quantity of one member changing is the same as that of the next member produced. Thus the fall of the nut by virtue of the cone turning it and screwing it down represents $-\lambda_Q Q$; the rise of the nut, by virtue of the screw turning and screwing the nut up, represents $+\lambda_P P$ of the general differential equation. The various radioactive constants are imitated (1) coarsely, by using screws with threads of different pitch; (2) finely, by varying the speed of the driving cone with reference to that of the main shaft by a suitable arrangement.

To imitate the frequent case where the period of the radioactive substance is infinitely long compared to the time of the experiment, the rubber-tyred disc may be arranged, by undoing a lock-nut, to "free wheel" without turning the nut. This then corresponds to a screw of infinitely fine pitch, or to a radioactive substance of infinitely long period. The numerous cases of radioactive change discussed in the lecture were imitated by graphs drawn by the machine.

In addition to offering a consistent explanation of all the known facts that had been accumulated in radioactivity, the theory of atomic disintegration suggested a large number of new problems. Only two of these original problems remain not yet completely answered. One had to do with the nature of the ultimate product or products of the disintegration of the atoms of the two primary elements, uranium and thorium. This problem may be likened to the task of trying to find a meteor after its flight, when its energy is spent and nothing but the matter remains. Much indirect evidence points to lead as the final product of uranium, although no direct proof has been obtained, whereas for the case of thorium there is still no hint of the answer. The other had reference to the origin of radium. This element in the intensity of its activity, and, therefore, in the rapidity of its disintegration, resembles the short-lived active constituents uranium *X* and thorium *X*, whilst in the apparent permanence of its activity it resembles the primary radio-elements. Even the first rough estimates indicated that the period of average life of radium was not greater than a few thousand years. The present estimate, due to Rutherford, is 2500 years. A few thousand years hence the radium in existence to-day will for the most part have disintegrated. Very little of the radium in existence at the time the Pyramids were being built can still exist. Hence arose one of the most interesting and crucial of the problems of atomic disintegration. Does the regeneration of radioactive constituents, observed in the cases where the period is short compared to the span of human life, apply also to radium—to an element, that is, with a definite spectrum, atomic weight and chemical character, filling a vacant place in the periodic system, and forming one of a family of common elements ? After the separation of radium from a mineral does the non-radium part of the mineral grow a fresh crop with lapse of time, the quantity present before separation being the balance or equilibrium quantity when the rate of production is equal to the rate of supply ? A somewhat similar prediction made with reference to the production of another well-defined element, helium, in the radioactive process had only to be tested, as it was first in 1903 by Sir William Ramsay and myself, to be proved correct. The question, however, of the origin of radium is still, in spite of many discoveries, not entirely solved.

At first sight the experimental trial of the view appeared easy. This problem is not analogous to the finding of a meteor after its flight is spent. The quantities of radium which can be detected and recognized unequivocally by radioactive methods are thousands of times smaller than can be detected even by the spectroscope, sensitive as the spectroscopic test of radium is. The first product of the disintegration of radium is a gas, the radium emanation, and the test for radium consists in sealing up a solution of the substance for a month, then boiling the solution in a current of air, and introducing this air into the electroscope. For the instrument employed and shown (Fig. 7), a millionth of a milligram of radium would be rather an undesirably large quantity, whilst a few hundredths of this amount is the best suited for accurate measurement. The volume of radium emanation, measured at N.T.P., obtainable from one gram of radium is only 0·6 cubic millimetre. If a thousandth part of this quantity were distributed uniformly through the air of this room, estimated as 50,000 cubic feet, or about 1⅗ tons by weight, and the electroscope were then filled with the air of the room, it would produce an effect much greater than any dealt with in the work to be described. (The effect of breaking a tube containing the emanation in equilibrium with 3 mg. of radium outside in front of the fan supplying air to the building was demonstrated by the electroscope.)

Only two primary radio-elements, uranium and thorium, were known changing sufficiently slowly to account for the maintenance of radium in the earth to-day. Indeed, these two elements are the only ones known with atomic weights greater than that of radium, and are the only ones to choose between, if radium is produced by the known process in the disintegration of one element, rather than by the at present entirely unknown process consisting of a synthesis of several together. Of these two, since Mme. Curie discovered radium in minerals containing large quantities of uranium, it was natural to select uranium as the probable primary parent in the disintegration of which radium is produced. If uranium were the direct parent of radium, 100 grams of uranium, purified chemically from radium, would generate a fresh quantity cf radium easily detected by the instrument shown after the lapse of only a few hours.

In preliminary experiments started nine years ago, a kilo of uranyl nitrate (= 500 gr. of uranium) was carefully purified from radium by precipitating sulphate of barium in its solution. The solution was preserved in a closed vessel, and tested from time to time. After 500 days it was found that the quantity of radium had increased by an amount, which although excessively minute, was about one hundred times as much as the radium initially present. The methods of testing for radium were not then so accurate or refined as those later introduced by Strutt and Boltwood, but there is no reason to doubt their general trustworthiness. The observed growth of radium was, at most, only one thousandth part of what, according to existing data, should have been produced if uranium were the direct parent of radium. Small as it was, however, this was the first observation of a production of radium. It indicated the existence in commercial uranium compounds of a substance capable of generating radium with the lapse of time. In the meantime the character of the problem had changed. As a simple consequence of the theory of atomic disintegration, it follows that, if uranium is the parent of radium, proportionality must exist between the quantities of radium and uranium in all radioactive minerals, which have not been altered by external agencies whilst contained in the earth's crust. The quantity of radium after the lapse of some tens of thousands of years will come into equilibrium with that of the uranium when as much radium is produced as disintegrates in each unit of time. Then $\lambda_P P = \lambda_Q Q$ of the differential equation, so that the ratio between the quantities must be the same as the ratio between the periods of average life of the two elements. Researches carried out by McCoy, Strutt, and Boltwood, particularly the last two, proved that this was the case. The ratio of radium to uranium in all unaltered minerals is, on present data, $3 \cdot 1 \times 10^{-7}$ to 1, that is, there are 310 mg. of radium per ton of uranium. From this ratio, the period of uranium calculated from that of radium is 8,000,000,000 years. Thus was obtained fairly conclusive, though indirect, evidence that uranium is the primary parent of radium. Unfortunately it still remains the only evidence available. To account for the excessively slow growth of radium in the first uranium preparations studied it was necessary to suppose that between the uranium and radium an intermediate product existed of period of life great by comparison with the time of the experiment. Such a product would enormously retard the initial growth of radium. Its existence complicates what first appeared as a very simple problem in many other ways. It is no longer a question of simply detecting a growth of radium. It is necessary to measure the form of the growth-curve accurately.

In the first place this intermediate parent must be present in uranium minerals, and therefore, to greater or less extent, in commercial uranium salts. The mere separation of radium therefrom initially, as in the first experiment, is not sufficient purification. In addition every trace of the intermediate parent must also be separated, or a growth of radium will not prove that uranium is the parent. On this account, in conjunction with Mr. T. D. Mackenzie, a fresh series of experiments were begun in Glasgow in 1905, in a new laboratory uncontaminated by radium. Three separate quantities, each initially of 1 kilogram of uranyl nitrate, were purified by repeated extraction with ether, which was considered to be the method most likely to separate all the impurities, not merely the radium.

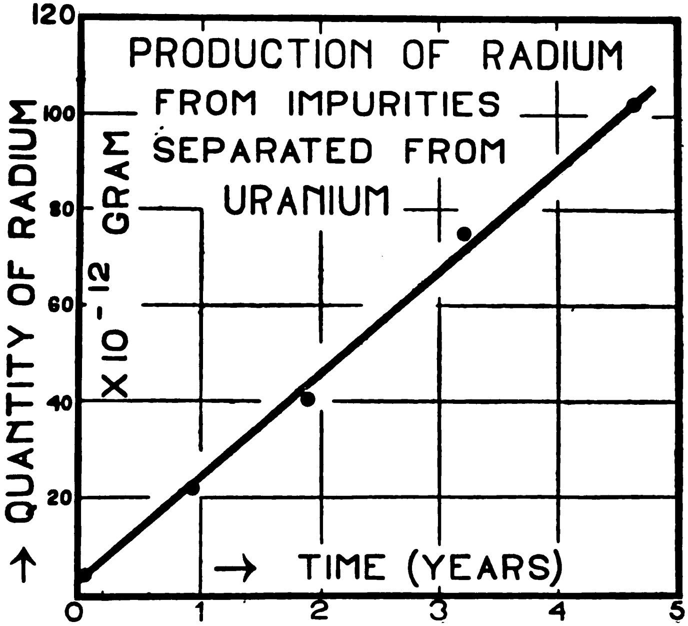

FIG. 3.

Observations on these preparations have now been in progress for six or seven years. At the same time a portion of the impure fraction separated from the original material was sealed up, freed from initial radium by the barium sulphate method, and tested for radium from time to time along with the pure uranium preparations. The diagram (Fig. 3) shows the growth of radium in this impure fraction. The unit used for expressing the quantity of radium is 10^{-12} gram. It confirms unequivocally the original observation that a substance is present in commercial uranium salts capable of generating radium and not removed from it by the barium sulphate method used first for separating the radium, but separated, at least mainly, by the ether method.

The methods of storing the preparation, and of extracting the emanation from the solutions and measuring it, are illustrated in Figs. 4 to 8. Figs. 4 and 5 represent a diagram and photograph respectively of the arrangement used for boiling out the emanation from the uranium in the flask A (Fig. 4) under reduced pressure, and of collecting it in the bottle G, from which it is expelled into the exhausted electroscope (Figs. 6 and 7). The leak is taken after 3 hours, the electroscope being kept charged throughout the whole interval. The electroscope is calibrated by similar tests with solutions containing known small amounts of pitchblende of known uranium content. Fig. 8 represents a more recent experiment with a very large quantity of uranium in which a reflux condensor is sealed into the flask. This addition makes it possible to work with large volumes of fluid even more easily and accurately than for the older quantities.

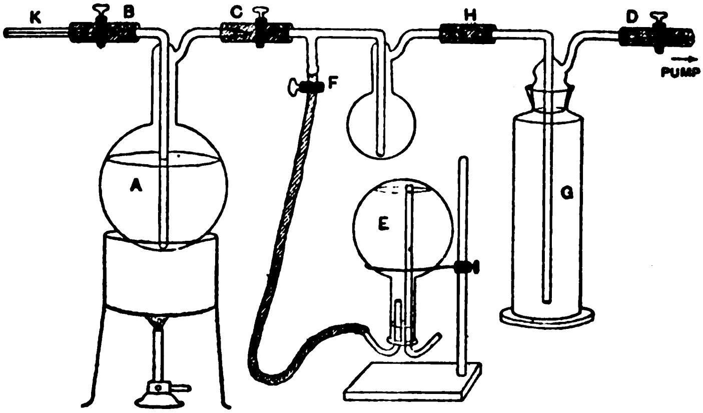

Fig. 4.

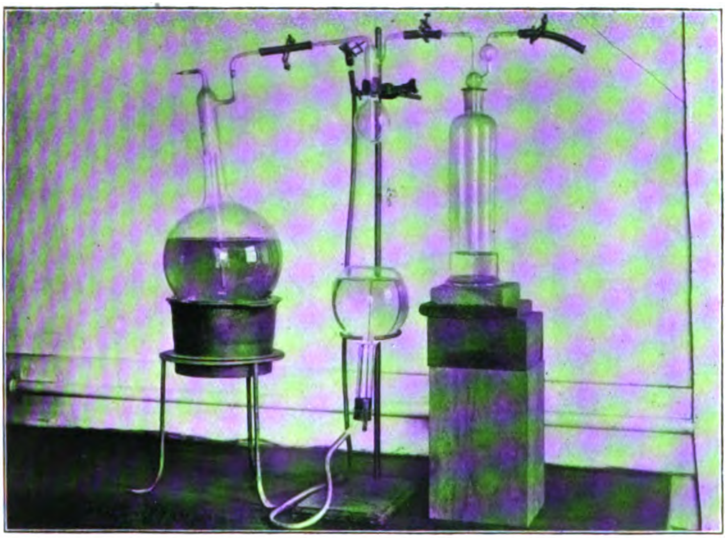

FIG. 5.

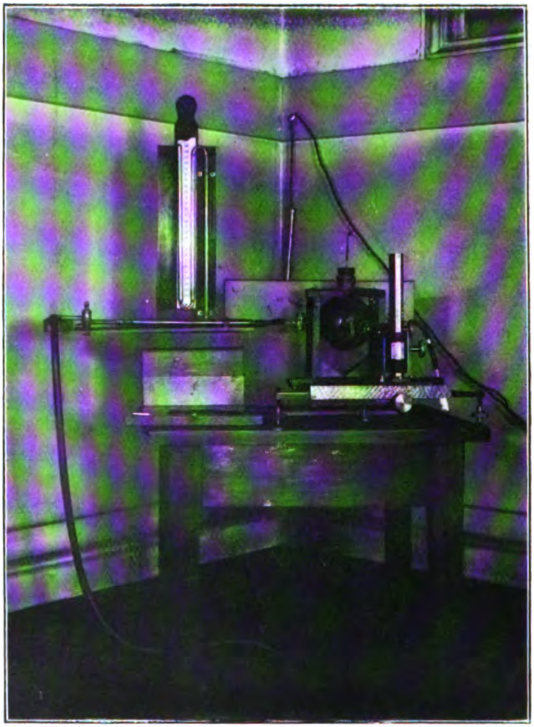

FIG. 7.

In the meantime a cognate discovery of first importance was made by Boltwood, in America, who proved that actinium preparations obtained from uranium minerals, and initially free from radium, grow a fresh crop of radium with lapse of time. The growth is not by any means a very minute one as in my experiments, in which the growth can only be put beyond all doubt after the lapse of years. The growth of radium from constituents separated from minerals can be readily detected and measured in a relatively short space of time. The curve shown (Fig. 9) is taken from a paper by Keetman (Jahr. Radioact. Elektronik, 1909, vi. 270), who has worked upon this parent of radium in Germany. Although the total quantity of radium represented by this curve is only nine millionths of a milligram, it is enormous compared with that shown by the other diagram (Fig. 3), in which the quantity of radium produced in a period about eight times longer is nearly a hundred times less. Further work on this parent of radium proved that it was not actinium, but a new radio-element admixed with it, which Boltwood called ionium. It is radioactive, and its radiation consists entirely of α-rays of very low range. The chemical nature of this ionium is absolutely identical, so far as is known, with that of thorium, and it cannot be separated from it. On the other hand, it is easily separated from any mixture, however complex, by adding a trace of thorium and separating and purifying the latter. It is interesting to note that no less than three at least of the known radio-elements—ionium, radiothorium, and uranium *X*—are absolutely identical in chemical properties with thorium. This complete similarity with known elements is one of the features of the chemistry of the radio-elements.

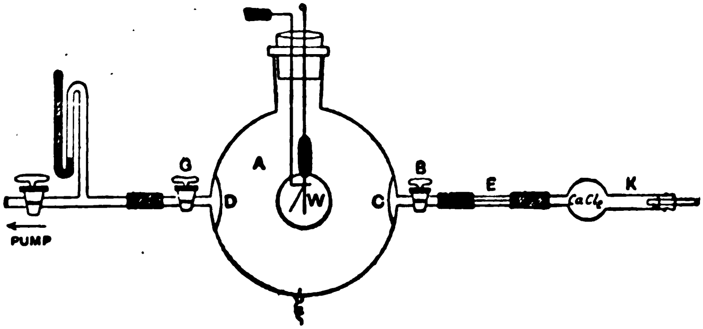

Fig. 6.

Returning to the experiments with the uranium solutions purified by ether, Fig. 10 shows the growth of radium therein. The three curves labelled I, II, III, refer to these preparations. No. III was the last prepared, after experience with the others, and contained both the greatest quantity of uranium and the least radium initially. No. IV refers to a much later experiment with no less than 6 kilograms of uranyl nitrate, purified by repeated crystallization in the course of other work. In all, there has been a distinct growth of radium, but it is so small, and the period over which the measurements extend is so prolonged, that the errors of the individual measurements are relatively great. The general scope of the curves, as indicated in the figure, are, however, probably not far wrong. A conservative view to take is that in all cases the curves are straight lines. There is some indication in No. I of an increasing slope, but it is negatived by the evidence of Nos. II and III.

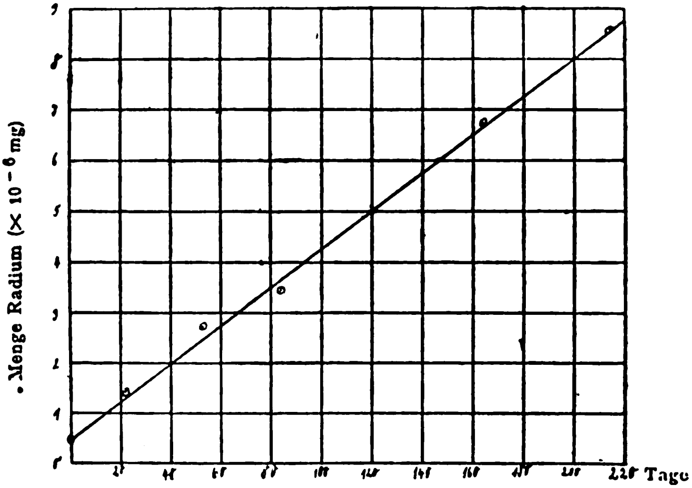

FIG. 9.

The quantity of uranium in the four preparations differs widely. In Fig. 11 the curves are replotted in a different way to eliminate this difference. The ordinates represent the quantities of radium formed in terms of the amounts of radium in equilibrium with the uranium. The equilibrium amount is the amount that theoretically should be formed after the lapse of sufficient time, if uranium is the ultimate parent of radium. It will be seen that the slopes of the four curves are all different and diminish in order, the growth in the first being the greatest, and in the last, after all the experience in methods of purification, the least. This is additional evidence that,

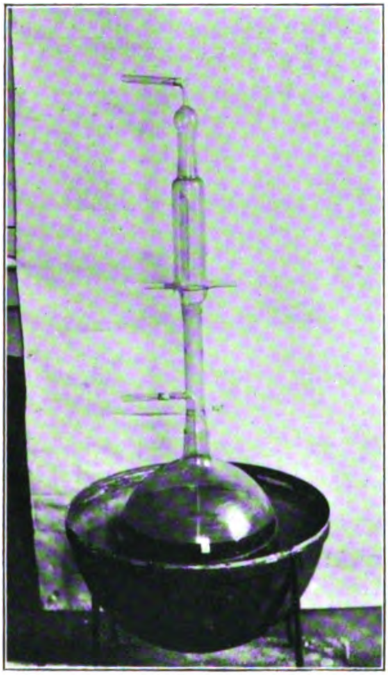

FIG. 8.

so far, the radium formed is derived, not from the uranium, but from varying infinitesimal quantities of ionium still unremoved by the purification processes.

Taking No. III as the best of the first batch of preparations, the growth of radium therein is only about $\frac{1}{30000}$ part of what would have occurred if uranium were the direct parent of radium. Some idea of the minuteness of the quantities of radium indicated by these curves can be got by the following consideration. Radium bromide at its present price costs about 16*l*. per milligram. For the element, radium, this is at the rate of 750,000*l*. per ounce. The diagram (Fig. 10) is 2 feet high. To represent a pennyworth of radium on this scale would require a diagram over 6000 feet high, whereas to represent Keetman's curve (Fig. 9) would require one as high as St. Paul's Cathedral.

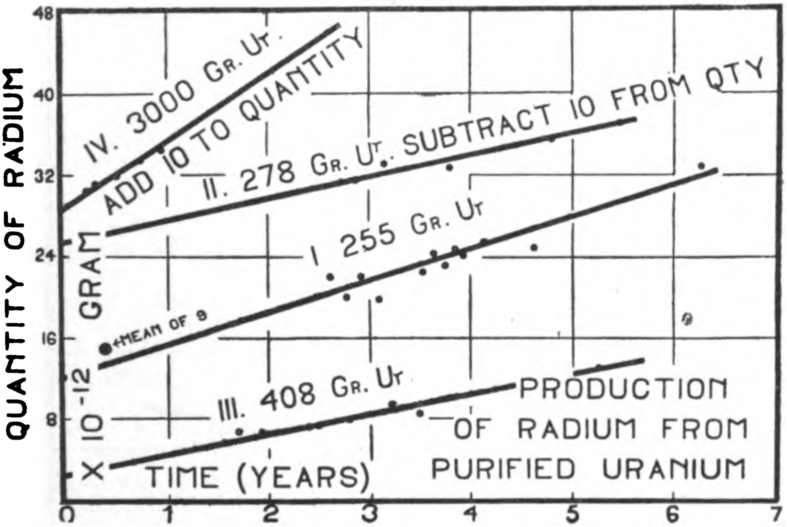

FIG. 10.

These results, therefore, confirm absolutely the view that uranium does not produce radium directly. As Rutherford first showed, if ionium is the only long-lived radio-element between uranium and radium, the growth of radium from uranium must initially be proportional to the square of the time, and should be represented by the equation $R = 6 \times 10^{-8} \lambda T^2$, where R is the radium formed per kilogram of uranium, T is the time in years and $1/\lambda$ is the period of ionium. (As was shown with the machine, with all the nuts free-wheeling to imitate radio-elements with periods infinitely long compared with the time of the experiment, when no intermediate substance intervenes, the growth is a straight line as in Figs. 3 and 9, when one intervenes the curves rise at a rate proportional to the square, when two intervene, according to the cube of the time.) Hence, if uranium is the primary parent of radium, it is to be expected that the rate of growth of radium from the preparations will increase as time goes on according to some power of the time higher than unity. As Fig. 10 shows there is still no evidence of this increase of slope in any of the preparations. This indicates, either that the period of ionium must be enormously long, or that several intermediate long-lived members intervene. If ionium is the only intervening member a *minimum* possible limit to its period may be arrived at by applying the above equation to the results. If it is assumed that the growth observed is due to uranium and that no ionium was initially present, the minimum periods calculated in the several experiments are as follow: No. I, 28,000 years; No. II, 41,400 years; No. III, 80,000 years; and No. IV, 69,200 years. Since, in all, certainly some of the

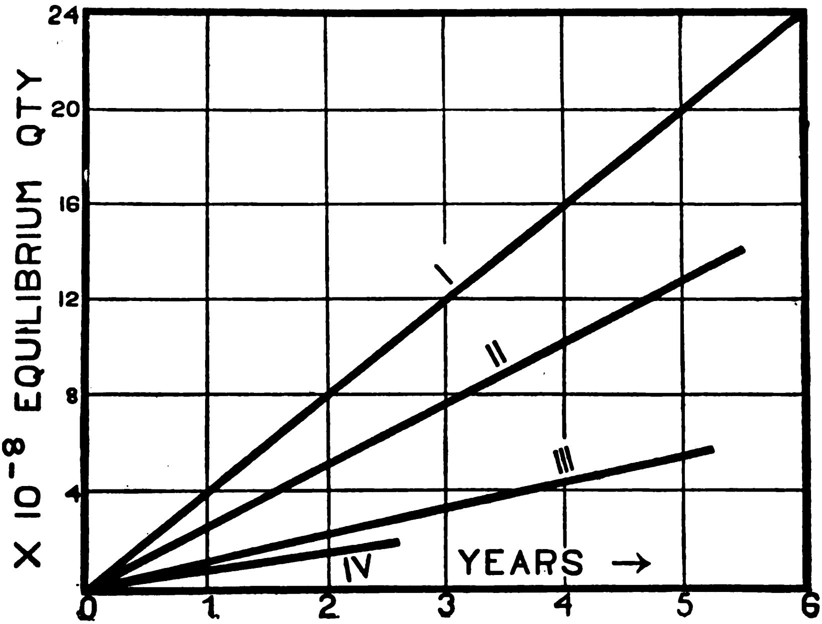

FIG. 11.

growth is due to ionium initially present, the period of ionium must certainly be greater than the longest of these periods. We may safely conclude, if ionium is the only intermediate member, that its period is *at least* 100,000 *years.* This is 40 times longer than the period of radium itself.

Entirely independent confirmation of this conclusion was obtained in another way. The gap in our knowledge is, strictly speaking, not between uranium and ionium, for the direct product of uranium is well known and is called uranium *X*. It gives β-rays alone in disintegrating and has a period of only 35·5 days, so that in all the preceding work it has not been necessary to take it into account. It would retard the growth of radium inappreciably. But, if the view is right, the product of uranium *X* must be ionium, which gives α-rays. Concomitantly with the rapid decay of the intense β-rays of uranium *X* there should occur a growth of α-rays due to the ionium produced. Whether these α-rays can be actually detected will depend on the period of ionium. Fifty kilograms of pure uranyl nitrate were presented to me by Dr. Beilby, and the uranium *X* out of this quantity was separated, in the first place by fractional crystallization of the material. The experiment consisted in watching for the growth of a very feeble α-radiation during or subsequent to the decay of the β rays. Owing to the fact that the β-rays are readily deviated by a magnet, the intense β-radiation of the uranium *X* was largely suppressed, and any α-radiation remaining could be measured from day to day, as the β-rays decayed. This experiment has been repeated many times, but with completely negative results. There always was a feeble α-radiation, due to impurities, but it remained constant throughout. Neither during, nor yet, several years after the separation of the uranium *X*, has there been any detectable growth of α-rays. Actinium was also looked for, as the origin of this element, like that of radium, remains unexplained, but the result also was completely negative. The product of uranium *X* remains unknown, and if it be ionium, the period of the latter substance must be enormous. The minimum period it could possess, calculated from these negative results, is 30,000 years. This, then, confirms the conclusion of the first totally distinct set of experiments, that ionium must have a period very much greater than that even of radium.

Marckwald and Keetman have since concluded that uranium *X* is identical in chemical behaviour with thorium, and therefore with ionium. This conclusion has been confirmed by Mr. Fleck in my laboratory after an exhaustive series of tests, not yet published. Hence in the repeated separation of uranium *X* from the large quantity of uranium the very best method was adopted unwittingly for removing ionium at the same time. This accounts for the gratifying result with preparation No. IV, which was the purest fraction of the large quantity obtained after numerous crystallizations of the original material.

The question arose whether by any means an upper limit, or maximum value, for the period of ionium could be assigned. By the law already discussed there must be many times as much ionium as radium in uranium minerals, and if the actual ratio were known the period of ionium could at once be found. For example, if the period were 100,000 years, there should be 12·5 grm. of pure ionium per ton of uranium. Auer von Welsbach, in a masterly chemical separation of the rare-earth fraction from 30 tons of Joachimsthal pitchblende, separated a preparation, which he described as thorium oxide, containing ionium, the activity of which was measured by Meyer and von Schweidler. To obtain a maximum estimate for the period of ionium, I assumed that Welsbach's preparation was in reality pure ionium oxide (which it certainly was not, as it gave the thorium emanation), and so I obtained the period of a million years as the upper possible limit. In proportion as the percentage of ionium oxide present is less than 100 %, this period must be reduced.* Thus we have fixed the period of ionium as between 10^{-5} and 10^{-6} years, if ionium is the only intervening long-lived member.

Quite recently a method has been devised for calculating the period of ionium from the range of its α-particles, which is based upon an empirical mathematical relation holding between this range and the periods of the substances giving α-rays in the case of the other members of the series.† The most recent estimate by this method is about 200,000 years, which may be accepted provisionally as the most probable at the present time. If this is correct, there should be 25 grams of ionium per ton of uranium in minerals. A variety of evidence thus leads to the conclusion that to detect the growth of radium from uranium either still larger quantities of uranium or still longer time is necessary. Even after ten years, that is at the end of 1916, if the period of ionium is as estimated, the uranium in No. III preparation should only have produced 12×10^{-12} grm. of radium, which is less rather than half the amount that will then have been formed by the ionium initially present. Nos. I and II preparations are very much less favourable. But it is interesting to consider No. IV preparation, which, though only 2·6 years old, has over seven times as much uranium as No. III. From the present slope of the curve it appears to have little more than one-half as much ionium, relatively to the uranium as No. III, whereas the relative initial quantity of radium is about twice as great as in No. III. After eight years, that is in 1917, the quantity of radium produced from the uranium should be about equal to that which will have by then been produced from the ionium present. A distinct upward slope should be detectable in the growth curve some time before this. But this is the best, if the estimate of the period of ionium assumed is correct, that the present set of experiments can offer to the solution of the problem. With the experience already gained, especially in dealing with large quantities of uranium and in the methods of measurements of the minutest quantities of radium, there should be no difficulty in obtaining and dealing with sufficient uranium, say 20 kilograms, of the requisite degree of purity as regards ionium and radium, to determine directly in a few years the period of ionium from the growth curve provided it is not greater than 200,000 years.

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* Soddy, Le Radium, 1910, vii. 297.

† Geiger and Nuttall, Phil. Mag., 1911, xxiii. 613; 1912, xxiii. 439.

A favourable opportunity is being awaited to initiate this large-scale experiment. It requires a small room to itself in a permanent institution uncontaminated with radium, and some guarantee that once installed the preparations will remain undisturbed for a reasonable term of years, and that the measurements will be continued in a comparable manner should the period of life of the original investigator prove insufficient. It is not enough to set aside a quantity of uranium for our successors to see if any radium has grown in it. It is essential that the exact form of the growth curve should be known before the problem in question can be fully answered. There *may* be more than one long-lived intermediate product between uranium and radium. However, such indirect information as has been acquired as to the life period of ionium indicates that it, alone, is sufficient to account for the present results as regards the absence of growth of radium from uranium.

I desire to acknowledge my indebtedness to Mr. H. N. Beilby and Mr. George Weller for their expert advice and help in overcoming engineering difficulties encountered in the operation of the machine, and to Mr. A. H. Bodle, the mechanic of the Chemistry Department, for his skill and energy in constructing it.

[F. S.]

WEEKLY EVENING MEETING,

Friday, March 22, 1912.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., in the Chair.

PROFESSOR D'ARCY W. THOMPSON, C.B. M.A.

The North Sea and its Fisheries.

FOUR-SQUARE the North Sea lies, and its gates are three. To the northward lies the broad opening to the northern ocean, a frequented highway of the fisherman, where the sails of commerce are few. The eastern gate is the wide channel of the Skagerack, that leads through the narrow passes of the Belts and Cattegat to the great inland Baltic Sea; I like to think of it as an old road, a route of very ancient trade, the old highway of the Hanse merchants, the road to Muscovy! And lastly, in the south-west, there is the narrow strait that widens into the British Channel, the chief and busiest street of the modern maritime world. Of these three gateways, two open to the ocean and one to the inland sea, two to the salt waters and one to the brackish or the fresh; and herein, as we shall see presently, we have the simple clue to much of the physics and not a little of the biology of the North Sea.

Sailing in imagination round the North Sea, we pass from the rock-bound shores of northern Scotland, through all the varied scenery of our eastern borders, to the dull levels of the Dutch and Frisian coast, to a long line of low-lying shores, sandy or muddy, fringed with low islands, where through islands and broken coast the great rivers of central Europe find their outlet to the sea. Along the shores of Jutland, low, level stretches of sand confront us, until, crossing the Skagerack, we are again of a sudden in presence of rock and precipice. Then onwards along the many hundred miles of Norwegian coast we have more or less similar scenery of cliff and mountain, often glacier-topped, and the broken barrier of islands, behind which the deep fjords are sheltered from the Atlantic billows.

Around this long coast-line the fishing population is universally but unequally distributed. In the old days almost every sheltered creek or sandy bay, where the boats could be drawn up in winter, received its sparse settlement of fishermen, and their number, if in part regulated by the nature of the coast, was still more governed by the racial characteristics of the people: for some breeds of men are fishermen born, and some are not; and some races, such as the Cornishmen, the Dutchmen, and the Norsemen, were long pre-eminent, and the

Dutch the greatest of all. In the days of Queen Elizabeth, before ever a herring was caught by our own people, the Dutch sent to our coasts a yearly herring-fleet of 3000 sail. It was Dutch colonists, under William of Orange, who first taught Englishmen to trawl at Brixham; and to that Brixham fishery, and the direct influence and participation of the men who conducted it, all our modern trawling industry harks back. And again, in Scotland, our prosperous east coast fishery, far different from the struggling efforts of the western Celt, owes its origin to those Dutch and Frisian settlers who (as history and tradition tell us) came over under Mary Queen of Scots and her son, and who still retain no small trace of their origin in speech and custom and costume. These good people present a problem to the administrator, when (as oftentimes) they cling not only to their old ways, but, resisting all economic tendencies to concentration, cleave to the ancient homes of their forefathers, and make heroic efforts, and demand the like heroism on the part of His Majesty's Treasury, to fit their multitudinous petty havens to the needs of an enlarged and altered industry.

It is different with the great centres of the modern trawl-fishery, the site of which is determined by deep-water harbours, by proximity to a great capital, or by convenient railway facilities. These conditions greatly limit the number of trawling centres, of which Grimsby and Hull, Aberdeen and Granton, Ostende and Ymuiden, Geestemunde and Cuxhaven, are the chief. Proximity to the fishing-grounds matters less to these distant voyagers than to the herring-fisher. With him, ports contiguous to the successive seasonal fishing-grounds are a prime necessity, and railway facilities are of minor importance; for the fish must be cured in haste—and may be exported at leisure, generally (because most cheaply) by sea. And so it is that all down our east coast the herring ports are numerous, and are often remote from the greater centres of population.

The North Sea is a very shallow sea. We can sail from here to Hamburg, save for one little bit, in water under 20 fathoms deep, and from here to the north of Denmark in water that never exceeds 30 fathoms. Suppose the bottom of the North Sea to be raised up by successive stages—raise it by 10 fathoms, or 12 paces (just the breadth of this street from wall to wall), and immediately the islands of the Frisian shore are linked together in an even coast line, while a broad belt ten miles broad or more is added to the Danish coast; a multitude of low islands spring up off the Belgian and East Anglian coasts, and a greater island rises up in the Dogger Bank, where even now in heavy storms the waves break upon the sunken land. Let the North Sea rise but 20 fathoms, and from Flamborough Head eastward dry land fills the southern North Sea, save for a shallow inlet parallel with our coast, that has been scoured out a little deeper than the rest by the tidal inflow from the Channel; the Dogger Bank is now a great low island some 150 miles long. Let our upheaval proceed some 10 fathoms more, or 30 fathoms in all, and now from the Yorkshire coast straight across to the most northerly point of Denmark the new shore line appears; and all to the south of it, an area of some 70,000 square miles, is now dry land, save for a few small lakes, chief among which are the celebrated Silver Pits, where nowadays the soles congregate. Once more let the bottom of the North Sea rise up 50 fathoms, or 300 feet (not yet near the height of St. Paul's), and the new coast line now runs round the Orkney Islands, and then from somewhere about Peterhead through the Skagerack to Sweden, with one conspicuous dip or bend, that under the conditions we have imagined would form a sort of new Zuyder Zee. Northward, far beyond the 50-fathom line, and away to the north of Shetland, the comparatively shallow bottom of the North Sea slopes downwards to the north, until we reach the 100-fathom line a little to the north of Shetland. But some 60 miles from the Norwegian coast this 100-fathom line bends southward, until it, like our other contour lines, enters the Skagerack. The deep groove that surrounds the Norwegian coast, and cuts off from it the comparatively shallow plateau of the North Sea, is a geographical feature of great importance, whose meaning and history has not yet been fully told. The 100-fathom line is succeeded to the northward at no great distance by the 200-fathom line, and beyond this the depths increase rapidly, for we are now at the edge of the Continental shelf, and the old abyss of ocean is but a stone's throw away. Elevate, then, in imagination, the bottom of the North Sea by, say 150 or 200 fathoms (rather less or rather more than the length of Albemarle Street), and all the North Sea to beyond the Shetlands and all the British Islands and the British seas become part of the Continent; all that remains of the North Sea is a large lake, immensely deep, that occupies the greater part of the present Skagerack, and continues the chain of great deep cold lakes, with their ancient faunas, still showing traces of their origin from the sea, that are so conspicuous a feature of the geography of Sweden.

Of all these physical features the greatest is that which is represented, or approximately represented, by the 100-fathom line. The geographer traces it along all the western coasts of the Old World, from the north of Norway to southern Africa. It encircles our own islands, it broadens here and there, it is the edge of our continental area, and beyond it the Continent plunges into the abyss of ocean. The geologist sees in it, in all probability, the actual coast line of early Tertiary times after the great changes that had raised part of the bed of the cretaceous ocean into dry land: the coast line of an age when broad plains or chalky downs stretched over the North Sea. And now that subsequent and successive changes, in which again subsidence and upheaval have alternated, and the great ice sheet has scraped and scooped the North Sea and filled its bed to unknown depths with its drift and clay, now over the shallow slopes and levels that the 100-fathom line bounds, the fisherman finds his place and calling. Here and there in the world, as for instance, off the coast of Portugal, are isolated deep-sea fisheries; here and there the adventurous trawler, or halibut-fisher, plies his craft on the deeper slopes of the continental shelf to 200 fathoms, or a little more; but broadly speaking, the 100-fathom line bounds and limits the ordinary operations of the fisherman. Where that continental shelf narrows, the fisherman's field is narrowed; where it widens out, he finds an ampler range; and in the region of the White Sea and the Murman coast, the whole of our North Sea area, in a belt round our western coasts, a broad girdle round France, a narrow one off Portugal and Spain, here and there in Africa, as off Morocco and down in Greyhound Bay—in all of these regions the continental shelf or plateau extends its rich and productive bed a long way from the land, and yet but a little way into the territory of Ocean.

What I have called the gateways of the North Sea are not merely highways of commerce, they are the doors by which Ocean itself enters into the narrow seas, bringing with it its quickening influence on life, and its regulating and ameliorating effects on climate; and there have been times when one or another, or all, of these gates were closed. It is to the opening or shutting of these gates, and of others leading to more southern seas, that the geologist ascribes much of the successive changes of climate and of fauna during Tertiary times.

The topography of the North Sea, as well as of our land, bears its fragmentary records of these old times. The Dogger Bank is perhaps but a great moraine, and over it (when the great ice-cap had passed away) roamed the rhinoceros, the reindeer and the mammoth. The deep groove off Norway was probably in part a channel whereby the river system of eastern Europe ran seaward, in part an eddy, where the Scandinavian glaciers gripped and scooped their hardest, and first of all, probably a great crumple in the earth's crust. In the Moray Firth a deep channel, more than a hundred fathoms deep, exists; it is the course by which an older and greater Spey ran tributary into an older and greater Rhine.

Apart from the great tidal waves that roll in twice a day from the ocean round by our northern and southern gates, the great dominating movement of our seas lies in the Atlantic current, or system of currents, that we commonly call the Gulf Stream. The Gulf Stream itself is a river in the ocean (as Maury called it); but partly as a river, and in part as a great, wide, slow-drifting flood, the warm waters of the bosom of the Atlantic creep ever northward and eastward to bathe our shores, and to soften the climate of sea and land in northern Norway and distant Spitzbergen. A little branch of the current enters in by the southern gate, a somewhat greater eddies round the north of Scotland, and under these two impulses (aided by local differences in the density of the waters of the North Sea basin) a circling current flows down our eastern coasts, across to Denmark, and in part out again along the Norwegian shore. The direct influence of this system of currents on the life of fishes is immense, for by its means their floating eggs and young are dispersed and disseminated broadcast. In the south those of the plaice and sole are carried over to their nursery grounds on the flat Danish shore; and in like manner the eggs and fry of the cod are drifted from the western coasts round the north of Scotland into the North Sea, and in part out again to the Sea of Norway.

Simply and clearly we may see by our chart the distribution of temperature in the north Atlantic, due on the one hand to the Gulf Stream current, and on the other to the opposing currents from the Pole, that bend westward in their southerly course and cool the Newfoundland Banks and the shores of the Eastern States, while a minor offshoot from the direction of Iceland, submerged beneath the warm Atlantic waters, approaches or invades our own seas. We see, in passing, the close pressed isotherms on the Newfoundland Banks, where the two waters meet, and we may note, by the way, that it seems to be a fact that fish tend to accumulate just at such meeting places of different waters. But looking broadly at our own temperature phenomena the most striking points are: our western coast bathed by the warm current, the eastern remote from its influence; again the rapid change of temperature from the favoured regions of southern Ireland and south-western England as we go farther north; and lastly the uniformity of temperature over the wide region that sweeps round from the North Sea by way of Iceland all round the North Atlantic to Newfoundland again.

The difference of temperature between our western or southern coasts and the eastern is in close relation with the great contrast between the fish of the two regions. Broadly speaking, to the former belong southern fishes, while fishes whose home and distribution are in the north characterize the latter. There cannot be a more striking contrast than that between one of our fish markets and a market of Lisbon, Genoa or Marseilles. The cod and the haddock, and nearly all their allies (save the hake) are absent from the latter; flat fish are few, and the great order of the spiny-finned fishes, the bream and the sea perch, the mullet, the gurnard, and a multitude of others, mostly alien to our markets and strange to our eyes, form the staple commodity. A difference, similar in kind though less in degree, exists between our western fisheries and those of the North Sea. The pilchard, the chief Clupeoid of the Atlantic coasts, finds its appropriate temperature on the Cornish coast, and rarely penetrates the colder waters to the east. The hake, which takes the place of the cod along the Atlantic seaboard, comes round indeed into the North Sea with the Gulf Stream eddy, but in meagre quantities. The bream, which both fresh and salted is an important food of the poor on the west of Ireland, is not in the North Sea an article of commerce.

The trawlers that seek the coasts of the Spanish Peninsula and of Morocco find in these warm waters a fishery totally unlike that of the North Sea; while, on the other hand, our temperature curves make it plain and easy for us to understand how the North Sea has common attributes with regions so far off as the White Sea itself, with Iceland and Newfoundland and the Eastern States, and how our staple fishes, such as the cod, the haddock, the plaice and halibut, and the herring itself, find their extensive distribution in all these remote, but not dissimilar seas.

Lastly, ere we leave this matter of temperature, let me point out to you that the ocean not only acts in this part of the world as a warming influence, but also here and everywhere has a great steadying influence upon the temperatures. In another chart I show, not the mean temperatures, but the *range* of temperature, the difference between the summer heat of the sea and its winter cold. A little way west of Ireland the annual range of temperature is but 4°, and in Shetland it is but 6°; but the further we go into the narrow seas the more violent is the seasonal fluctuation, the greater is its range, till down in the German bight you have a range of at least 12 or 14° C., or 30 to 35° F. The water there is far colder in winter than in other parts of our sea. But there comes a great compensatory warmth in summer, which again has its influence in favouring this region as a nursery for young fish.

The problem of salinity, the distribution of the amount of salt in the sea, is a laborious one to investigate, but, as far as the North Sea goes, its main results are easy to understand. Some of you will see at a glance from this chart of Isohalines, how beautifully simple the arrangement is, and how perfectly it is in accord with the distribution of the three gateways of the sea, the two inlets of salt water, and the great Baltic source of fresh. But the further study of the salinity of the North Sea is very complicated indeed, for the mean condition which my chart represents is subject to change, and the changes are partly regular or periodic, and partly irregular and obscure. There is a constant battle, as it were, between the quantities of fresh water, on the one hand, that the Baltic sends in, and the rivers bring down (for the former source especially tends to be dried up when the inland sea is frozen in winter time), and the varying supply of salt water from the ocean, for even the great ocean currents have their annual pulse, their ebb and flow. In the summer time over great part of the North Sea, water of low salinity spreads from the Baltic, and such changes as this have, we have every reason to believe, their close and intimate bearing on the migrations of the herring.

Lastly, together with these physical phenomena of salinity, temperature and current, we study the distribution of plankton, as it is called nowadays, the floating life of the sea. On his great voyage across the ocean, Darwin himself spoke of it as a weary waste of waters. It was but a few years afterward that Johannes Müller and others showed that every gallon of the waters over which we sail is a teeming world of microscopic life. A thousand varied forms people the surface waters. Some have their home around the shores while others are denizens of the great ocean currents, and these coming more or less periodically within our reach, mark and render visible the currents to which they belong. These organisms are animal and vegetable, and among them the myriad tiny green algæ play their part in the economy of Nature, renewing in the sunlight the oxygen of the sea, as the green herbage restores the balance of oxygen on land. Some few fishes, but fishes of great importance, feed all their lives upon plankton organisms, and their distribution is accordingly closely correlated with the abundance of these. The herring feeds, as many of the great whales do, on the teeming shoals of small crustacea that are especially characteristic of northern seas; the pilchard, which at times feeds on the same diet, is said to come to the Cornish coasts at the season when minute vegetable organisms reach their greatest abundance. But in early life all fishes whatsoever live on these floating microscopic organisms, on diatom and peridinian and copepod, while these same organisms are again the nutriment, direct or indirect, of the multitudinous worm and shell-fish and crustacea on which the older fishes are in turn nourished. There is another and more difficult chapter still of the same story, relating to those yet smaller organisms, the bacteria, by whose subtle alchemy the nitrogenous contents of the water are controlled, and which lay the first foundations of the ladder by which the inorganic elements pass into the fabric of living things. And lastly, among the elements of the plankton must be reckoned the eggs and earliest stages of the vast majority of our food fishes. For it is an elementary and cardinal fact that, with the single important exception of the herring, every food fish of our seas lays eggs, tiny, globular and transparent, which float in the surface waters of the sea. The eggs of the herring, on the other hand (as Walker showed in 1803, and as Goodsir and Allman rediscovered), are laid in sticky masses attached to weeds and zoophytes at the bottom. Here they are devoured in quantities by the haddock and other fish, and here they may at times be disturbed by the operations of the trawler, while the eggs of all the other food fishes float safely and undisturbed above.

But it is high time to pass to the fisheries of the nations bordering on the North Sea, and to consider their scale and magnitude in the briefest possible review.

Wherever there is sea-coast there are fishermen, and accordingly all the North Sea nations participate in the fishery; but the extent to which the fishery is pursued, its actual produce, and its importance relatively to the other sources of each country's wealth—all these things differ greatly.

Taking the last year (1908) for which statistics are easily available, Great Britain and the other five North Sea powers bring to land some two million tons of fish a year; and of this great quantity Britain has for her share over 60 per cent., Norway has 25 per cent., and the other four nations share among them 15 per cent. of the whole. Of the grand total catch of Great Britain no less than 84 per cent. is landed on the east coast of England and Scotland.

The composition of the catch is very different in different countries. I show you a diagram to illustrate how overwhelming is Norway's catch of cod; and another to illustrate the absence of plaice from the fisheries of that country, the small importance of this fish in Scotland, its greater importance in England, and its especial and peculiar predominance in Denmark. When we deduct our three staple fishes—herring, cod, and haddock—there remains less than 10 per cent. of the Scottish catch, a fifth of that of Holland, a third of that of England, about half of that of Denmark, two-thirds of that of Belgium.

When we translate the above catches into money-value, we find that six nations earn from their fisheries close upon twenty millions a year (or say, 50,000*l.* a day), of which Britain takes 11,000,000*l.*, or actually about 62 per cent.; and that first return is probably trebled, or nearly so, by the indirect earnings and profits of the trade. The several shares are not alike in regard to quantity and value: for instance, Norway, with about a quarter of the total catch, has but an eighth of the total money-value, for her cod and herring are relatively cheap; while Denmark takes over 4 per cent. in money in return for a little over 2 per cent. in quantity, for her plaice and eels are costly fish.

But without pressing statistics further, it is plain that the small or even petty shares which certain countries earn from the fisheries are far from being less vital to them than is our greater share to us. It was common for our older writers of two centuries ago to attribute the wealth of Holland wholly, or almost wholly, to the herring-fishery. "It is almost wholly from the Herring-fishery," says one, "that they have raised a country labouring under the disadvantage of intemperate air, excessive Expense in maintaining their Dykes, and want of almost all those Necessaries in which we so greatly abound, to that Plenty, Wealth and Power they at present enjoy." And when Charles V. made his pilgrimage to the tomb of the man who, long generations before, had invented pickled herrings, he manifested a similar belief. If no nation be nowadays so exclusively dependent on this or any other single industry, yet we may easily realize that, wealth and population considered, the two millions that Norway earns, or the three-quarters of a million that Denmark earns, from her fisheries, are, more even than in our case, of indispensable and immeasurable importance to the support and well-being of the people.

When we come to consider the quantities of fish that come from the North Sea, we find that England, in spite of the distant voyages that some of her trawlers make, and in spite of the considerable fisheries of her western and southern coasts, still takes two-thirds of her whole fish supply from that great fishing ground, the North Sea. Scotland takes an even greater part, more than four-fifths of the whole, and Holland, whose herring fishers go as far as Shetland, does not go beyond, and takes practically the whole of her fish from the North Sea area. Germany, on the other hand, takes only half her supply from the North Sea, the rest coming from the Baltic, and in part from her Iceland and other deep-sea trawlers. Denmark, again, gets the bulk of her supply from her Baltic coasts; and Norway, whose greatest fisheries lie far north upon her Atlantic shores, takes only one-fifth of her total catch from the North Sea.

Numberless methods are employed for the capture of fish, numberless modifications of bait and trap, of net and line; but for our purposes we may speak particularly of three only, the methods of the line-fisher, the fisher of nets, and the trawl fisherman. In each one of these methods great changes have taken place within recent memory, changes that have revolutionized the industry and brought far-reaching consequences to the lives and prosperity of the fishermen.

Eighty years ago there was not a single first-class fishing-boat, not a single fishing-boat over 30 ft. long, in Scotland. Thirty years ago there were more than 5000 such, and our Board in its first Report said, even then, that there had been a revolution in the industry. But another and a greater revolution had yet to follow, for trawling was then in its infancy, and steam had scarce begun to oust the sailing boat. We have now in England some 1300 steam trawlers, in Scotland about 300, and about 400 more in the rest of northern Europe. Besides this, we have in Scotland about 1100 steam fishing-boats other than trawlers—mostly herring-drifters, whose value is about 2½ millions of money; England has between 500 and 600 of these, and the rest of northern Europe at the last statistics about 150.

Steam and ice and railway facilities have done, in the last generation, for the fisheries what steam had done for the spindle and the loom: to the immense advantage of the people at large, and with the inevitable accompaniment of loss to some. But in the case of the fisheries, the loss and hardship have been tempered and attenuated by the fact that the great herring industry has, in great measure, escaped the tendency to concentration, both in regard to locality and in regard to capitalization. Even the large steam-drifters, costing over 2000*l.* a-piece, are, to a very large extent, the property of the fishermen themselves. The fishermen remain free men; they are independent, industrious, and prosperous; and, speaking at least for Scotland, though there are fewer fishermen than there were forty years ago, I think there can be no doubt that their prosperity as a class was never greater than it is now.

Let me say a word about the herring fishery. The herring constitutes more than two-thirds of the total quantity of fish landed in Scotland, and considerably more than half the value of the whole; and in Holland the numbers are all but identical. In England, on the other hand, it represents less than one-third of the entire quantity, and about one-eighth of the total value. If we deduct trawled fish, and deal only with the produce of the less capitalized industry, the industry of the men of net and line, then the comparison becomes still more striking; for we find that in Scotland 87 per cent. of the catch of such fishermen, and 83 per cent. of its value, is contributed by the herring alone. It is, and always has been, the mainstay of our fisherfolk.

There are many ways of catching herring. In the shallows of the Baltic Sea they capture them with fixed nets, forming great complicated traps. In Norway, in America, and to some extent on our west coast, they encircle them with a seine, after the manner of the pilchard fishery. But the great North Sea fishery is by means of the drift net, roped and buoyed, which forms a vertical wall, miles long, against which the shoal swims, and the fish are caught fast by the gills. Two hundred million square yards of netting are used in our Scotch herring fishery. The net is only a narrow strip, but make it into a single square, and it would more than cover London.

The herring is a northern fish, but it is one of the most widely distributed of fishes. It surrounds the North Atlantic, and even extends into the Pacific, where it forms one of the chief fisheries of Japan. But even in our own area the herring are not all alike, but fall into several well-marked varieties, or separate races. We have, for instance, the winter-herring, that breeds close inshore in early spring, loving water that is but little salt; and in the North Sea we have several races of such herring as this. Then we have another and greater sort, or set of races, that breed in summer and autumn, and these the fishermen follow throughout the year. They begin in spring or early summer to fish in the Hebrides a great herring whose home is in the Atlantic; a month or so later the fleets are in Shetland, first on the west and afterwards on the east coast; in the height of summer and early autumn the Scotch east coast fishery is at its height, and by taking the average of many years we can precisely mark the successive dates, following each other week by week, or day by day, when the fishery culminates at successive points more and more to the southward along the coast. By October the fishery of the north-east coast is over, and the fleets are gathered at Lowestoft and Yarmouth, but here the herring that they capture is of another and a smaller race; and in the winter-time yet another, but lesser fishery, occurs in the Channel. I show you a few pictures of the busy times of the herring fishery.

The great bulk of the produce of the herring goes abroad, most of it by Königsberg and Danzig and Stettin, to those Eastern provinces by the Oder and the Vistula, where even in Strabo's time dwelt the tribe of the Ichthyophagi. But our own food-supply comes mainly from those fishes which, unlike the herring, dwell at the bottom of the sea, and are caught, not by net, but by trawl and line. Of such fish the trawler brings in everywhere nowadays the bulk of the supply. In Scotland, owing to the growth of steam-lining, he accounts for but 75 per cent. of the whole, but in England the trawler yields us 93 per cent. of these so-called "demersal" fish, such as the cod and the haddock, the plaice, turbot, and sole: of the last, indeed, he gives us every one. Hence the great modern concentration of this industry in a few great harbours and markets, such as Grimsby and Aberdeen. I show you a diagram of the percentage of all such fish (all fish other than herring) monopolized by Aberdeen alone, which, thirty years ago an unimportant fishing station, now provides us with about 70 per cent. of the whole Scottish supply.

The English trawling industry, far as it extends, is still busiest and most intense in the region of the Dogger Bank, where every square mile yields over five tons of fish in a year. But this is by no means the richest part of the North Sea, for, measured by the daily catch of a trawler, the quantities steadily increase as we go northward; the kinds, however, are different, and it is the cheaper and coarser fish that swell the northern catch. But I can speak no more on this subject; I can only show you a few pictures to illustrate the great market of Aberdeen, where 400 tons of fish or more are laid out every morning of the year, a market, however, which Grimsby still surpasses in magnitude. And, by the way, we had an average of 650 tons in Aberdeen every morning of last week.

I have spoken, ever so briefly, of the North Sea as it appears to the topographer and the physicist, and of the fisheries as the economist and statistician deal with them, but I have said even less of the special studies of the biologist. He has to deal with and investigate, for instance, all the questions appertaining to the food of fishes, to their rate of growth (by means of the rings upon their scales, the concentric zones of their ear-bones, and in other and more indirect ways); by marking living fish he studies their migrations and their diverse rates of growth on different grounds; and he enquires into the question of their local races and varieties, and all the complex problems connected with their multiplication and their distribution.

In the end we come back to the ultimate problem of all, the most practical and urgent of problems, the statistical question, whether the fish in our seas are being diminished in number by the operations of man. A whole lecture would scarce be enough for me to explain to you the difficulties of this problem, the methods by which it is attacked, and the preliminary conclusions which we may more or less confidently affirm. Let me say this in a word, that there is no one answer to the question, but that we must separately set and answer it for each species of fish, and even for this or that particular ground. More than a hundred years ago, when our fisheries were trivial, the haddock deserted our coasts, and became, for the time being, a rare fish. Again, in 1866, long before steam-trawling began, Huxley's Commission reported that the haddock was the only fish of which it might perhaps be said or shown that its numbers had suffered diminution. In Great Britain alone, we take 100,000 tons of haddock a year from the North Sea, and, in spite of fluctuations, I cannot find that its numbers perceptibly or significantly diminish. The cod shows no signs of recent diminution, and has even been increasing in the north. It is otherwise with the plaice, whose diminution was already made clear to the Committee of 1893. All authorities are agreed that this fish shows serious diminution ; and only next month our International Council meets at Copenhagen to take in hand, after long investigation, this important and burning question. The plaice is of small comparative importance to us in Scotland, for, as I have already shown you, our plaice are few ; but even in Scotland our statistics tell us that the diminution of this fish, and especially of the large plaice, has been great and rapid.

Many important questions I have had to leave untouched in this hurried sketch, but on one of these I must yet say a word, I mean the case of the small fisherman. We have seen in many ways that the industry as a whole tends towards concentration, to the use of larger boats, to the need of greater harbours : tends, in the case of line and trawl fishing, to gravitate towards the great centres of population and the great highways of traffic. And we have seen that an overwhelming proportion of the gain goes to those who work the fisheries on this larger scale, and that from their labours comes an overwhelming proportion of the supply. But there are still some 6000 small fishing-boats in England and 8000 in Scotland, and (though it is impossible to obtain exact figures) I think that about one-seventh or one-eighth of the 35,000 fishermen in Scotland, and a somewhat larger proportion of those in England, still live, as their fathers lived, by a petty industry, an industry closely akin to that by which thousands of men in Norway and Denmark live. With us they are the men who have been left behind, sometimes from lack of energy, often through poverty or the remoteness of their habitations, by the tide that has carried so many of their fellows to wider efforts and to comparative wealth. They are the fishers of crab, and shrimp, and lobster, the hand-line fishers of plaice and haddock and codling, the men who take, now and then, a day at the lines, a night at the herring, the dwellers in the antiquated harbours and in the tiny creeks of outlying coast and distant island. The kindliest of Scotch proverbs tells us that "it takes all sorts to mak' a world," and these men have their claim upon us and their right to live. It is not too much to say that nowadays every fishery department in the kingdom is making these men's case the subject of its anxious and peculiar care.

It is partly for biological reasons, connected with the preservation of the general supply of fish, but it is in great part for these men's sake, and for the line-fishers in general, in order that they may have a stretch of waters of their own, that we close against the trawlers the territorial and more than the territorial zone. When we close to trawling the waters of a shallow and sandy coast or bay, we are, on the one hand, encouraging the lesser fishermen of the coast, and, on the other hand, we are trying to protect the young fish, flat-fish especially, whose nature it is to congregate on such grounds.

In some ways I think that the fishing industry, and the trawling industry in particular, may justly and rightly, and for the general good, have to submit in the future to greater restrictions than in the past—restrictions especially aimed, for the benefit of the industry itself, at lessening the waste of the younger fish. But, as Huxley said years ago, " Every legislative restriction means the creation of a new offence ; means that a simple man of the people, earning a scanty livelihood by hard toil, shall be liable to fine and imprisonment for doing that which he and his fathers before him had, up to that time, been free to do ! " Science, practical policy, and the interests of class and of constituency, do not always tell the same story. And if responsibility be great upon the legislator, it is scarcely less upon the scientific enquirer, who, without pressing his side of the case too far, nor thinking that his opinion is all in all, must yet play a considerable part in reporting upon the merits of all fishery legislation, and in advising as to what had best be done, what it were better to leave undone, in the best light of his judgment, and with regard to the best interests of all.

[D. W. T.]

WEEKLY EVENING MEETING,

Friday, March 29, 1912.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D. F.R.S., President, in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. M.A. LL.D. D.Sc. F.R.S. *M.R.I.*, Professor of Natural Philosophy, Royal Institution.

Results of the Application of Positive Rays to the Study of Chemical Problems.

[The substance of this discussion is included in a paper on Further Experiments on Positive Rays, in the 'Philosophical Magazine,' Series 6, Vol. xxiv. No. 140, Aug. 1912, page 210.]

GENERAL MONTHLY MEETING,

Monday, April 1, 1912.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

Frank Cyril Tiarks, Esq.
Mrs. Tiarks,
Alfred Wagg, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mrs. Ludwig Mond and family for their Gift of a large Bronze Medallion of the late Dr. Ludwig Mond, F.R.S., the founder of the Davy Faraday Laboratory.

The Honorary Secretary announced the decease of Professor Petr Nikolajewitsch Lebedew, on March 14, 1912, and the following Resolution passed by the Managers at their Meeting held this day was unanimously adopted:—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the Scientific world in the decease of Professor Petr Nikolajewitsch Lebedew, Ph.D. (Strasburg), Professor of Physics in the University of Moscow, and an Honorary Member of the Royal Institution since 1910.

He made numerous valuable contributions to our knowledge of physical problems. Twenty-seven years after Clerk Maxwell had put forward his theory of light pressure, Professor Lebedew described before the Congrès International de Physique in Paris in 1900 an experiment in which he had detected this pressure by directing the beam from an arc lamp on to platinum discs suspended in a very high vacuum, and found a value in accordance with Maxwell's theory. He also conducted important investigations on Electro-Magnetism and Hydrodynamics.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Catalogue of the Imperial Library, Calcutta, Part II. 8vo. 1910.

British Museum, The Trustees—Catalogue of Greek and Roman Jewellery. 4to. 1911.

Catalogue of German Woodcuts, Vol. II. 8vo. 1911.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXI. 1° Semestre, Fasc. 4–5. 8vo. 1912.
Allegheny Observatory—Publications, Vol. II. No. 14. 4to. 1912.
American Geographical Society—Bulletin, Vol. XLIV. No. 2. 8vo. 1912.
Astronomical Society, Royal—Monthly Notices, Vol. LXXII. No. 4. 8vo. 1912.
Backhouse, T. W., Esq., F.R.A.S. (the Author)—Catalogue of 9842 Stars for the Epoch 1900. 4to. 1911.
Baillehache, Comte de (the Author)—Détermination de la force Électromotrice de l'élément Weston Normal. 8vo. 1912. (Journal de Physique, Feb. 1912.)
Belgium, Royal Academy of Sciences—Bulletin, 1912, No. 1. 8vo.
Mémoires in 4to, 2^{e} Serie, Tome III. Fasc. 8. 8vo. 1912.
Birmingham and Midland Institute—Meteorological Observations, 1911. 8vo. 1912.
Bright, C., Esq., F.R.S.E. (the Author)—Inter-Imperial Cable Communication; a Non-Party Question. 4to. 1912.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIX. Nos. 9–10. 4to. 1912.
British Astronomical Association—Journal, Vol. XXII. No. 5. 8vo. 1912.
Cambridge Philosophical Society—Proceedings, Vol. XVI. Part 5. 8vo. 1912.
Carnegie Institution, Washington—Tenth Anniversary. 8vo. 1912.
Annual Report of Department of Terrestrial Magnetism, 1911. 8vo.
Data for Magnetic Disturbances, 1908–9, etc. 8vo. 1911.
Chemical Industry, Society of—Journal, Vol. XXXI. Nos. 5–6. 8vo. 1912.
List of Members, 1912. 8vo.
Chemical Society—Proceedings, Vol. XXVIII. No. 398. 8vo. 1912.
Cracovie, Imperial Academy of Sciences—Bulletin: 1911, Classe de Philologie, Nos. 6–8; 1912, Classe des Sciences, Nos. 1A–1B. 8vo.
De Villamil, Lieut.-Col. R., M.R.I. (the Author)—A.B.C. of Hydrodynamics. 8vo. 1912.
Editors—Agricultural Economist for March, 1912. 8vo.
American Journal of Science for March, 1912. 8vo.
Athenæum for March, 1912. 4to.
Author for March, 1912. 8vo.
Canada, March, 1912. 8vo.
Chemical News for March, 1912. 4to.
Chemist and Druggist for March, 1912. 8vo.
Concrete for March, 1912. 8vo.
Dyer and Calico Printer for March, 1912. 4to.
Electrical Engineering for March, 1912. 4to.
Electrical Industries for March, 1912. 4to.
Electrical Review for March, 1912. 4to.
Electrical Times for March, 1912. 4to.
Electricity for March, 1912. 8vo.
Engineer for March, 1912. fol.
Engineer-in-Charge for March, 1912. 8vo.
Engineering for March, 1912. fol.
Ferro Concrete for March, 1912. 8vo.
Gardeners' Chronicle for Jan.–March, 1912. 8vo.
Horological Journal for March, 1912. 8vo.
Illuminating Engineer for March, 1912. 8vo.
Journal of the British Dental Association for March, 1912. 8vo.
Law Journal for March, 1912. 8vo.
London University Gazette for March, 1912. 4to.
Model Engineer for March, 1912. 8vo.
Motor Car Journal for March, 1912. 8vo.

Editors—continued.
Musical Times for March, 1912. 8vo.
Nature for March, 1912. 4to.
New Church Magazine for April, 1912. 8vo.
Nuovo Cimento for Feb. 1912. 8vo.
Page's Weekly for March, 1912. 8vo.
Science Abstracts for Feb. 1912. 8vo.
Zoophilist for March, 1912. 8vo.
Engineers, Society of—List of Members, etc., 1912. 8vo.
Florence Biblioteca Nazionale—Bulletin for Jan.-March, 1912. 8vo.
Florence, Reale Accademia dei Georgofili—Atti, Serie Quinta, Vol. IX. Disp. 1. 8vo. 1912.
Franklin Institute—Journal, Vol. CLXXIII. No. 3. 8vo. 1912.
Geneva, Société de Physique—Compte Rendu, No. XXVIII. 8vo. 1911.
Geological Society—Quarterly Journal, Vol. LXVIII. Part 1. 8vo. 1912.
Abstracts of Proceedings, Nos. 920-922. 8vo. 1912.
Heidelberg, Academy of Sciences—Sitzungsberichte, 1909-11. 8vo.
Abhandlungen, 1910, Ab. I. 4to. 1910.
Horticultural Society, Royal—Journal, Vol. XXXVII. Part 3. 8vo. 1912.
Johns Hopkins University—Studies, Series XXIX. No. 2. 8vo. 1911.
Literature, Royal Society of—Transactions, Vol. XXXI. Part 1. 8vo. 1912.
London County Council—Gazette for March, 1912. 4to.
Merck, E., Esq.—Prüfung der Chemischen Reagenzien auf Reinheit. 8vo. 1912.
Meteorological Society, Royal—Record, Vol. XXXI. No. 122. 8vo. 1912.
List of Fellows, 1912. 8vo.
Methuen & Co. Ltd., Messrs. (the Publishers)—The Science of Hygiene. By W. C. C. Pakes and A. T. Nankivell. New edition. 8vo. 1912.
Montpellier Académie des Sciences—Bulletin, 1912, Nos. 1-3. 8vo.
National Church League—Gazette for March, 1912. 8vo.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for Feb. 1912. 4to.
Paris, Société Française de Physique—Journal de Physique for Feb. 1912. 8vo.
Pharmaceutical Society of Great Britain—Journal for March, 1912. 8vo.
Photographic Society, Royal—Journal, Vol. LII. No. 3. 8vo. 1912.
Post Office Electrical Engineers—Papers, No. 36. 8vo. 1912.
Royal Colonial Institute—United Empire, Vol. III. No. 3. 8vo. 1912.
Royal Irish Academy—Proceedings: Vol. XXIX. A, No. 5, B, Nos. 7-9; Vol. XXXI. (Clare Island Survey) Nos. 11-13, 23, 63. 8vo. 1912.
Royal Society of Arts—Journal for March, 1912. 8vo.
Royal Society of London—Philosophical Transactions: A, Vol. CXI. Nos. 482-3; B, Vol. CCII. No. 289. 4to. 1912.
Proceedings: A, Vol. LXXXVI. Nos. 586, 587; B, Vol. LXXXIV. No. 575. 8vo. 1912.
Year Book, 1912. 8vo.
St. Petersburg Imperial Academy of Sciences—Bulletin, 1912, Nos. 4-5. 4to.
Bulletin de la Commission Sismique: 1907, Oct.-Dec.; 1908, Jan.-Sept.; Tome III. Liv. II. No. 2—III. Tome IV. Liv. I.-II. 8vo. 1910-11.
Seismometrische Tabeller von F. B. Galitzer. 8vo. 1911.
Sanitary Institute, Royal—Journal, Vol. XXXIII. No. 3. 8vo. 1912.
Selborne Society—Selborne Magazine for April, 1912. 8vo.
South Africa, Union of—Agricultural Journal for Feb. 1912. 8vo.
South African Association for the Advancement of Science—Journal, Vol. VIII. No. 8. 8vo. 1912.
Statistical Society, Royal—Journal, Vol. LXXV. No. 4. 8vo. 1912.
Stonyhurst College Observatory—Results of Meteorological and Magnetical Observations, 1911. 8vo. 1912.

United Service Institution, Royal—Journal for March, 1912. 8vo.
United States Army, Surgeon-General's Office—Index Catalogue of the Library, Second Series, Vol. XVI. 8vo. 1911.
United States Department of Agriculture—Experiment Station Record, Vol. XXV. Nos. 7-8. 8vo. 1911.
United States Patent Office—Official Gazette, Vol. CLXXV. No. 4; Vol. CLXXVI. Nos. 1-3. 8vo. 1912.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1912, Heft 2. 4to.
Vienna Imperial Geological Institute—Jahrbuch, 1911, Band LXI. Heft 3-4. 8vo. 1911.
Zoological Society of London—Proceedings, 1912, Part 1. 8vo.
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WEEKLY EVENING MEETING,

Friday, April 19, 1912,

THE RIGHT HON. THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

ALAN A. CAMPBELL SWINTON, Esq., M.Inst.C.E. M.I.E.E. *M.R.I.*

Electricity Supply: Past, Present and Future.

IT was about the year 600 B.C. that Thales of Miletus discovered that when amber was rubbed with silk it acquired the property of attracting light bodies owing to its electrification, and hence it is from the word *electron*, which is Greek for *amber*, that the term *electricity* has been derived. The Greeks and Romans, however, made no use of electricity.

During the seventeenth and eighteenth centuries of our era much scientific work was done in elucidating electrical phenomena until, in the year 1799, Alexander Volta announced the construction of his first pile, which was to lay the foundations for those scientific applications which have led to electricity supply being the important factor it has become in our modern civilization. For a long time galvanic batteries based in principle on Volta's pile were the most efficient known generators of electric currents, and it was with a battery of this description, consisting of 2000 plates arranged in 200 porcelain troughs, one of which actual troughs is on the lecture table this evening, that Humphry Davy first obtained, in 1809, in this very Royal Institution building, the electric arc light, which was destined at a later date to play so important a part in bringing about a demand for electricity supply as we now know it. After Davy, for many years the chief application of electricity was in connection with telegraphy, and here, until quite recently, galvanic batteries were generally employed to afford the necessary supply, though magneto generators, based on the discoveries of Faraday, to which allusion will be made later, were also used in the telegraphic apparatus of Wheatstone, Henley and others.

So far as this country is concerned, it was not till 1853–1856 that we find really practical applications of the electric arc lamp, it being then employed for the lighting of Dungeness and other lighthouses in conjunction with the Holmes magneto-electric machine. It was on the occasion of a visit to one of these lighthouses that

Faraday remarked to Holmes, "You have made a man of my baby." As will be seen, this "baby" of Faraday has now developed into a whole army of very giants.

Improvements in electric generating machinery continued to be made, together with better arc lamps, till a great impetus was given to the whole science of electric lighting by the invention of the Jablochkoff candle. It was with this device, supplied by the identical Gramme alternating dynamo, which, by the kindness of the Institution of Electrical Engineers, is here this evening, that the Victoria Embankment in London was first lighted on December 13, 1878. From that date onwards small electricity supply undertakings for lighting specific areas with arc lamps, both at Charing Cross Railway Station in London, where the Brush arc lamps were supplied with energy from a station across the river, and in various parts of the country, became fairly common. They were, however, mostly of a temporary nature, and public electricity supply for all purposes, in the modern sense, was still unknown.

It was not, however, unanticipated. The first person to imagine, or at any rate to patent, a public electricity supply to all and sundry was St. George Lane Fox, whose specification is dated the 9th October, 1878. From the claims and the drawings attached to Lane Fox's specification, one of which drawings is shown in Fig. 1, it is clear that he anticipated a general supply of electric current by mains, branches, and sub-branches to numerous lamps, using the earth as return, storage batteries of accumulators being employed to assist the generators at the periods of maximum load, a constant pressure being maintained by a curious regulating device worked by an electrostatic voltmeter, and meters based on the principle of the voltmeter being provided for the purpose of measuring the current taken by the various consumers. It is curious to note, however, in spite of the considerable detail that is gone into in the specification, there is no mention of switches or any means of turning the light on or off.

It was during the year 1878 that a panic occurred amongst the holders of gas shares, the idea having got abroad that electricity was going to supplant gas, and that immediately. A great, but, as it turned out, only temporary depreciation of gas stock resulted.

In 1880 the British Museum was electric lighted by Messrs. Siemens, and the South Kensington Museum on the Brush system, of course, in both cases with arc lamps.

There were also about that time a few private installations. For instance, as early as January 1879 the late Lord Armstrong, at Cragside, had utilized the energy of a waterfall to produce electricity, which was brought over a distance of 1500 yards, along overhead wires, into his library for the production of the arc light. By December 1880 the arc lighting had been abandoned, and forty-five 25-c.p. Swan incandescent lamps substituted—an installation which has continued in permanent use from that date, being added to from time to time. There was also some experimental lighting at Alnwick Castle in December 1880 and in March 1881, and, by the kindness of the Duke of Northumberland, one of the early Swan lamps that was employed on these occasions is shown. The experiments were abandoned owing to a fusing of the wires having set the Castle on fire. Further, by June 1881 Lord Kelvin, as we learn from a letter from him to Sir William Preece, had lighted his house in Glasgow, and by the end of that year he had 106 85-volt 16-c.p. lamps in his house, and soon after twelve more in his laboratory, fifty-two in his class-room, and ten in the Senate Room of the University. These were supplied by means of a dynamo and gas-engine working in conjunction with a Faure storage battery—probably the first instance of the use of accumulators for such a purpose in the country.

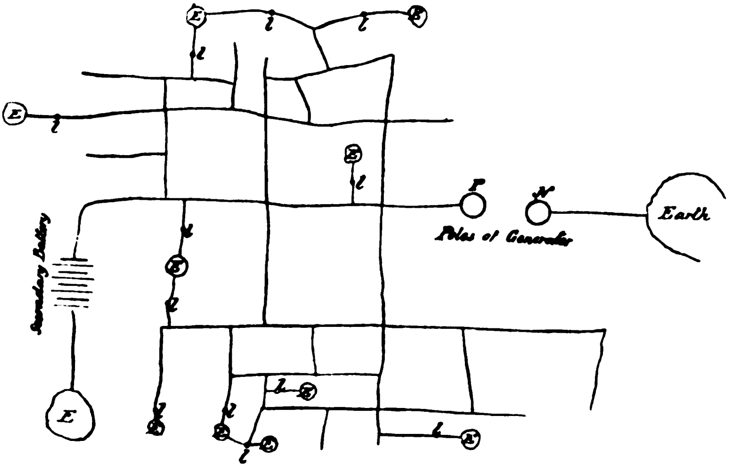

Fig. 1.—Electricity Supply System, from Lane Fox's Patent Specification of 1878.

In 1881 was held, in Paris, the first Electrical Exhibition, and from that exhibition electric lighting, as we now know it, may be said practically to date. Not only was there a large exhibition of the newest and largest plants for generating electricity, together with many and ingenious forms of arc lamps, but there were shown on a large scale, for the first time, the incandescent lamps of Swan, Edison, Lane Fox, Maxim, and others, and also, the first example of an electric tramway, laid down experimentally in the Place de la Concorde by Messrs. Siemens. To many, including the present writer, that exhibition proved an inspiration. Sir William Preece became the apostle, or perhaps rather the major prophet, of the new electrical evangel, while Sir William Crookes, as he recently expressed it, came back from Paris "red hot," and proceeded to light his house at 7 Kensington Park Gardens, not only laying with his own hands the necessary wires under the floor-boards, in glass tubes with plaster-of-Paris joints—wires that are still in use to this day—but even making his own lamps, with the result that by September 1881 the house was lighted up by means of current supplied by a small Bürgen dynamo in the basement, driven by an Otto gas engine.

By the courtesy of Sir William Crookes there are on view on the lecture table specimens of the incandescent lamps that he made at that time, together with other early lamps of Swan, Edison, and Maxim of the same date. Sir William Crookes' house seems undoubtedly to have been the first house in London permanently lighted by electricity. Mr. Spottiswoode also had very early electric lighting at Combe Bank, and Sir Joseph Swan at Bromley; while Mr. Robert Hammond's house, it is claimed, was the first to be electric lighted from cellar to attic, to the complete exclusion of all other illuminants. This was in 1882.

It was also in 1882 that Parliament passed the first of the Electric Lighting Acts which have done so much to hinder the progress of electricity supply in this country. This Act was in part based upon recommendations made by a Select Committee on Lighting by Electricity that sat in 1879, and as an instance of the want of proportion in the ideas that then prevailed it may be mentioned that before that Committee Mr. Joseph Rayner, the Town Clerk of Liverpool, explained that one of the reasons why the Corporation of Liverpool were seeking for Parliamentary powers to supply electricity within their borough was because they were in a specially advantageous position to do this, as they had an engine which was used during the daytime for working a fountain and might well be used for supplying electricity during the night, that engine having a capacity of 20 horse-power. At the end of last year the electric supply plant of the Corporation of Liverpool amounted to about 50,000 horse-power, which, when compared with this 20 horse-power engine, affords a commentary on the parochial character of the ideas in accordance with which the first of the Electric Lighting Acts was framed.

Again, in the year 1882 the first electric supply station for supplying incandescent lamps on a public scale in London was established by the Edison Company on Holborn Viaduct. The Holborn station was equipped with two Edison dynamo machines, one of which is illustrated in Fig. 2, and it is interesting, as giving an inkling of the notions then prevailing, that these machines were described by the then editor of one of our chief engineering papers as "enormous," it being added, evidently as a matter of wonder, that "no less than 1000 full size or 16-candle incandescent electric lamps were maintained constantly in operation from one machine." It may be mentioned that each of these dynamos was driven by a high-pressure

Porter engine of 130 horse-power, which shows that even in 1882 ideas had not progressed very far beyond those already alluded to in connection with Liverpool three years earlier. The design of these early Edison machines, with their multiple-magnet limbs each with its separate winding, is also illustrative of the ignorance then prevailing on electro-magnetic subjects, it being obvious in the light of modern knowledge that the arrangement was altogether inefficient and absurd. It was the late Dr. John Hopkinson who first put the design of continuous-current dynamos and their magnetic circuits on a sure foundation.

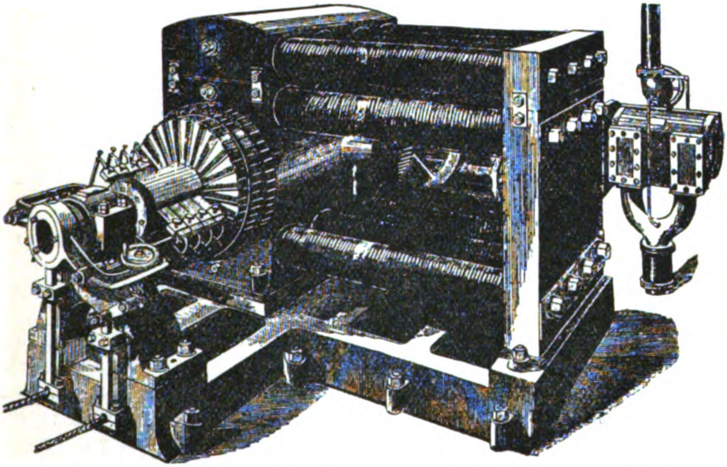

FIG. 2.—EDISON DYNAMO, USED ON HOLBORN VIADUCT IN 1882.

In the same year we find the Gordon alternating dynamos, then considered of very large size, installed to supply electricity for the lighting of Paddington Railway Station and Hotel. It is only quite recently that the use of these machines was discontinued.

In November 1882 we have Sir William Siemens, in his opening address as Chairman of the Society of Arts, discussing in considerable detail the supply of electricity for general purposes in London, and his opinions and figures are really very interesting in the light of how matters have actually turned out. His main difficulty appears to have been that of the economic distribution of the electricity for the general public and private lighting of London with the low-pressure incandescent lamps which were then in vogue, and in the absence of high-tension transmission and of distribution on the three-wire system, which have since become so general and enable much greater distances to be covered with a given loss. As a result, he comes to the conclusion that in the densely populated areas, such as St. James's, there should be a generating station for about every quarter of a square mile, which would mean, taking into account parks, squares, and sparsely inhabited areas, which need not be considered, that there should be about 140 generating stations to supply London. As a matter of fact, London is at present supplied by fourteen companies and thirteen local authorities, and some of these have several stations. There are, however, nothing like 140, and it would be much more economical if the number that there are were reduced, and the whole generation concentrated in two or three stations.

Sir William Siemens is more happy in his estimates of the capital required, which he puts at £14,160,000 for London, and £64,000,000 for Great Britain and Ireland. These figures may be compared with the £20,000,000 for London, and £67,000,000 for the whole country, which are approximately what has actually been expended at the present time.

So far from assisting electricity supply, the Electric Lighting Act of 1882 had the immediate effect of crushing enterprise in that direction, the period of seven years for which licences, or the twenty-one years for which provisional orders were granted to promoters of electric supply undertakings being found quite inadequate to enable money to be raised for such purposes. Between 1883 and 1888, when the Act was amended, only ten licences were applied for, all of which afterwards expired or were revoked, and though in the first year there were a considerable number of applications for provisional orders, not one of these was carried into effect, capitalists refusing to find money for undertakings which had only a tenure of twenty-one years. No doubt, also, this unsatisfactory result was assisted by the severe reaction that had set in from the speculative mania in electric lighting affairs of a few years earlier.

It was not until 1885 that Sir Coutts Lindsay laid down an installation in Bond Street to light the Grosvenor Picture Gallery and the premises of some of the neighbouring tradesmen, which installation in its subsequent development had probably more influence than anything else on the fortunes of electricity supply, not only in London, but in the country generally. Quite a novel system of distribution was employed, the current being alternating and distributed at high pressure by means of overhead wires, and transformers (or secondary generators, as they were called) on the Gaulard and Gibbs system being used to reduce the pressure to suit that of the lamps. One of these actual Gaulard and Gibbs transformers is on the table.

To begin with, the system did not work well, and, on the advice of Lord Kelvin, Mr. S. Z. de Ferranti was called in to assist. The station was immediately reorganized and fitted with machinery of much greater capacity, and so successful was the outlook that, early in 1888, the London Electric Supply Corporation, Limited, was formed with a capital of £1,000,000 sterling, and what were then considered as immense works were started upon as far away as Deptford, six miles from the centre of London, the scheme being to transmit the electricity from where land, coal, labour, and water for condensing could be cheaply obtained, at a pressure of no less than 10,000 volts, with suitable sub-stations where it could be transformed and thence distributed at lower pressures. The great courage shown by those responsible for the venture was deserving of a better fate—but alas for the uncertainty of human endeavours! While the working of the station at Deptford was still in its inception, the plant at the Grosvenor Gallery became ignited by a short circuit and was burnt out; while the London Electric Supply Corporation soon afterwards went into the hands of a receiver, leaving unfinished, and never to be finished, the 10,000 horse-power sets of dynamo and engine which Mr. Ferranti's genius had dared to devise.

Though so very unsuccessful financially at its start, there can be no question as to the enormous influence that the Deptford undertaking had on the history of electricity supply, not only in London or in this country, but throughout the world. Here, at length, was an electricity supply proposition on a scale similar to those of the great undertakings that furnish gas to the Metropolis, with generating plant and means of distribution designed for the sale of electricity over a large portion of London. The more cautious procedure adopted by other concerns which sprang up about the same time and later was no doubt more successful from a business point of view, but the impulse given by this ambitious scheme became manifest from the great competition that was shown for provisional orders for different parts of London, leading to the public inquiry that was held by the Board of Trade immediately after the passing of the amended Electric Lighting Act of 1888, in which the period of twenty-one years, after which the undertaking was subject to purchase without any allowance for goodwill, was extended to forty-two years.

It is worthy of note that the London Electric Supply Corporation has now some time ago successfully emerged from its period of financial distress, while Mr. Ferranti, though, as has been shown, he was one of the pioneers of electricity supply, still remains with us as one of the most vigorous intellects in the electrical industry, and one who, as President of the Institution of Electrical Engineers, is even now dreaming fresh dreams of higher things and lower costs as far as electricity supply is concerned.

During the period with which we have been dealing, so far as the public were then aware, the chief improvements that had been effected in connection with machinery for electricity supply had reference to the dynamos which generated the current, the batteries that stored it, the cables and switches and other apparatus that distributed it and regulated its performances. True, to some extent, special designs of steam engines had been got out to suit the requirements of driving the fast-running dynamos, as, for instance, the well-known Willan's engine. As yet, however, there had been no departure from the reciprocating engine.

Early in the year 1885 the present writer had the privilege, for the first time, of seeing running in the works of Messrs. Clark, Chapman & Parsons, Gateshead-on-Tyne, the first true rotary engine that ever gave useful results. The invention had been patented by Sir Charles Parsons in April 1884, and in the interval this first practical steam turbine had been constructed. Fig. 3 shows this machine, which now has its appropriate abode in the South Kensington Museum in the congenial company of Watt's beam engine and Stephenson's "Rocket."

As will be observed, it is a very small machine directly coupled to a dynamo giving about six electrical horse-power when running at the great speed of 18,000 revolutions per minute, and it is interesting to compare its parts, as for instance its blading, with that of the very large steam turbines on exactly the same principle that have been constructed in recent years, as, for instance, portions of blading such as is used in the turbines of the "Mauretania."

As will be seen later, the steam turbine has now come into very general use, being employed to the almost complete exclusion of other heat engines where very large electrical powers are wanted. At its inception, however, its inventor had many difficulties to encounter, together with much prejudice. Since the days of James Watt inventors up to that time had been continually trying to produce a successful rotary engine, and all had failed. It was natural, therefore, for engineers to ask why this new inventor should succeed any more than those who had gone before. They did not realize that the advances that had been made in thermo-dynamics, and more especially in machine tools and workshop methods, had rendered things practicable which, up till that time, had not been so; nor did they understand that here at last the subject was being tackled on really scientific principles by one exceptionally endowed by nature to grapple with it. Another difficulty that Sir Charles Parsons had to contend with was that, in the nature of things, experiments must usually be conducted, in the first instance, on a small scale. Moreover, at that period, when the steam turbine was only employed for driving dynamos, there was no demand for machines of any but what at the present day would be considered of very small size. Now it is one of the peculiarities of steam turbines that they are much easier to make in large sizes than in small sizes to give reasonable economy. Thus it was by reason of the very small powers that were wanted that in these earlier days turbines earned the opprobrious epithet of "steam-eaters."

The Parsons steam turbine was first chiefly employed for the elec-

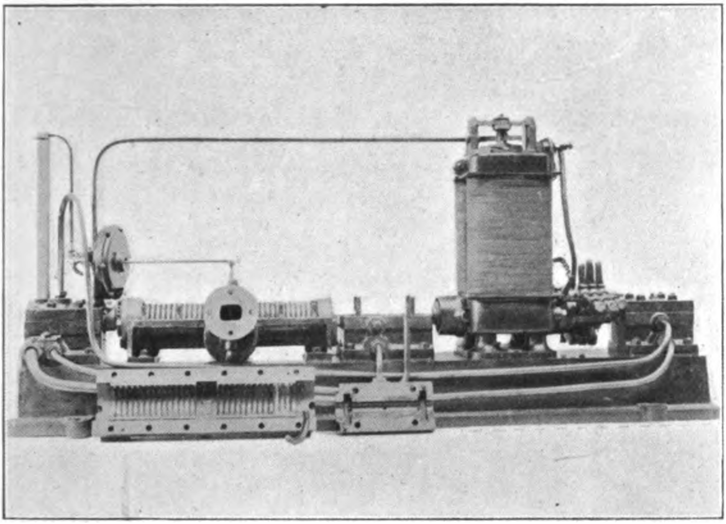

FIG. 3.—THE ORIGINAL 6 H.P. PARSONS STEAM TURBINE OF 1884.

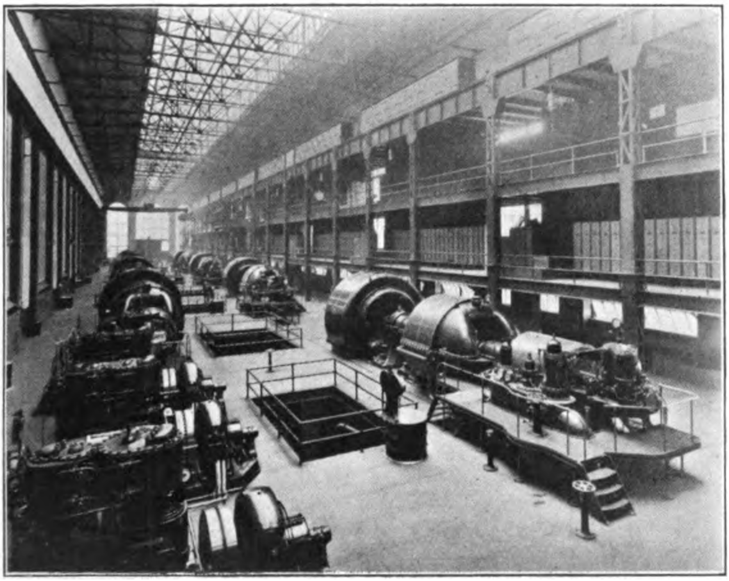

FIG. 4.—METROPOLITAN DISTRICT AND LONDON ELECTRIC RAILWAYS, 1905. CHELSEA POWER STATION: 75,000 H.P. OF STEAM TURBINES.

tric lighting of ships, but in 1887 the whole of the electricity for lighting the Mining, Engineering, and Industrial Exhibition that was held in Newcastle-upon-Tyne was generated by a number of Parsons machines; while a little later the Newcastle and District Electric Lighting Company was formed, whose works on the banks of the Tyne were the first in which steam turbines were employed to afford a public supply of electricity for general lighting and other purposes. The first machines employed in this station were only of about 100 horse-power, while others of an improved type, which were first employed at Cambridge and at Scarborough, were of about double this power, and were considered as very large. The first steam turbines to be employed in London were used for the lighting of Lincoln's Inn Hall, where they worked for many years, to be followed not long after, in 1891, by three others, each of 50 horse-power, at New Scotland Yard, which still exist, and to-day are providing electricity for lighting, printing, and other purposes, for the Metropolitan Police.

Fig. 4 shows a more recent steam turbine installation in London, being the interior of the engine-room at the Chelsea Power Station, where 75,000 horse-power of Parsons turbines supply power to the Metropolitan District and other Railways.

Some fifteen years ago, in evidence that he gave before the Judicial Committee of the Privy Council in connection with Sir Charles Parsons' application for a prolongation of his patent, Lord Kelvin characterized the Parsons turbine as the most important development in steam engines since the days of James Watt. At the time this seemed a somewhat bold assertion, but in the light of experience it has proved to be a fact.

Just as it was on the banks of the Tyne that the steam turbine was first applied to the public supply of electricity, so it has also been on the banks of the Tyne, and in the adjacent areas of Northumberland and Durham, that the greatest existing development in this country of electricity supply for industrial purposes has taken place. Not the least of the causes that have led to this is the fact that, apart from London, where the circumstances are very special, in Newcastle-upon-Tyne, alone among the great manufacturing cities of Great Britain, has electricity supply remained in the hands of private enterprize, and not become municipalized. The results have been very remarkable, as is clear from the following illustration kindly supplied by Mr. Charles Merz, the distinguished engineer, with whose name this great work will ever be associated.

Fig. 5 is a map of the district covered by this vast power-supply undertaking, which extends as far north as Morpeth, as far west as Consett, is bounded on the east by the sea, and extends right away down through the county of Durham to Stockton-on-Tees, Middlesborough, and Cleveland. There are seventeen generating stations, of which six are coal-fired stations, and the remainder most interesting waste-heat stations, where steam for making the electricity is obtained

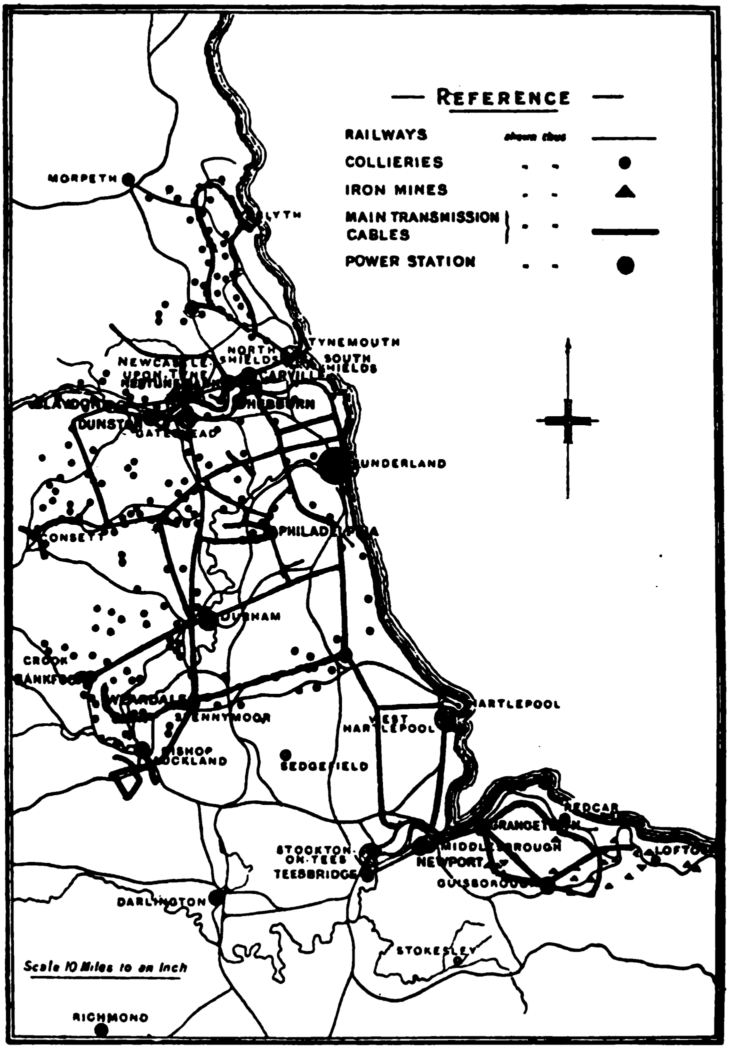

FIG. 5.—NORTH-EAST COAST POWER SYSTEM.

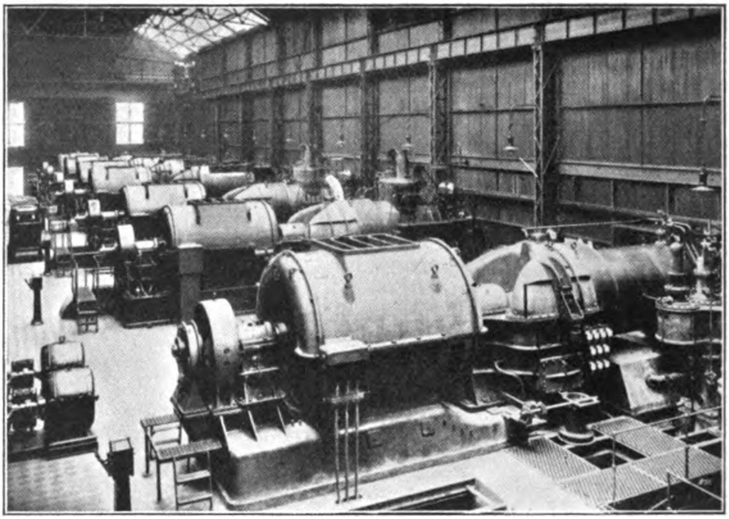

Fig. 6.—Carville Power Station: 52,000 H.P. of Turbo-Alternators.

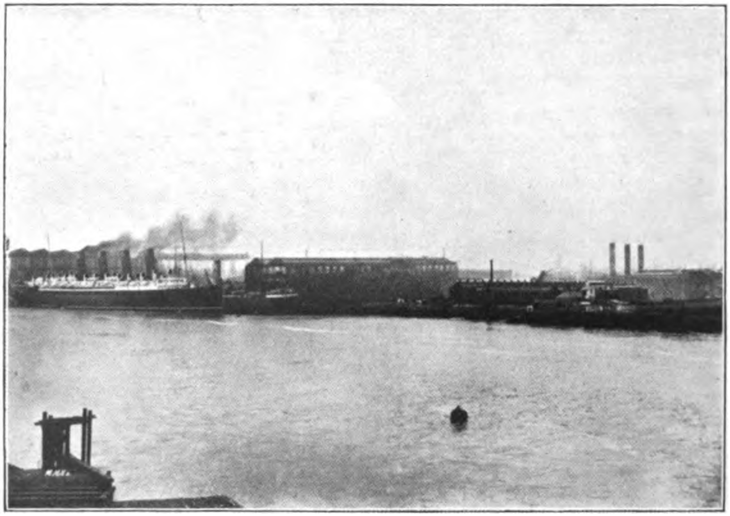

Fig. 7.—River Tyne, showing s.s. "Mauretania," 70,000 H.P., and Carville Power Station, 52,000 H.P.

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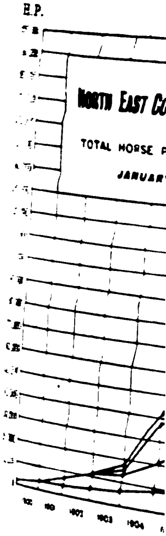

Fig. 6 gives the interior
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either from exhaust steam that has already done work in blowing or other engines, or by steam raised by blast-furnace gas, or from the waste heat and gas from coke ovens.

Excepting in the old original power station at Neptune Bank, where power supply was inaugurated by Lord Kelvin in June 1901, and where there are still some reciprocating engines, the whole of the works are equipped with alternators driven by steam turbines, mostly of the Parsons type, supplying 3-phase 40-cycle current at voltages varying from 3000 to 12,000. The power is supplied to all the leading manufacturers for every kind of purpose, and also to the railway from Newcastle to the sea, which has been electrified.

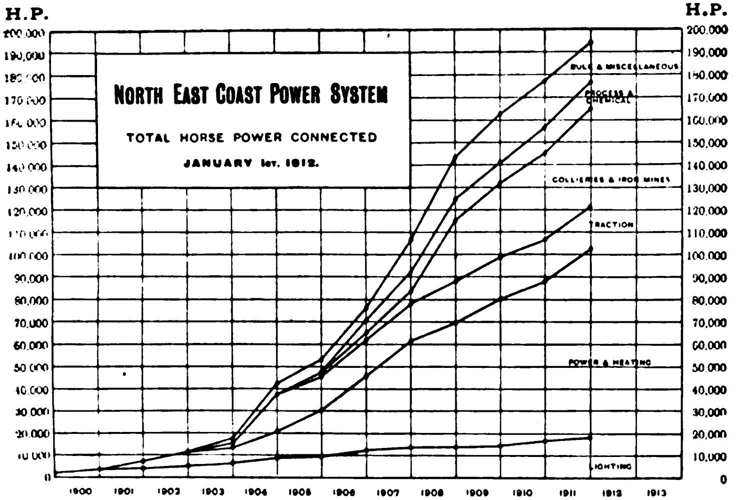

Fig. 8.

Fig. 6 gives the interior of the Company's principal power station at Carville, where 52,000 horse-power of turbine plant is installed, and Fig. 7 an exterior view of the works with the s.s. "Mauretania" lying at an adjacent quay.

Fig. 8 shows that altogether the total horse-power connected amounts to nearly 200,000, and clearly indicates how this large amount is split up amongst the various industries that are supplied.

It is obvious that an undertaking of these vast dimensions, covering as it does large portions of two counties, and several large towns and industrial centres, could never have been undertaken by a single municipality. It is equally obvious that it could never have succeeded as well as it has had Newcastle itself, which has been from the first the nucleus of the undertaking, been cut out of the area of supply.

This explains why other electric power schemes, such as those being worked on the Clyde and in the area round Glasgow, in Yorkshire and in Lancashire, have failed to go ahead anything like as rapidly as the one on the North-East Coast. Parliament in its wisdom, at the instance of municipal parochialism, cut nearly all the large towns out of the areas supplied by these schemes, with the result that progress has been impeded with real benefit to no one.

Fig. 9 shows, in the form of a curve, the growth of the total capital expended on electricity supply undertakings in the United Kingdom from 1881 up to the end of last year. The authority for the figures upon which the curve is based, during the first fifteen years, is Mr. Robert Hammond, while the remainder are from Mr. Garcke's well known handbook of "Electrical Undertakings." The curve is interesting as showing how practically nothing was done in the way of electricity supply after the passing of the 1882 Act until this Act was amended in 1888. Of course, there was some slight expenditure, but it was of such small amount that it cannot be shown in a diagram on this scale, seeing that it did not exceed £100,000 until 1888, and £500,000 till 1890. A better illustration could scarcely be given of how the Act of 1882 forcibly arrested all progress in electricity supply for six years, with the result that

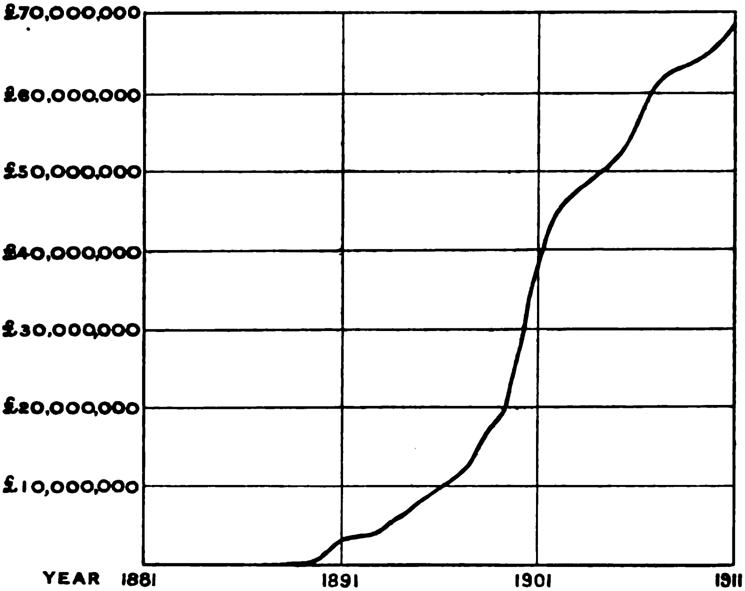

FIG. 9.—CURVE SHOWING CAPITAL EXPENDED ON ELECTRICITY SUPPLY IN THE UNITED KINGDOM FROM 1881 TO 1911.

though at the start this country was fully as far advanced as any other, it got left behind owing to this direful legislative effort which Parliament passed with the laudable intention of giving the country electricity supply on the most economical terms, but which resulted, as we have seen, for a considerable time at all events, in the country obtaining no supply worth speaking of at all.

The factors which lead to the best economy in the generation of electricity in power stations are numerous. First of all, we have such local conditions as cheap fuel (which is, of course, a matter of paramount importance), cheap labour, cheap land, and, if steam is to be used as a source of power, an ample provision of cheap water for condensing. Then there is the question of the nature of the load, the "load factor," as it is termed among engineers, and this is a matter of the greatest importance, and one the influence of which was not realized at all in early days. The load factor may be defined as the ratio between the number of Board of Trade units actually supplied from a generating station during the year, to the number of Board of Trade units that would have been supplied supposing the consumption had, at the maximum rate demanded at any one moment, extended over the whole of the 8,760 hours of which the year is composed.

In the case of companies supplying electric energy chiefly for lighting, the consumption in winter falls to a minimum during the early hours of the morning; rises for a short time to some extent while the population is getting up and having its breakfast; falls again during the day-time to the small amount required for lighting cellars, basements and other dark places; begins to rise again in the afternoon and evening at hours that vary according to the period of the year; reaches a maximum during those portions of the year when it is dark at six o'clock, at the moment when shop lighting is still continuing, and gradually dies away again as the shops are shut, and later as the population goes to bed. In a case like this of stations supplying chiefly lighting, the load factor varies from 16 to 20 per cent. in London, down to as low as 6 to 10 per cent. in the provinces, in places where the shops shut and the people go to bed early.

On the other hand, the load diagrams of concerns which supply a great deal of energy for traction, power, and other purposes, give load factors as high as 25 per cent.; while the figures of the great power companies with stations supplying electricity for railway working furnish still higher load factors again.

Now, the reasons for the importance of the load factor are several. First of all, it is obvious that if you spend money on a generating plant and only use this plant one or two hours out of the twenty-four, or to the extent, say, of only 10 per cent. of its available output, you are only earning a small percentage of the interest that that plant would be producing were it working at full power all the time, that is to say, on a 100 per cent. load factor. Further, these sudden variations in load and the short periods for which the full load is in operation necessarily adversely affect economy both as regards fuel and as regards labour. Steam has to be got up to provide for the maximum load, and during the period of minimum load either the fires have to be banked, or steam has to be let down and then got up again, with consequent inefficiency. Similarly, under the conditions of small load factors it is very difficult to secure economy as regards labour.

It is this question of load factor which is the explanation of what most people fail to realize the reason for, namely, the differences in prices that the consumer has to pay for electricity for lighting and electricity for heating and power purposes. When the machinery has once been provided to deal with the maximum load which the lighting requirements entail, supply undertakings can afford to provide electricity for use during the hours of daylight, when electricity for lighting is not wanted, at a very cheap rate, for the plant and the attendance is there, and it only means a small extra consumption in fuel; it is, indeed, expressly for the purpose of improving the load during the day, and consequently the load factor, that these differences in prices are quoted.

There is another very important factor controlling the cost of electricity to which full attention has only quite recently been paid, that is, the scale on which the electricity is being generated. Only a few years ago this question of scale was not much thought of; indeed, in evidence given on behalf of some of the earlier Power Bills before Parliamentary Committees, eminent engineers gave it as their opinion that there were comparatively low limits beyond which the scale of the operations would make no difference. Their idea was that the largest engines and dynamos then existing, giving some two or three thousand horse-power apiece, were as large as was advisable, and that no great advantage, either as regards first cost or as regards economy in fuel or operating, would be obtained by increasing the size of these units, and that it was simply a question of multiplying their number. No doubt, with the reciprocating engine there was something in this point of view, but, anyway, the advent of the steam turbine has entirely altered matters, as with these machines there seems to be practically no limit to the extent whereby greater economy is obtained by a larger size, and this not only as regards steam consumption and working costs, but also as regards capital expenditure.

And here notice may be drawn to the latest thing in the way of steam turbine plants, and it is interesting to compare it with the original Parsons turbine and dynamo of six horse-power, capable of giving about 4½ kilowatts of electric energy. The new machine, which is illustrated in Fig. 10, is capable of giving a maximum continuous output of 25,000 kilowatts at unity power factor, 25 cycles, 4500 volts, this corresponding to over 33,000 electrical horse-power. This machine has recently been ordered from Messrs. Parsons by the

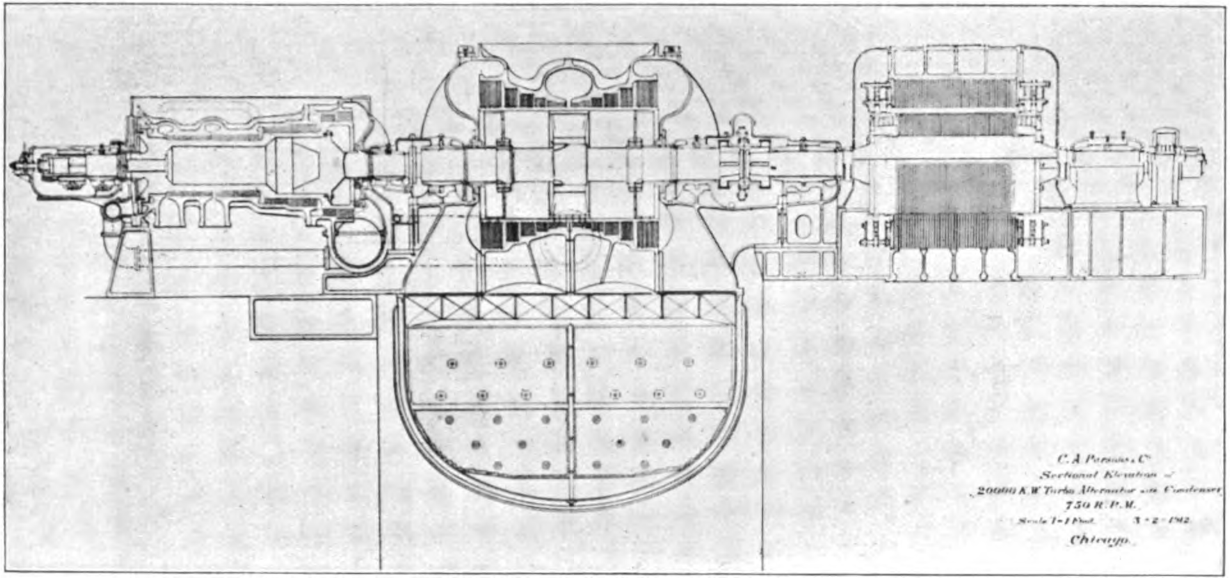

FIG. 10.—25,000 K.W. PARSONS TURBO-ALTERNATOR FOR CHICAGO.

Commonwealth Electric Co., of Chicago, in America, for their Fisk Street Station, the order having come to this country mainly by reason of the superior economy guaranteed, the figure that it is confidently expected will be obtained being something like ten per cent. lower than anything that has previously been obtained with steam plant. This ten per cent. economy in steam, with a plant of these dimensions, means a saving of about two tons of coal per hour when working at full load.

As will be observed from the sectional drawing, the turbine is divided into two. The steam first passes from one end to the other through a high-pressure part, where the blades are shown as made of copper, which is better suited to stand super-heat than the usual bronze. Next, the steam is carried to the centre of a low-pressure turbine, where, just as was the case in the original Parsons machine, it divides, and passing partly to the right and partly to the left, finally reaches the surface condenser, which has a cooling surface of about 39,000 square feet, and is made part of the machine so as to avoid all losses in connecting pipes. The speed of the machine will be 750 revolutions per minute, and the steam pressure employed 200 lbs.per square inch with 200° F. normal superheat.

In electricity supply the next most important thing after economical generation is obviously economical transmission and distribution, and in relation to these latter considerable advance has been made in recent years, more especially in the direction of employing very high voltages, which enable great distances to be covered with comparatively little loss. There used to be great prejudice in this country against the use of overhead wires, but this is disappearing, particularly in industrial districts where the æsthetic point of view does not count.

In America and in Canada, notably by the Ontario Power Company, pressures as high as 100,000 volts are in use on overhead lines, and similar voltages are at present being adopted in Germany in one instance. In the ordinary way consumers are all connected up in parallel, exactly as was shown in the diagram attached to Lane Fox's early specification, but, of course, using a metallic and not an earth return. A very interesting method in which the consumers are all joined in series, instead of in parallel, has, however, been worked out in Switzerland by Mr. Thury, and is largely adopted in Geneva. It has now been applied by Mr. Highfield for the Metropolitan Company in the outskirts of London.

In the latter case there are seven miles of cable constructed to carry 100 amperes and up to 120,000 volts continuous current; the sub-stations, where there are motor generators, taking a voltage dependent upon the power required, being placed all in series. The ultimate capacity of the circuit is 10,000 kilowatts running at 100,000 volts and 100 amperes. At present only 1500 kilowatts are being transmitted with 15,000 volts. The voltage will be raised from time to time as more power is required at each sub-station, and as further sub-stations are inserted, all the sub-stations being in series.

A very interesting feature is that the earth is being used as a return. The earth connections are made 40 ft. down, and it is to this considerable depth that must be attributed the fact that no electrolysis or other inconvenience results.

The chief advantage of the system is that, owing to its being continuous current, it enables higher voltages to be used on underground cables than would otherwise be practicable.

As to the future, it is obvious from the lessons of the past how very dangerous it is to prophesy, it being frequently the entirely unexpected that turns up. So far as the immediate future of electricity generation on a large scale is concerned, the steam turbine appears likely to hold the field, though in regard to the smaller stations, where units up to 500 or 1000 kilowatts are what are wanted, the internal combustion engine is undoubtedly gaining ground. Will it, however, ever catch up the steam turbine in the case of the really large power stations? As has been shown, in these, units up to 25,000 kilowatts are now in actual use or in contemplation, and as electricity becomes more and more employed, not only for power but for electro-chemical and metallurgical purposes and for domestic heating, units of plant of still larger dimensions may be expected. At present about 2000 horse-power, or about 1500 kilowatts, seems to be about the maximum that is considered can be safely obtained per cylinder from the internal combustion engine, so that increased powers can only be obtained by a process of multiplication, which leads, in the case of very large units of plant, to great complication. Then again, as the steam turbine, particularly with the employment of superheated steam, tends to increase in fuel economy as the dimensions of the unit of plant is increased at a much greater ratio than does the internal combustion engine, a point must be reached when, as the units of plant are enlarged, taking all things, such as first cost, lubricating oil, attendance and upkeep, into account, the steam turbine will be as cheap or even cheaper than its rival.

So far, the internal combustion or gas turbine has not been alluded to, but some of the difficulties in the way of its successful realization may be mentioned. All turbines essentially consist of machines by means of which power is obtained by the passage of fluids or hot gases through narrow apertures, and by their impingement on blades, in such a manner that the fluids or gases are in intimate contact with large surfaces of metal. Now, as all engineers are aware, the law which limits the efficiency obtainable in any heat engine is expressed by the formula $(T' - T) \div T'$, where T' is the absolute temperature of the working gas, and T the absolute temperature of the condenser or the exhaust. From this it is clear that if we are to get maximum efficiencies, the temperature of the working fluid must be as high as possible, and the sole reason for the extra good efficiency of an internal combustion engine is because in this machine the temperatures that can be successfully dealt with are very high. In the cylinder the combustion takes place when the gas is in considerable mass, and though those portions of it which are in contact with the walls of the cylinder become cooled, still, the interior of the mass keeps very hot, indeed, at temperatures which could not possibly be employed in turbines, unless we could find the materials of which to construct the blades which would maintain their tenacity while running at a red heat. It is conceivable that the science of metallurgy may be able to provide new metals or alloys with the necessary properties for doing this in the future, but at present no such material exists, and the only way in which the internal combustion turbine can for the moment be worked is by reducing the temperatures of the gases by the introduction of water, steam, or air, to a reasonable amount; indeed, in practice the temperature has to be reduced to that usual with superheated steam, when of course, according to the formula quoted, the maximum efficiencies theoretically obtainable with the internal combustion engine and the steam turbine become equal. Even then, if other things were equal, the internal combustion turbine might have some advantage by doing away with boilers; but, unfortunately, there are other difficulties—such as the bad economy of all methods of compressing the gaseous mixture as is necessary to obtain the full advantage of its combustion.

No doubt the future of electricity supply lies with very large stations employing very big units of plant, and combining the generation of electricity with chemical manufacture, the electricity on the one hand, and the chemicals on the other, being by-products each of the other's manufacture. So far as this country is concerned, for electricity supply at all events, we are not likely to depart from the use of coal as long as that source of energy holds out. For the propulsion of ships oil may present advantages, but on land, in Great Britain, coal must remain the cheaper. In all probability, however, in the future the coal will not be simply burnt. Undoubtedly it will have to be turned into gas, and the sulphate of ammonia and the tar, with all its interesting constituents, saved. Whether the gas will be burnt under boilers for the raising of steam to supply steam turbines, or whether it will be used in internal combustion engines, will depend on the progress made by the latter in respect to attaining larger dimensions, and also as regards improvements in the gas firing of boilers, in respect of which, as has recently been shown at the Royal Institution by Professor Bone in his interesting lecture on Surface Combustion, there is still much to be done.

Anyway, future generating stations are almost certain to be established—not in the large towns, but at the coalfields, and, instead of the coal, the electricity will be transmitted at high pressure to where it is required. Sir William Ramsay's somewhat revolutionary scheme for the manufacture of gas by ignition of the coal in the seam itself is at any rate very interesting. Obviously such a scheme, if it could be practicably worked, would have enormous advantages in many ways, and in one, perhaps not the least, that it might enable seams to be worked which, owing to their thinness, are now quite unprofitable. On the other hand, no saving of residual or by-products would appear feasible, unless, indeed, from the washing of the gas; and it appears very problematic what area of coalfield could be tapped from a single borehole without stoppage resulting from the falling in of the roof when no longer supported by the coal. At the same time, when working near the boundaries of a property, it would be difficult to know whose coal was being gassified. Anyway, the scheme is apparently to be given a practical trial, and this certainly seems desirable.

There are still nineteen years to elapse before the purchase of the London electric companies can take place under the Act of 1888, and nineteen years is a long time, having regard to the present rate of progress in electrical matters. If a suggestion may be hazarded, it is that when the time comes it will probably be best for the London County Council, or whoever the ultimate purchasing authority may be, to confine themselves to the distribution of electricity, which by that time they will no doubt be able to purchase very cheaply wholesale from others, and not to embark directly upon electricity manufacture, which by then will in all probability be carried on at the coalfields on an enormous scale alongside of chemical manufacture, metallurgy, and other processes of which we now know but little.

When the coal and oil and also the peat are exhausted, what then? The date may be distant, but come it must, and that within a period short in comparison with our past civilization.

The water power existing on the earth, when all harnessed, would only supply a very small percentage of the demand for power, light and heat. The utilization of the tides does not appear a very hopeful project, any more than does the utilization of the internal heat of the earth. There remain the energy dependent on atomic transformation, the availability of which the highest authorities appear to regard as probably impracticable, and the radiant energy that reaches this planet from the sun. The latter, as calculated by Sir J. J. Thomson, amounts on a clear day to no less than 7000 horse-power per acre, or about 4,500,000 horse-power per square mile of the earth's surface.

Here is obviously an ample supply of energy sufficient for all purposes provided it can be converted into work by some reasonably efficient process. This should not prove impossible, and we have therefore here a problem for the physicist of the utmost importance to the race.

In conclusion, it may be pointed out that all the many interesting developments which have been discussed are based upon discoveries made within the walls of the Royal Institution, with the identical pieces of historical apparatus which are lying on the lecture table. With these simple coils of wire and these small magnets, in this very building, Faraday, in 1831, discovered the induction of electric currents and their generation by the motion of conductors in magnetic fields. The working of all dynamo-electric generating plant is based upon these discoveries. Thus we see how, from small beginnings, great matters eventuate, and that scientific discoveries, which, at the time of their origin, may have appeared of but academic interest, can have stupendous influence on the resources and the history of mankind.

[A. A. C. S.]

WEEKLY EVENING MEETING,

Friday, April 26, 1912.

The Right Hon. The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Sir George H. Darwin, K.C.B. M.A. (Cantab.) LL.D. D.Sc. F.R.S. *M.R.I.*

Sir William Herschel.

During the last twenty years there has been a great revival of statistical investigations as to the distribution and motions of the so-called fixed stars. Kapteyn of Groningen is the leader of those who are renewing the attempt to obtain in this way some idea as to the construction of the Universe. Earlier astronomers had of course done something in this direction, but the work of William Herschel so far transcends that of all others, that it would be fair to describe him as the originator of this class of investigation. It may be of interest to mention that a complete edition of his works is now in course of publication, under the direction of a joint committee of the Royal and Astronomical Societies.

The interest of Herschel's writings, and the simple charm of his style —written it is to be remembered in a language which was not his from birth—have led me on to read about the man as well as about his scientific work. Throughout his life's work his name is inseparable from that of his sister Caroline, and I hope it may prove of interest to you to hear of what they were, as well as of what they did. They were born at Hanover, he in 1738, she in 1750, the children of a bandsman of the Hanoverian Guards. At the age of fifteen Herschel was already a member of the Guards' band. In 1757 the regiment, which had been in England for about a year, served in Germany during the Seven Years' War, and William seems to have suffered from the hardships of the campaign. His parents, seeing that he had not the strength for a soldier's life, determined to remove him from the regiment. The removal may be described more bluntly as desertion, for we learn that when he had passed the last sentinel at Herrenhausen, he took off his uniform and his luggage was secretly sent after him to Hamburg. At any rate, fortunately for science, he escaped, and in 1757 or 1758 made his way to England.

It would perhaps be impossible to follow him throughout his wanderings, but we know that he was at one time instructor of the band of the Durham Militia, and afterwards that he gained his living as a musician in Leeds, Halifax, Pontefract, and Doncaster. In 1764 he even ventured back to Hanover for a short time, and thus saw his favourite sister again.

During her early years Caroline seems to have been practically the household drudge or general servant, and whatever she learnt was by stealth or in the scanty intervals snatched from her household duties, for her mother thoroughly disapproved of education for a girl.

When we reflect on the difficulties under which both brother and sister laboured, and then consider how much they were able to accomplish, we might be tempted to under-rate the value of educational advantages. Concerning education, Bishop Creighton once said in my hearing, "It is surprising how little harm we do notwithstanding all the pains we take." Paraphrasing the remark, although spoiling the epigram, I would say, "It is surprising how little harm the lack of opportunity does to a great genius."

In 1766 William took a position as organist at Bath, then at the height of fashion. The orchestra at the Pump Rooms and at the theatre at Bath was then one of the best in the kingdom, and Elizabeth Linley, daughter of the director of the orchestra, was the prima donna of the concerts. When in 1771 she became engaged to Charles Sheridan, Herschel thought that the expected vacancy would make an opening for his sister at Bath, and suggested that she should join him. And, in fact, after a time such a vacancy did occur, for Elizabeth Linley, after flirting with Charles Sheridan, jilted him and eloped with and married the celebrated Richard Brinsley Sheridan.

Caroline was very anxious to accede to her brother's suggestion, but the rest of the family would not for a time hear of it. At length, however, in 1772, Herschel came to Hanover and carried off his sister with the mother's reluctant consent. Even from boyhood his intense love of astronomy had been manifest, and it is interesting to note that in passing through London on their way from Harwich to Bath, when they went out to see the town, the only sights which attracted their attention were the opticians' shops.

On Mr. Linley's retirement from the orchestra at Bath, Herschel became the director and the leading music-master in the town, and he thus obtained an established position. Although Caroline sang a little in public, her aspiration to become the prima donna of Bath was not fulfilled. But she was kept busy enough at first in the cares of housekeeping, with endless wrangling with a succession of incompetent slaveys, and then she gradually became more and more her brother's astronomical assistant.

In the midst of Herschel's busy musical life he devoted every spare moment to astronomy, and when his negotiations for the purchase of a small reflecting telescope failed—and they were all small in those days—he set to work to make mirrors for himself.

One room in the house was kept tidy for pupils, and the rest of the house, including the bedrooms, was a litter of lathes and polishing apparatus. He made reflecting telescopes not only for his own use, but also for sale, for the purpose of providing funds to enable him to continue his researches. His industry must have been superhuman, for later in his life he records that he had made over 400 mirrors for Newtonian telescopes, besides others of the Gregorian type. These mirrors ranged in diameter from a few inches to 4 feet, in the case of the great 40-foot telescope. I should say that mirrors are not specified by the diameter of the reflecting surface, but by the focal length. Thus, whatever may be the diameter of the reflecting surface, a 20-foot telescope means that the mirror is approximately portion of a sphere of 40 feet in radius, and this will give a focal length of 20 feet. You must, in fact, double the focal length of a telescope to find the radius of the sphere of which it forms a small part.

In order to learn anything of the making of reflectors it is necessary to go to original memoirs* on the subject, and even of them there are not many. I feel, therefore, that I shall not be speaking on a topic known to many of the audience if I make a digression on a singularly fascinating art. Mirrors are now made of glass with a reflecting surface of chemically deposited silver ; formerly they were made of speculum metal, an alloy of copper and tin. Of whatever substance the mirror is made the process of working it to the required form is much the same. The most complete account of the process of which I know is contained in a paper by Professor G. W. Ritchey in Vol. xxxiv. (1904) of the Smithsonian Contributions to Knowledge. He there gives a full description of the great reflector of the Yerkes Observatory. The process only differs from that employed by Herschel in that he worked by hand, whereas machinery is now required to manipulate the heavy weight of the tools. The Yerkes mirror is formed of a glass disk 5 feet in diameter, and it weighs a ton ; the grinding tools are also very heavy.

I must pass over the preliminary operations whereby the rough disk of St. Gobain glass was reduced to a true cylindrical form, smooth on both faces and round at the edge. Nor will I describe the grinding of a shallow depression on one of the faces by means of a leaden tool and course emery powder.

It will be well to begin by an account of the manufacture of the tools wherewith the finer grinding and polishing is effected, and then I shall pass on to a short description of the way they are used.

Two blocks of iron are cast with the desired radius of curvature, the one being concave and the other convex. The castings are then turned so that the concavity and convexity fit together as nearly as may be. For the large mirror these blocks are a little over 2 feet 6 inches in diameter, but for small ones they are made of the same diameter as the mirror to be ground. The two are then ground together for a long time with emery powder and water until every part of one surface fits truly to every part of the other. They must then both be portions of a sphere of the same radius, because the sphere is the only surface in which a universal fit is possible. The concave iron is very precious because it furnishes the standard for regrinding the convex grinding tools when they have become worn by use. In order to make a plane mirror, three surfaces are ground two and two, for if A fits B and C, and B fits C all over each surface they must all be true planes. However, I shall only speak of the figuring of concave mirrors.

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* Sir Howard Grubb's lecture at the R.I. in 1887 is one of these, vol. xi. p. 413. Lord Rosse's papers are amongst the most important.

The roughly hollowed glass disk is now laid on several layers of Brussels carpet centrally on a massive horizontal turn-table. The convex iron tool just described is suspended by a universal joint from a lever, and it is counterpoised so that only a portion of the weight of the tool will rest on the glass when it is in use. A complicated system of cranks and levers is so arranged that the tool can be driven by machinery to describe loops or curves of any arbitrarily chosen size over the glass, and as these loops are described by the tool the turn-table turns round slowly. In this way every part of the tool is brought into contact with every part of the glass disk in a systematic way. When working near the edge a large part of the tool projects beyond the edge of the glass.

Emery powder and water are supplied in a way I need not describe, and the tool is lowered gently on to the glass. The motive power is then applied, and the grinding is continued for many hours until the preliminary rough depression has been hollowed to nearly the desired shape—namely, that of the standard concave iron.

For finer grinding a change of procedure is now adopted, and very finely powdered emery is used. Another convex tool is formed, by grinding with the standard concavity; the working face of the tool is, however, now cut up into small squares by a criss-cross of narrow and shallow channels. Such channels are found to be necessary in order to secure an even distribution of the emery and water all over the surface. The grooved tool is now used for many hours, and the surface is tested at frequent intervals with a spherometer. The work ceases when it is no longer possible to detect errors of curvature in this way.

The next stage is polishing. The thickness of the layer of glass worn off in polishing is to be estimated in ten-thousandths of an inch, and can scarcely be detected even with the finest spherometer. For polishing the iron tool is discarded and the work is carried on by hand. As lightness is essential, the tool is built up by a stiff latticework of wood with a continuous wooden working face. It is obvious that however carefully the face may be turned it cannot be made sufficiently true, and the requisite accuracy is obtained by means of the plastic properties of rosin or pitch. A number of squares of rosin about a quarter of an inch thick and an inch square are made, and these are glued in rows on the convex face of the wooden tool, with a narrow space intervening between each rosin square and its neighbours. The tool is then warmed slightly so as to soften the rosin a little, and it is then pressed lightly on to the glass disk. By means of this "warm-pressing" a nearly perfect fit is attained.

Each of the rosin squares is then painted with hot melted wax. This is done because wax is harder than rosin and affords a better working face. Finally, when the tool is quite cold, the surface of the glass is painted all over with very finely powdered rouge and water, and the tool is placed gently on the glass with some additional weight resting on it. It is left thus for several hours, but is moved slightly every ten minutes to ensure an even distribution of the rouge and water. By means of this "cold-pressing" a perfect fit is secured of the wax-coated rosin squares with the glass face. Cold-pressing has to be repeated every day before the work begins.

The polishing is now carried on in much the same way as the grinding, but by hand instead of by machine power. The turn-table can be made to tilt so as to bring the glass to stand vertically, instead of horizontally, and the disk is frequently tilted up so as to submit the surface to optical tests. These latter tests are far more searching than those with a spherometer, and enable the observer to detect an error in the radius of curvature of portion of the reflector of a hundredth of an inch. To correct such an error it will be necessary to remove a layer of glass of $\frac{1}{500000}$ths of an inch!

The most refined optical test is by the observation of the image of a brilliant light issuing from a pin-hole close to the intended centre of the spherical surface. The observer examines the image of the pin-hole with a microscopic eye-piece placed as close as possible to the pin-hole. He then causes a straight-edge close in front of the eye-piece to move slowly across the reflected beam of light, either from left to right or from right to left, so as to eclipse the light. Previously to the eclipse the whole of the glass seems to be a uniform blaze of light, and if the curvature is perfect the light which enters the observer's eye comes from all parts of the disk, and the surface is seen to darken equably all over. But if the surface is imperfect the light from some part is eclipsed sooner than that from others, and the disk seems to possess considerable hills and valleys illuminated, as it were, by a setting sun.

The interpretation of these apparent hills and valleys shows where further local polishing with a small tool is requisite. Sir Howard Grubb says that if he suspects a hollow, he holds his hand near the surface for a minute or two; if a hill is suspected, he washes the region with an evaporating wash. The warmth in the one case and the cooling in the other tend to rectify, and indeed over-rectify, the errors.

When success is finally attained, after all we have only a spherical surface, and it becomes necessary to obtain a parabolic form. This last stage is done by further tests of the kind described, with a diaphragm placed over the mirror which only permits the observer to see the light reflected from chosen zones of the mirror. The time at my disposal will not allow me to describe this in further detail, or to tell you how there is always found to be one definite diameter of the glass along which its weight must be supported. I must pass by, too, the system of counterpoised levels used for supporting the back of the glass, and the method by which silver is chemically deposited on its surface. Meagre although this sketch has been, it will have served to show you how beautiful are the processes employed, and I would ask you to realize that at first Herschel was a mere amateur, and had to discover everything for himself.

As I have said, Herschel had to do all his polishing by hand, and he found when once the final stage had begun, it was necessary that it should never stop even for a moment. Caroline relates how she was kept busy in attending on her brother when polishing: "Since by way of keeping him alive I was constantly obliged to feed him by putting the victuals by bits into his mouth. This was once the case, when in order to finish a 7-ft. mirror, he had not taken his hand from it for sixteen hours together."

The making of the mirror is, however, but a small part of the difficulty of making a telescope, for it involves high engineering skill to provide a solid stand, an observing platform, the graduated circles in right ascension and declination for setting the telescope and the clock, whereby it is made to follow the stars in their daily motion. The great size of Herschel's mirrors and the weight of the long tube introduced mechanical difficulties which were at that time entirely new.

A dozen years after his establishment at Bath, Herschel began to be well known in the world of science, and many of the most illustrious astronomers came to see him. In 1781 he was elected to the Royal Society, and in the same year he discovered the planet Uranus, and called it by the now almost forgotten name of Georgium Sidus, in honour of George III. The magnitude of the discovery may be estimated by the fact that only the five principal planets, familiar to all men for centuries, were then known; and the asteroids or minor planets had not yet been discovered by Herschel himself. His fame from this and his other discoveries led to a command from the King to take his 7-ft. telescope to Windsor, and there he was requested to act as celestial showman to the King, the Queen, and the Princesses. The expedition put him to much expense, and he was kept hanging about Windsor for months, but at length the King offered him the post of Private Royal Astronomer, with the modest salary of £200 a year.

Herschel's friend, Sir William Watson, said that never had a monarch bought honour so cheap, and Caroline pours scorn on the

King's meanness; but I think this was hardly fair. It must have been well known that Herschel had deserted from the Hanoverian Guards, and while the King might consent to forget this, it was a strong measure to take the deserter into his service. At a later date, moreover, when the King was informed by Sir Joseph Banks of Herschel's financial difficulties, he granted him £2000, afterwards increased to £4000, for the construction of the great 40-ft. telescope, with the condition that he should retain it for his own use. To this was added a further £200 a year for maintenance, and a pension of £50 a year to Caroline Herschel. And besides he was allowed to make specula for sale, and half the observatories of Europe were so furnished by him at prices which were then thought considerable.

At any rate Herschel jumped at the offer, which, by relieving him from his musical slavery, allowed him to follow the wish of his life. The Herschels then came to the neighbourhood of Windsor, and after several removals they finally settled at Slough. The change was delightful for him, since he now had space for his telescopes and workshops, but the difficulties of housekeeping in a rambling and dilapidated house rendered the change somewhat less agreeable to his sister.

The closeness to Windsor was perhaps a necessity of the case, but it had its disadvantages, since he was frequently summoned to take his telescope to Windsor, or large parties from the Castle would visit him at his house in order to see the wonders of the heavens. When his time had been wasted in this way he would make up for the loss by redoubled labour.

The fury, as I may call it, with which they worked may be gathered from Caroline's journal, and the work was not free from danger, because in his eagerness Herschel would not always delay his observations until the telescope was properly fixed. To stand in the dark on a platform without a railing, when your attention is distracted from your position, cannot be very safe, and they both met with a good many accidents which might easily have proved fatal.

The incessant work, together with the interruptions by the visitors from the Castle, began at length to tell on Herchel's health. His sister notes that on the 14th of October, 1806, after working all day, he was out from sunset till past midnight surrounded by fifty or sixty persons, without food or proper clothing, and that he never seemed to recover completely from this great strain on his strength.

But I have passed by an event of importance in the lives of both brother and sister, for in 1783 he married Mrs. Pitt, a lady of singularly amiable and gentle character. To the sister, however, the marriage was a great blow, for, although she continued to be his secretary and assistant, she moved into neighbouring lodgings, and was no longer so closely associated with him as theretofore. Mrs. John Herschel writes: "It is not to be supposed that a nature so strong and a heart so affectionate should accept the new state of things without much and bitter suffering," and tradition confirms this belief. All her notes and memoranda relating to a period of fifteen years from the time of the marriage were destroyed by her when, as we may presume, her calmer judgment showed her that the record of her heart-burning would be painful to the surviving members of the family. At any rate, she was on affectionate terms with her sister-in-law throughout all the later years of her life, and the brilliant career of her nephew, the celebrated Sir John Herschel, and correspondence with him, afforded the leading interest of her old age.

Although Herschel lived until 1822, and accomplished an enormous amount of work up to the end of his life, yet his health seems to have declined from about the time I have noted. On his death Caroline felt that her life, too, was practically ended, and she returned to Hanover. Ever afterwards she used to cry, "Why did I leave happy England?" and it is incomprehensible that she should not have returned to the place where all her real interests lay.

Although she felt the death of her brother as practically the end of her life, she was always full of jokes and fun. In a letter to her nephew, she told him that her father used to punish her, a grown woman, by depriving her of her pudding if she did not guess rightly the angle of the piece she had helped herself to. Dr. Groskopf writes of her when she was eighty-nine years of age, "Well! what do you say of such a person being able to put her foot behind her back and scratch her ear with it, in imitation of a dog, when she was in one of her merry moods." She only died in 1847, having very nearly completed her ninety-eighth year.

Herschel himself must have been a man of singular charm, as is testified to by Dr. Burney and his daughter Mdme. d'Arblay. That he possessed an incredible amount of patience is proved by the fact of his submitting to the reading aloud of the whole of a portentous, and fortunately unpublished, poem in many cantos by Dr. Burney, entitled "A Poetical History of Astronomy." It appears that Herschel had had an interview with Napoleon in Paris in 1802, and the poet Campbell asked him whether he had been struck by Napoleon's knowledge. "No," said Herschel, "the First Consul surprised me by his versatility, but in science he seemed to know little more that any well-educated gentleman, and of astronomy much less, for example, than our king. His general air was something like affecting to know more than he did know." He was struck, too, by Napoleon's hypocrisy in observing "how all these glorious views gave proofs of Almighty Wisdom."

And now having endeavoured to show what kind of people Caroline and her brother were, I must turn to what they did. Herschel's discoveries were so numerous that I am compelled to make a selection. I shall therefore only attempt to sketch his endeavour to understand the general construction of the stellar universe, and to speak of his work on double stars.

The only general test of the relative nearness or farness of the stars is their brightness, because the faint stars must, on the average, be more distant than the bright ones. Herschel then proposed to penetrate into space by means of a celestial census of the distribution and of the brightness of the stars. With this object he carried out four complete reviews of the heavens, as far as they may be seen from our latitude, passing successively to the fainter and fainter objects by means of the increased size of his telescope.

He divided the heavens into sweeps 2° 15′ of breadth in declination, and each zone was examined throughout by the process which he called star-gauging. His census was made with the 20-ft. reflector, with which instrument the field of view was about one quarter of the size of the full moon. It needs over 300,000 of such fields of view to cover the whole of the hemisphere of space, and Herschel surveyed the whole northern hemisphere, and as much of the southern one as he could.

Von Magellan in a letter to Bode describes the method of observation as follows: "He has his 20-ft. Newtonian telescope in the open air. . . . It is moved by an assistant who stands below it . . . near the instrument is a clock . . . in the room near it sits Herschel's sister, and she has Flamsteed's Atlas open before her. As he gives her the word, she writes down the declination and right ascension. . . . In this way Herschel examines the whole sky . . . he is sure that after four or five years (from 1788) he will have passed in review every object above our horizon. . . . Each sweep covers 2° 15′ in declination, and he lets each star pass at least three times through the field of the telescope, so that it is impossible that anything can escape him. . . . Herschel observes the whole night through . . . for some years he has observed . . . every hour when the weather is clear, and this always in the open air."

Herschel points out that by this survey he was not only looking into the most distant space, but also into the remotest past, for the light of many of the stars must have started on its journey towards us thousands or even millions of years ago. The celestial museum therefore exhibits to us the remotest past alongside with the present, and we have in this way the means of reconstructing to some extent the processes of evolution in the heavens. In photography the modern astronomer possesses an enormous advantage, but Herschel laid the foundation of this branch of astronomy without it.

The most conspicuous and the most wonderful object in the heavens is the Milky Way. It runs all round the skies in a great band, with a conspicuous rent in it forming a streamer which runs through many degrees. To the naked eye it shines with a milky light, but Herschel was able to show that it consists of countless stars in which there lie embedded many fleecy nebulæ. There is good reason to believe that the Milky Way on the whole consists of stars which are younger than those in the other parts of space, for the stars in it are whiter and hotter, and the nebulæ are mostly fleecy clouds. On the other hand, the spiral and planetary nebulæ are more frequent away from the Milky Way, and these are presumably older than the cloudy and flocculent nebulæ. The shape of the Milky Way seems to resemble a huge millstone or disk of stars, and since it forms a complete circuit in the heavens the sun must lie somewhere towards its middle. It is probable that we look much further out into space along this tract than elsewhere, although it happens that by far the nearest of all the stars—namely, α Centauri—lies in the line of the Milky Way.

This great congregation of stars is far from uniform in density, for there are places in it where there are but few stars or none at all. Caroline Herschel, writing to Sir John Herschel at the Cape of Good Hope, in 1833, mentions that her brother, when examining the constellation of the Scorpion (which lies at best low down on our horizon), had exclaimed, "after a long awful silence, 'Hier ist wahrhaftig ein Loch im Himmel.'" And her nephew, as he said, rummaged Scorpio with the telescope and found many blank spaces without the smallest star.

It will explain some of the deductions which Herschel drew from his star-gauges, and will at the same time furnish a good example of his style, if I read a passage from a paper of his written in 1789.* He points out that the sun is merely a star, and, referring to the stars, he continues thus :—

"These suns, every one of which is probably as of much consequence to a system of planets, satellites and comets, as our own sun, are now to be considered in their turn, as the minute parts of a proportionally greater whole. I need not repeat that by my analysis it appears that the heavens consist of regions where suns are gathered into separate systems, and that the catalogues I have given comprehend a list of such systems ; but may we not hope that our knowledge will not stop short at the bare enumeration of phenomena capable of giving us so much instruction ? Why should we be less inquisitive than the natural philosopher, who sometimes, even from an inconsiderable number of specimens of a plant, or an animal, is enabled to present us with the history of its rise, progress, and decay ? Let us then compare together, and class some of these numerous sidereal groups, that we may trace the operations of natural causes so far as we can perceive their agency. The most simple form, in which we can view a sidereal system, is that of being globular. This also, very favourably to our design, is that which has presented itself most frequently, and of which I have given the greatest collection.

"But, first of all, it will be necessary to explain what is our idea of a cluster of stars, and by what means we have obtained it. For an instance I shall take the phenomenon which presents itself in many clusters. It is that of a number of lucid spots, of equal lustre, scattered over a circular space, in such a manner as to appear gradually more compressed towards the middle, and which compression, in the clusters to which I allude, is generally carried so far, as, by imperceptible degrees, to end in a luminous centre of an irresolvable blaze of light. To solve this appearance it may be conjectured that stars of any given very unequal magnitudes may easily be so arranged, in scattered, much extended, irregular rows, as to produce the above described picture; or, that stars, scattered about almost promiscuously within the frustum of a given cone, may be assigned of such properly diversified magnitudes as also to form the same picture. But who, that is acquainted with the doctrine of chances, can seriously maintain such improbable conjectures?"

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* Phil. Trans., vol. lxxix. p. 212.

Later in the same paper he continues:—

"Since then almost all the nebulæ and clusters of stars I have seen, the number of which is not less than three and twenty hundred, are more condensed and brighter in the middle; and since, from every form, it is now equally apparent that the central accumulation or brightness must be the result of central powers, we may venture to affirm that this theory is no longer an unfounded hypothesis, but is fully established on grounds which cannot be overturned.

"Let us endeavour to make some use of this important view of the constructing cause, which can thus model sidereal systems. Perhaps, by placing before us the very extensive and varied collection of clusters and nebulæ furnished by my catalogues, we may be able to trace the progress of its operation in the great laboratory of the universe.

"If these clusters and nebulæ were all of the same shape, and had the same gradual condensation, we should make but little progress in this enquiry; but as we find so great a variety in their appearances, we shall be much sooner at a loss how to account for such various phenomena, than be in want of materials upon which to exercise our inquisitive endeavours.

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"Let us, then, continue to turn our view to the power which is moulding the different assortments of stars into spherical clusters. Any force, that acts uninterruptedly, must produce effects proportional to the time of its action. Now, as it has been shown that the spherical figure of a cluster of stars is owing to central powers, it follows that those clusters which, *ceteris paribus*, are the most complete in this figure, must have been the longest exposed to the action of these causes. This will admit of various points of view. Suppose, for instance, that 5000 stars had been once in a certain scattered situation, and that other 5000 equal stars had been in the same situation, then that of the two clusters which had been longest exposed to the action of the modelling power, we suppose would be most condensed, and more advanced to the maturity of its figure. An obvious consequence that may be drawn from this consideration is that we are enabled to judge of the relative age, maturity, or climax of a sidereal system, from the disposition of its component parts; and, making the degrees of brightness in nebulæ stand for the different accumulation of stars in clusters, the same conclusions will extend equally to them all. But we are not to conclude from what has been said that every spherical cluster is of an equal standing in regard to absolute duration, since one that is composed of a thousand stars only must certainly arrive to the perfection of its form sooner than another which takes in a range of a million. Youth and age are comparative expressions; and an oak of a certain age may be called very young, while a contemporary shrub is already on the verge of its decay. The method of judging with some assurance of the condition of any sidereal system may perhaps not improperly be drawn from the standard laid down earlier; so that, for instance, a cluster or nebula which is very gradually more compressed and bright towards the middle may be in the perfection of its growth, when another which approaches to the condition pointed out by a more equal compression, such as the nebulæ I have called *Planetary* seem to present us with, may be looked upon as very aged, and drawing on towards a period of change, or dissolution. This has been before surmised, when in a former paper I considered the uncommon degree of compression that must prevail in a nebula to give it a planetary aspect; but the argument, which is now drawn from the powers that have collected the formerly scattered stars to the form we find they have assumed, must greatly corroborate that sentiment.

"This method of viewing the heavens seems to throw them into a new kind of light. They now are seen to resemble a luxuriant garden, which contains the greatest variety of productions, in different flourishing beds; and one advantage we may at least reap from it is, that we can, as it were, extend the range of our experience to an immense duration. For, to continue the simile I have borrowed from the vegetable kingdom, is it not almost the same thing, whether we live successively to witness the germination, blooming, foliage, fecundity, fading, withering, and corruption of a plant, or whether a vast number of specimens, selected from every stage through which the plant passes in the course of its existence, be brought at once to our view?"

I now turn to another line of discovery of which I cannot show any pictures, but which, to me at any rate, is more interesting. Until 1838—that is to say, until sixteen years after Herschel's death—no one had succeeded in determining the distance of a single fixed star, but in that year Henderson and Bessel almost simultaneously attained success in the cases of the two stars α Centauri and 61 Cygni. The attempts at this measurement had already been numerous, and Herschel amongst others had failed, but his failure was a glorious one, for he made incidentally a discovery of another kind and of at least equal interest.

The earth moves round the sun at a distance of 93 million miles, so that in six months we shift our position by 186 million miles. If, then, there are two stars of which one is relatively near to and the other far from the sun, but so situated as to appear to us very close together, the near one ought to shift its position relatively to the distant one in the course of each six months. The amount of this change of position, called by astronomers annual parallax, should furnish the distance of the nearer of the pair, provided that the other is very far off. This idea is as old as the time of Galileo, but no one had been able to make successful use of it.

As I have already said, the only general test of the distance of a star is its brightness, and therefore Herschel chose pairs of stars of very different brilliancy. He thought, at least at first, that it was mere chance which brought the stars so near to one another, and there are undoubtedly such pairs now known as "optically double stars." But Herschel's mode of attack was bound to fail if the seemingly neighbouring stars were really so, and were linked together by their mutual gravitation. Already as early as 1707 Michel had suggested the existence of such true double stars, but it was Herschel who proved their existence. His first catalogues of double stars, published in 1782, contained 203 cases of such doublets, and he already suspected a community in their motions explicable only by their real association; but by 1802 he had become certain. In many cases the two components of a binary pair were found to be moving in nearly the same direction and at the same speed, but superposed on this motion of the system as a whole there was an orbital motion of one star round the other. Herschel even lived long enough to see some of his pairs of stars perform half a revolution about one another.

After his death Savary took the matter one stage further, and showed that the revolution was governed by the laws of gravity, and thereby confirmed the truth of Herschel's belief. Thus the failure to measure the distance of stars led to the proof that gravity reigns amongst the stars as in the solar system.

Arago thought that of all Herschel's discoveries this was the one that had the greatest future, and his prophecy has proved singularly correct. Every year adds to the number of double stars, whose orbits are now accurately determinable. These systems are found to be very unlike our own solar system, for the component stars are, in many cases, far larger than the sun and revolve about one another in periods which, in various cases, may be either many years or only a few hours.

The spectroscope has, moreover, added enormously to our knowledge, for the speed of approach or recession of a star from the sun can now be determined as so many kilometres per second. Thus that component of the motion of a star which was concealed from Herschel is now known with the greater certainty. Moreover, being ignorant of the distance of the stars, he could only express the transverse component of motion in seconds of arc.

A wonderful corollary also results from the use of the spectroscope, namely, the existence of many stars known as "spectroscopic binaries." As seen even with the most powerful telescope such a star is a single point of light, but if the spectral lines are duplicated we know that the source of light is double, and that one component is approaching us and the other receding from us. In this way the orbits and relative masses of these visually inseparable stars are determinable. The number of known double stars, including both visual and spectroscopic ones, is already large, and Campbell, of Lick Observatory, has expressed his opinion that one star in six is double. Some of them revolve so near to one another and in such a plane that they partially eclipse one another as they revolve, and thus produce a winking light like that of a lighthouse. It would seem that we can now even tell something of the shapes of a pair of stars visually inseparable from one another. But I must not go further into this subject, and will only repeat Arago's saying, that this discovery of Herschel's has "le plus d'avenir."

It is a figure of speech to refer to the stars as fixed, for a large number of them possess a measurable amount of "proper motion" relatively to their neighbours. The existence of double stars was discovered by the observation of their movements, and thus the study of proper motions is linked to the subject of which I have just been speaking. Some few proper motions had been observed by earlier astronomers, but when Herschel took up the subject proper motion had not been accurately measured in any case.

If a man is walking through a wood the trees in front of him seem to be opening out before him, whilst those behind seem to be closing together. In the same way if our sun is moving relatively to the centre of gravity of all the stars, the stars must on the average seem to move away from the point towards which the sun is travelling, whilst they must close in towards its antipodes. These two points are called the apex and antapex of the sun's path.

Now Herschel concluded that there was something systematic in the proper motions of the stars, and that there was a point in the constellation of Hercules from which the stars were on an average receding, and that similarly they were closing in towards the antipodal point. The first of these is the sun's apex and the second the antapex. These conclusions were drawn from the motions of comparatively few stars, but the result has been confirmed subsequently from a large number. Moreover, we have now learned by means of the spectroscope that we are travelling towards Hercules at the rate of about sixteen miles a second.

During these last few years this grand discovery of Herschel's has gained a great extension at the hands of Kapteyn and of many others, and it has been proved that other systematic motions of the stars are discoverable. The time at my disposal will not permit me to pursue this subject further, but I may say that it now appears that if we could view the universe from the centre of gravity of the stars of the Milky Way, we should see a current of stars coming from a definite direction of space and penetrating our system.

What a vista of discoveries do these ideas open up to the astronomer! Some centuries hence the sun's apex may have shifted, and we may perhaps learn that the solar system is describing the arc of some colossal orbit. The drift or current of stars may also have begun to change its direction, and our descendants may have begun to make guesses as to its future course and as to its meaning. But whatever developments the future may have in store, we should never forget that the foundation of these grand conceptions of the universe was laid by Herschel. Holden ends his "Life of Herschel" with words which may also serve as a fitting end to my lecture: "As a practical astronomer he remains without an equal. In profound philosophy he has few superiors. By a kindly chance he can be claimed as the citizen of no one country. In very truth his is one of the few names which belong to all the world."

[G. H. D.]

Wednesday, May 1, 1912.

SIR JAMES CRICHTON BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1911, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Forty-six new Members were elected in 1911.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1911.

The Books and Pamphlets presented in 1911 amounted to about 281 volumes, making, with 677 volumes (including Periodicals bound) purchased by the Managers, a total of 958 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.

TREASURER—Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.

SECRETARY—Sir William Crookes, O.M. LL.D. D.Sc. F.R.S.

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Alexander Siemens, Esq., M.Inst.C.E. M.Inst.E.E.
The Right Hon. Sir James Stirling, P.C. LL.D. F.R.S.
Alan A. Campbell Swinton, Esq., M.Inst. C.E. M.Inst.E.E.

VISITORS.

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Carl E. Melchers, Esq.
Major G. J. W. Noble.
William Stone, Esq., M.A. F.L.S. F.C.S.
Harold Swithinbank, Esq., J.P. F.R.S.E.

WEEKLY EVENING MEETING,

Friday, May 3, 1912.

SIR JAMES CRICHTON BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

W. C. D. WHETHAM, Esq., M.A. F.R.S.

The Use of Pedigrees.

INTEREST in ancestors, their qualities, their work and their lives, is a deep-rooted and healthy instinct in mankind. The achievements of one's forefathers are a keen incentive to an honourable and strenuous life; their faults and failings are a warning and a danger signal in the path. Desire to prove a distinguished descent has led to the discovery, and one may add the invention, of many remarkable pedigrees.

Few authentic pedigrees, even of our oldest families, begin before the latter half of the twelfth century, though numerous claims of Norman and Saxon descent are made. Many of these claims are founded on an agreement between the Christain name of some Saxon chief, sufficiently prominent to satisfy the ambition of the claimant, and the surname, long afterwards adopted by the descendants of some obscure person, who chanced to have the same Christian name and lived at the time when surnames were coming into use. Interest in family history was probably stimulated keenly by the introduction of surnames. The old question of "What's in a name?" is best answered by showing how much greater trouble has been taken to trace descent from father to son where there has been a fixed name to transmit.

Indeed, the power of the name has often obliterated interest in other lines of ancestry which are of equal importance from the point of view of heredity. This effect has been intensified by the laws of England controlling the inheritance of real property and of the right to bear arms, with which the heralds, the official genealogists, were primarily concerned. Hence it is that we find so often in early pedigrees such courageous pictures as that shown in Slide No. 1, where a single line is traced through no less than seven individuals, of whom nothing is ventured beyond names of more than doubtful authenticity. Somewhat similar treatment is seen in the earlier part of the heraldic roll of the Lovell predigree kindly entrusted to me for the evening by the Master and Fellows of Gonville and Caius College. In the later part of this roll, and in the finer and earlier roll, belonging to my friend Mr. Huddleston, of Sawston, and shown in Slides 2, 3 and 4, greater interest is shown in females, especially where an heiress brings a new quartering, if not lands and money as well, into the family. Thus the great Nevill heiress, to whom the Huddlestons owe Sawston, is especially emphasized.

To draw up such pedigrees as these the heralds made periodical visitations of the counties of England. It is said that in Elizabethan days the length of the line they were able to trace, and the nobility of the origin they discovered for the family, depended to some extent on the munificence of the entertainment provided for them by that family's existing representative. But no scandal about Queen Elizabeth or her heralds can blind us to the soundness of the underlying idea of the visitations—the idea of a complete genealogical survey of the governing classes of the country.

In modern times the College of Arms is a stern guardian of genealogical truth, and all confidence may be felt in its work. To the courtesy of one of the officers of the College I owe not only the first slide that I showed, but also the interesting pedigree of his own family illustrated on Slide No. 5. This slide will serve to introduce us to a new use of pedigrees, a use which is the principal subject of this lecture. The family of Wollaston has the great distinction of producing many eminent men of science, no less than seven of whom were Fellows of the Royal Society. Somewhat similiar is the pedigree of the allied families of Darwin, Galton and Wedgewood, where in five generations we see sixteen men of scientific attainments, of whom nine were Fellows of the Royal Society. Another form of ability is traced in the pedigree of Slide No. 6, where the characteristic genius is legal and administrative. In these diagrams the particular quality to be traced is indicated by a clearly marked symbol.

Such cases could be extended to a great number, but not indefinitely; the stock of such ability in the country is large, but not unlimited.

Now, of course, it is easy to say that some of the families we have described had the benefit of a suitable environment. Quite true. But a good environment, though it may develop, cannot create ability, and a detailed study of the pedigrees we have given, and others like unto them, makes it clear that no amount of "influence" can explain the achievements of the men therein recorded.

Even where position and opportunities are more or less equal, we find enormous differences between the average value of different families. One of the difficulties of such comparisons lies in the scarcity of records save of those who are highly placed in the world. Hence the advantages which Dr. F. A. Woods found in investigating the history of the Royal Houses of Europe. The great concentration of genius on Slide No. 7 shows the origin of the ability of Frederick the Great, and his remarkable fraternity of brothers and sisters. The ancestry is really abler than the table shows, because the pedigree is so closely inbred, the chief stocks being Montmorency, Coligny, Brunswick, and especially Orange. This is emphasized in the next slide, No. 8, where of the eight great-grandparental strains, comprised in six individuals, four are shown to bring in the great Orange blood by way of William the Silent.

In this case, where the abilities and character are good, a certain amount of inbreeding does but accentuate them. Similarly, where both are bad, inbreeding serves to visit them in double measure on the unfortunate descendants. Slide No. 9 shows a table from the "Cambridge Modern History," giving the descent of the Houses of Castile and Arragon, and the next slide, No. 10, shows the inbred ancestry of Don Carlos, the son of Philip II. Owing to this inbreeding, Don Carlos had only four great-grandparents, instead of the normal eight; two of the four are described as of weak character, and one as insane. In the preceeding generation Isabella of Castile appears three times, and thus, able though she herself was, brings in three times the blood of her weak-minded father and her insane mother, who have an equal or better chance of influencing her descendants than those other abler ancestors from whom she herself chanced to pick her ability.

It is impossible to explain such phenomena as these on the theory of environment. Inborn qualities force their way to the front, though they may be modified or developed by training and other circumstances.

Bad qualities are not confined to royal houses, nor, whatever the "spirit of the age" may say, to the upper classes. The next two slides show pedigrees collected from the poorhouses of a great republic. Here we have definitely feeble-minded families that generation after generation show their characteristic mental defects. Both pedigrees illustrate the established fact that two feeble-minded parents of this type never produce a single normal child; all their offspring are mentally defective. Yet, in this country at least, nothing is done, either by the law or the Church, to prevent their union.

Now the social importance of these results can hardly be exaggerated. A large proportion of our paupers, probably at least a quarter, are themselves mentally defective, while many others come from families of this kind. A typical group of pauper families is shown in Slide No. 13. A large proportion of petty crime, and a considerable proportion of that which is more serious, is due to the same cause. The direct cost to the country of these people in poor-rates, police, and prisons is enormous, while the indirect cost in the lowering of average efficiency is incalculable. It is to be hoped that the long promised Bill to carry into effect the recommendations of the Royal Commission of 1904–1908 will find its way through our distracted Parliament this session, and place these unfortunates under life-long care and control. Each year's delay probably brings into the world a number of defectives who will cost society about twice as much as the estimated annual charge of the Commissioners' scheme, while the misery and degradation involved are fearful to contemplate.

When we pass from the pedigrees of the feeble-minded to those of the insane we again meet with clear evidence of heredity. Even the particular type of delusion, and the action based on it, are inherited. Thus in Slide No. 14 we have suicidal melancholia manifested in the family A in a tendency to drown themselves, while in the family B it takes the form of shooting. When a marriage between representatives of the two families took place the ten children were in a difficulty. Two drowned themselves, one shot himself, two, as a compromise, took poison; two more, hesitating too long, were confined safely in asylums; while three are described as normal.

Pedigrees might be given to illustrate the inheritance of almost any quality, physical or mental. Even the tendency to produce twins is hereditary (Slide No. 15).

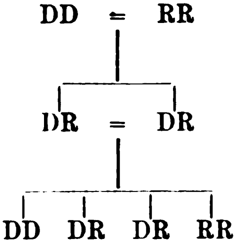
Special interest is now felt in tuberculosis, which has been proved to be an infective disease. This result has somewhat tended to obscure the no less certain fact that the special liability to the infection which makes some individuals succumb is definitely hereditary. Countless pedigrees might be given to show this inheritance. Here is one (Slide No. 16). In present conditions the germs of tubercle are so wide-spread that nearly everyone is infected. But most of us are resistent to a moderate attack. Only those specially liable contract the disease. Those liable fall into two groups. In one the predisposition to tuberculosis is an isolated weakness in an otherwise sound stock which may be of great value to the nation; in the other it is but one of many forms, in which radical unsoundness shows itself.

Hitherto we have dealt with the facts of the pedigrees as they came, and made no attempt to discover any definite laws of inheritance. I expect that of late years this room has often resounded with the name of Mendel, and I will give but the shortest account of the phenomena associated with his name.

It is found that certain qualities are inherited as definite units which are either present or absent. With regard to such qualities we are not a vague blend of all our ancestors, but are composed of various units, picked here and there by chance from our ancestral tree.

As an illustration we may take Mendel's original experiments on green peas. A dwarf variety crossed with a tall variety gave seedlings which were all tall. Hence tallness is said to be dominant and dwarfness recessive. But, when self-fertilized, these offspring proved different from the original pure-bred tall parent. One quarter of their seeds gave rise to pure tall plants which bred true, one quarter to pure dwarf plants which also bred true, while the remaining half gave "talls," outwardly indistinguishable from the first group, but, when self-fertilised, reproduced the mixed descendants of the second generation.

These phenomena are explained if we assume that the germ cells of the pea bear either the character "tall" (D = Dominant) or the character "dwarf" (R = Recessive). By the chance conjunctions of opposite cells we then get the pedigree,



A pure-bred dominant crossed with a pure-bred recessive gives offspring all of the mixed type, DR, which self-fertilized give, from a large number of seed, one quarter DD, one quarter RR, and half DR,

which agrees with the experimental relations we have described.

Similar results, often complicated it is true with the linkage of qualities and in other ways, have been found for many characters of plants and animals. In mankind also the phenomena of the segregation of unit characters appear. Thus eye-colour is a Mendelian character, brown being dominant, and blue or grey recessive. Brown eyes can only be transmitted by those who themselves possess them, while two blue-eyed parents never have a brown-eyed child (Slide No 17). Many diseases and physical defects also show Mendelian descent. Instances are the shortening of fingers, known as brachydactyly (Slide No. 18), and cataract (Slide No. 19).

Now, the importance of these Mendelian relations lies in the fact that they enable us to predict the probable result, as far as these qualities are concerned, of any given marriage, and, in large families to foretell almost the exact proportion in which a character may be looked for in the descendants. When more of these phenomena have been traced, and are generally understood, they cannot but have a profound social significance.

The facts we have passed in review force on our minds the conception of the nation as composed of families as the true social unit. Each family with its inborn qualities, changing from generation to generation in accordance with the wisdom or folly of its marriages, but at any given time of definite value, is an asset to or a charge on the nation of which it forms part. Environment may give those qualities a fair chance or condemn them to useless sterility; hence the importance of a good environment, towards which, indeed, all our efforts have hitherto been directed. But it seems that acquired characters are not inherited, and so environment cannot create inborn qualities, though a race of good inborn aptitudes will inevitably create ultimately a good environment.

How far is the more fundamental and more important factor of the inborn character of the race under our own control? Can we modify it to our will, and, if so, is it safe to interfere? I believe it is only safe or justifiable for legislation to interfere in very clear and definite cases, of which as yet our scanty knowledge indicates perhaps but one—the case of congenital mental defect of the hereditary type. Not only are the feeble-minded a charge on and a danger to the community while at large, but they are themselves happier when under suitable control. For their own sake, no less than for that of society, they must be placed under care and control for their lives. More knowledge is needed before legislation should interfere in other cases. But that does not involve the uselessness of such researches as those we have traced. The pressure of opinion may do much when it is realized how much more important a good heredity is than large marriage settlements.

But though direct interference by law is feasible as yet in one case only, the indirect effects of legislative and social change may be very great. Whether we will or no, legislation and social change are continually lightening the conditions of life for some sections of the people as compared with others. It becomes relatively easier for them to marry early and to rear children. Their relative numbers tend to increase.

As an example of a social change which affected largely the composition of the nation, let us study the industrial revolution, as it is called, of the early years of the nineteenth century. Till the eighteenth century the population of these islands remained almost stationary save for comparatively small fluctuations. The resources were limited and nearly constant in amount, and could support only a definite number of people. But the discoveries which brought our coalfields into action as a source of power initiated a steady growth in resources, and the population at once began to rise to fill the places created. The ability of the country to support population grew with amazing rapidity, yet nothing was done to investigate the quality of the population which appeared. The policy of *laissez faire* was in possession, and, till the era of Factory Acts, even the environment was as bad as could be. The question of innate qualities had not even appeared.

By invention and discovery (the work of the brains of a limited number of able men) the energy of our coal was made available, and thus a new field for the growth of population was opened up, tilled and dressed. But no one saw that the really important thing was not the draining and fertilizing of the land, but the quality of the crop of men to be raised thereon. No one examined the seed planted, no one even asked whether it were the seed of good corn or of useless or harmful weeds. No one tended its growth to secure favourable chances for what was of value in the mixed crop.

Now, what should we do with a farmer who was supplied with good ground, and with basic slag and superphosphate in constantly increasing quantities, if he left the land to seed itself, and used neither hoe nor harrow on the weeds? I think that, in spite of the risks of the Agricultural Holdings Act, we should give him notice to quit. The policy of *laissez faire* may be sound economically; it does not work well with living beings, either corn or men.

The growth of our population still continues, and still little heed is given to the breed of men that take advantage of the increasing stores of energy that are brought into play by the ingenuity of our engineers and manufacturers. What sections of the people are tending to increase? What are their special qualities compared with those of other parts of the community?

Such a wide question cannot be answered with the knowledge at our disposal. But it is one that the country ought to face. The office of the Registrar-General should be given power and means to investigate these problems in a far more thorough manner than it can at present. A genealogical survey of the people is at least as important as a geological survey of the land. The inborn qualities of the race are the greatest of national possessions. They should be studied and treasured accordingly.

As a temporary and tentative expedient, the Eugenics Laboratory at University College, London, and the Eugenics Education Society collect, preserve and tabulate pedigrees. As an example of modern requirements, I give in Slide No. 20 the schedule sent out by the Society.

The questions to be asked and answered by such a Government survey cannot even be formulated in the short time at our disposal. I can but suggest in the most tentative way two out of the many considerations which would appear to be worth investigation. The first and most difficult is the broad question of race and racial qualities.

In modern Europe the distribution of the physical characters of the people indicates that in three regions certain combinations of those characters are found in great purity. Hence it is concluded that there are three great races, with numerous subdivisions. The three races are:—

(1) The Mediterranean race, which from early times has lived round the shores of that sea, and spread up the Atlantic coasts. Its chief physical characteristics are a long-shaped skull, small stature and dark colouring.

(2) The Alpine race, inhabiting the central mountainous region, from Asia to France. They have broad heads, medium stature and brown colouring.

(3) The Northern or Teutonic race, whose original home seems to be round the shores of the Baltic. Their characters are long-headedness, great stature and fair colouring.

In the British Isles the broad head of the Alpine race is practically absent, and we are left with the Mediterranean and the Northern races only, with subdivisions. As head shape fails us as a means of discrimination, we must rely on coloration (Slide No. 21) and stature (Slide No. 22). As a general result we may say that the characters of the Northern race are purest in the north-east, and those of the Mediterranean race in the south-west, and especially in South Wales and Cornwall. In these regions they took over the language and culture of the extinct broad-headed Celts.

Taking the country as a whole, it seems that the upper classes tend to be taller and fairer, and city dwellers tend to be shorter and darker, than the average, and we may ask whether this latter fact does not mean that our industrial and urban life is more favourable to the Mediterranean type of our people than to the Northern. But it is worth noting that on the Continent, where the Alpine race exists, it is found to be still less adapted to urban life than the Teutonic type.

In England, then, it seems likely that the characters of the southern race are tending to increase. By a study of the "Dictionary of National Biography," Havelock Ellis has found reason to believe that the characteristic genius of men of ability who come from different parts of the country shows differences which follow the racial divisions. The Anglo-Dane has great mathematical power, hence Cambridge, which in early days drew chiefly from the north and east became the home of mathematics and physical science. The south-west counties and the Welsh border breed a genius which is predominantly literary and philosophic—hence the character of Oxford. Broadly, it is a matter of race, and mental qualities follow the physical. A growth in the Mediterranean elements in our people, as measured by their physical qualities, then, would tend to favour their mental character also, to produce a quick emotional temperament, as against the slower but more strenuous and rational Teutonic character.

The movement to the towns has been going on for more than a century, but in the last forty years another phenomenon has appeared, perhaps of even more social importance. The voluntary restriction of the birth-rate began to affect statistics about the year 1875, and has gone on increasing ever since. In all ranks of life it affects the thrifty and far-seeing more than the casual and reckless, and it is specially marked among the skilled artizans and in the upper and professional classes. Here again we have a weighting of the scales in favour of somewhat similar qualities to those which we found predominantly associated with city life.

It seems likely then, if present tendencies continue, that, as the years pass on, observation may show that on the average our people will become continually shorter in stature, darker in colouring, more emotional, and less rational and persevering. The Northern race will tend to become merged in the Mediterranean stock.

Now it seems that there is some evidence to show that twice at least before in the history of the world a somewhat similar process has gone on. "The glory that was Greece, and the power that was Rome," are the most striking phenomena of the ancient world. And in each case most authorities now tell us that the nation was composed of a tall, fair-haired governing class of Northern origin, and a shorter, darker indigenous stock of Mediterranean source. It may be put forward as a suggestion for further consideration, that while a proper balance was maintained between the two elements those great nations flourished, but that, when the Northern elements were depleted too much by war, or by a differential birth-rate, or became merged in the mass of the population, the vigour of Greece and Rome decayed preparatory to a fall doubtless hastened by other causes. Houston Chamberlain holds that the similar influx of Germanic tribes into North Italy during the Dark Ages produced, when the time was ripe, the remarkable outburst of genius which marked the Renaissance in that favoured land.

I have tried to put before you examples of the knowledge we now possess. Still more have I tried to indicate the endless problems of race that lie ahead unsolved. Till we know more, action may be dangerous. But the danger of not searching for light is greater still.

[W. C. D. W.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1912.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

The Honorary Secretary announced that His Grace the President had nominated the following gentlemen as Vice-Presidents for the ensuing year :—

The Right Hon. Lord Avebury, P.C. D.C.L. LL.D. F.R.S.
The Right Hon. The Earl of Halsbury, P.C. D.C.L. LL.D. F.R.S.
Donald W. C. Hood, Esq., C.V.O. M.D.
Sir Francis Laking, Bart., G.C.V.O. M.D. LL.D.
Henry F. Makins, Esq., F.R.G.S.
Alexander Siemens, Esq., M.Inst.C.E.
Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S. (Treasurer),
Sir William Crookes, O.M. D.Sc. F.R.S. (Honorary Secretary).

Victor Blagden, Esq.
W. A. Harris, Esq.

were elected Members of the Royal Institution.

The Right Hon. Lord Rayleigh, O.M. P.C. M.A. D.C.L. LL.D. F.R.S., was re-elected Honorary Professor of Natural Philosophy.

Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. F.R.S., was re-elected Professor of Natural Philosophy.

The Honorary Secretary reported, That the Managers, at their Meeting held this day, had re-appointed Sir James Dewar, M.A. D.Sc. LL.D. F.R.S., Fullerian Professor of Chemistry, for a period of three years.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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WEEKLY EVENING MEETING,

Friday, May 10, 1912.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Professor W. Stirling, M.D.(Edin.) LL.D. D.Sc., Professor of Physiology, University of Manchester.

The Gaumont Speaking Cinematograph Films.

(Illustrated by the aid of Mons. Gaumont.)

Johannes Müller, one of the greatest physiologists of last century, when considering the time factor in nervous processes, was so impressed with the inherent difficulties of the question, that he said, "We shall probably never attain the power of measuring the velocity of nervous action, for we have no opportunity of comparing its propagation through immense space as we have in the case of light." As is often the case, when the forecast is darkest, light is near.

Shortly after this forecast was published, one of Müller's own pupils, Hermann Helmholtz, solved the problem on two inches of a frog's nerve attached to a living muscle—an epoch-making experiment of immense importance in nerve physiology. Thanks to advances in technique we can now measure not only the velocity of a nervous impulse but also the rapidity, or rather the slowness of the act of thinking, as well as the speed of vocalisation and articulation—also the time it takes to think, to will, and to execute any particular co-ordinate movement, be it of a limb or of the vocal organs themselves, in response to an adequate stimulus applied to the skin, eye, ear, or other sense-organ.

As correlation and accuracy of time-adjustment are important factors in the demonstration, which I shall have the honour of presenting to you to-night, it may be interesting to recall an historical event in the varied career of Charles V.

Having tired of King Craft, like some others of eccentric tendencies, the Emperor beguiled his leisure with horology and the study of clocks. After making valiant though unsatisfactory attempts to co-ordinate and synchronise three clocks, it at last dawned upon His Majesty that he had wasted much time and energy in trying to co-ordinate the irreconcilable and turbulent political factors in his own kingdom.

Co-ordination and synchronisation are marked functions of the central nervous system. The perfect co-ordination between the executive as represented by the motor organs of the body and the receptors of various kinds, and the way in which these organs are influenced now by one receptor, now by another, are trite facts in nervous physiology. A sound or a flash of light to the left is followed immediately by a turning of the head and neck and eye-balls to the left. One organ sets the completed machinery in orderly motion. As an example of the effect of a bugle sound falling on well-trained disciplined ears, I know of no more pathetic example than an event which happened on the battlefield at Thionville in 1870.

"After the slaughter of Thionville on the 18th August a strange and touching spectacle was presented. On the evening call being sounded by the first regiment of Dragoons of the Guard, six hundred and two riderless horses answered to the summons—jaded and in many cases maimed. The noble animals still retained their disciplined habits."

Royal Institution and the Cinematograph.

The question of the synchronisation of two very different instruments will be presented to-night; but before I do so, I should like to recall what an important part the Royal Institution has played in connection with one of these instruments, viz. the Cinematograph. It was on February 28, 1896, in the Royal Institution that Mr. Robert Paul showed an instrument which he called the "Theatrograph." This was the first demonstration before a scientific Society of the wonders of what was then called Animated Photography. The Cinematograph is merely a modified camera with a special projector. As a matter of fact, the Cinematograph as employed to-day—no doubt with numerous modifications—is in principle the same as the machine first used by Mr. Paul, and demonstrated to the members of the Royal Institution. Nor do I forget the lectures on the Gramophone delivered by Prof. McKendrick. In the Kinetoscope of Edison only one person at a time could see the moving picture.

The Problem stated.

The problem before us to-night is how to obtain *at the same time* records from a Cinematograph and from a Phonograph, Gramophone or talking machine, and having obtained these, how can they be reproduced and presented *simultaneously*, the one record to the eye and the other to the ear, so that a large audience—even six thousand in number—shall be able to see and hear that all marches in unison and produces an illusion so complete as almost to represent real life.

In the ordinary speaking and moving pictures which have been presented hitherto, the actor or singer has just to speak or sing into a phonograph placed close to his mouth, whereby a record is obtained. This is reproduced on an appropriate machine, and when he hears the sounds, he makes as best he can the appropriate sounds, movements and gestures while the cinematograph records. There is no question of *simultaneous recording and reproduction* of the double record. Consequently, the result is not satisfactory. By means of the Chronophone large scenes as well as the effects of a full chorus are obtained at one and the same operation.

In order to arrive at full intellectual satisfaction while contemplating any scene, such as an artist singing or an actor playing and speaking his part—where eye and ear are simultaneously appealed to—the spectator demands that measure of pleasurable satisfaction which is afforded by beauty of form, grace of movement, the allurement of colour, and the seductive sounds of sweet music. All these, I think, I can promise you to-night, thanks to the inventive ingenuity and technical skill of Mons. Léon Gaumont and his collaborators, the inventors of the "Chronophone." Mons. Gaumont has not only arranged and personally superintended the whole complicated installation which you see before you, but he has also favoured us with his presence here to-night. We give him a hearty Royal Institution welcome.

Difficulties of the Problem.

At first sight it might seem as if the problem of producing *simultaneously* combined pictorial and audible records was a comparatively simple one. It is, however, far from being so. Edison himself, when he invented the "Kinetoscope," cherished the idea of combining the production of movement by the aid of animated photography and of sound by the aid of the Phonograph. The solution of the problem presents many difficulties. Many attempts have been made both in America and Europe since the invention of the Cinematograph, but at present there is only one complete solution of the problem of simultaneous reproduction of the gestures and voice or articulate speech of an artiste or group of actors, or the movements of animals simultaneously with the vocal sounds which on occasion they emit, be it the strident tones of a Gallic cock or the growl or fierce roar of an angry lion.

Having regard to the enormous differences in the velocity of the propagation of sound waves and luminous waves, it might be thought that important corrections might have to be made. It is not so, however. To all intents and purposes, at a short distance as in a theatre, the illusion of blending speech voice and movement is complete.

Let me remind you of some of the difficulties of the problem, for the apparatus—the camera and the talking machine—must not only be synchronised, but the result must be such that the sound produced shall be audible to a large assembly of people. We may lay down the following conditions :—

1. Absolute synchronism between the Phonograph and the Cinematograph both in recording and reproducing the result.

2. Registration of sound by the Phonograph at a sufficient distance at the same time as the registration of the pictures on the moving film, without the Phonograph being in the field of the Cinematograph.

3. The amplification of the sound so that a large audience can hear the sound and observe the exact correlation between the movements of the speakers, or actors, or singers, and the audible sounds as regards pitch, loudness, and quality of the vocal or other sounds.

It has been calculated that in a record on an ordinary 12-inch disc of a gramophone the length of its sinusoidal sound line or spiral groove—counting 100 grooves to the inch from the centre to the circumference of the disc—is about 240 yards or 720 feet. If, however, the ripples made by the vibrating stylus as the disc revolves under it at the rate of 32 inches per second be added, it brings up the total length of the sound line—in the reproduction of a sound record lasting from three to four minutes – to, it may be, 500 yards or 1500 feet. The disc makes about 76 revolutions per minute or an average rate of each revolution in 0·8 second.

It is also to be noted that in reproducing sound from a phonograph or gramophone record the stylus, in passing from the circumference to the centre, travels over a regularly diminishing spiral, and therefore covers a steadily diminishing distance, but it accomplishes this *in the same time*, so that there is practically no alteration in pitch. The marks of the vibrations are consequently much farther apart at the circumference than at the centre of the disc.

First Synchronisation of the two Instruments.

It is not enough to have a perfect synchronism between the Phonograph and the Cinematograph—between the talking-machine and the camera. The vocal sounds of one or more speakers must be registered at a distance of several yards from the phonograph. To do this without altering the purity and intensity of the sounds emitted is no easy problem.

Obviously the Phonograph and Cinematograph must be placed in the same electrical circuit. Experience has shown that the Phonograph must control the action of both instruments. In July 1901, the Gaumont Co. obtained the first patent for such an arrangement.

The Chronophone.

Considerable progress in the technique has been made since then. The experiments which I have the honour to place before you to-night have been prepared under the superintendence of Mons. Gaumont himself, and I think you will agree with me that the problem has been solved in a masterly and convincing manner. Phonograph, Gramophone and Cinematograph have each and all independently been great industrial and commercial successes. Perhaps there is even a brighter future for the "Chronophone," combining as it does two specifically different instruments, the one appealing to the eye, the other to the ear.

First Demonstration of Chronophone and "Filmparlants," or Speaking Cinematograph Films, to a Scientific Society.

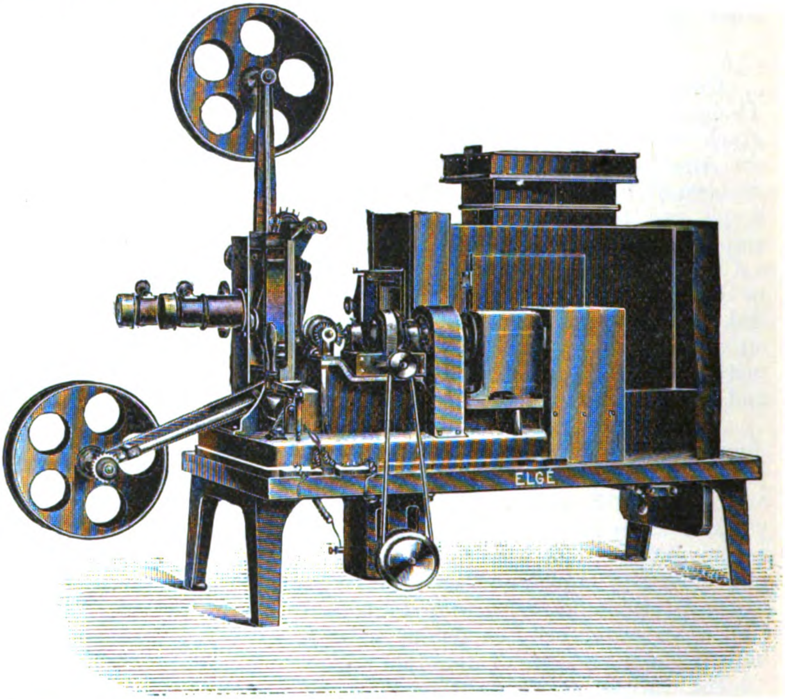
Only a short time ago, just before the close of the year 1910, Thomas Alva Edison himself—who was the inventor of the phonograph—is reported to have said when addressing some members of the American press, that he had worked for many years at the problem of how to obtain a perfect synchronism between the phonograph and the cinematograph. The problem of how to obtain perfect synchronism between the pictures projected on the screen by a Cinematograph and the words spoken by the actors participating in the scene in such a way as to add voice and speech to the reality, and so complete the illusion of life, is one which has occupied the attention of many investigators in both worlds for fifteen years or more. Its practical solution was accomplished by Mons. Gaumont and his collaborators more than a year ago.

Just as the success of Helmholtz followed the prediction of Johannes Müller, so, while Edison himself, that great inventor and pioneer of science, was still experimenting, Mons. Léon Gaumont, and his scientific staff and his collaborators, who had also devoted years of study to the problem, had practically solved the question, as you shall see and hear for yourselves to-night. The "Chronophone" and "Filmparlants," or "Speaking films"—were demonstrated to the Académie des Sciences in Paris on December 27, 1910, when Prof. d'Arsonval had the unique pleasure of seeing and hearing himself making a speech before that august body of Savants.

The earliest arrangement used for synchronisation is shown in Fig. 1. At A is a Gramme's ring divided say into eight sections, each of which is united by a corresponding wire to one of the eight contacts of the "collecteur" C. In A is a Siemens' bobbin through which a current from a battery can be passed when required. Two brushes, D and E, are mounted on an arm which can be rotated round C so as to distribute the current from the battery placed at the right, as shown in the figure. If D and E are at rest all is at rest. If, however, the brush contacts are rotated, the current from the battery is successively distributed in the eight sections of the ring.

If the bobbin is traversed by a continuous current, it will follow exactly the movements of the arrangement carrying the brush contacts. Under these conditions, all that is necessary is to couple up the distributor of the Phonograph and the shaft of the Siemens' bobbin with the Cinematograph, in order that the two pieces of apparatus shall move synchronously. Any serviceable form of motor may be used for the Phonograph.

Since then, other arrangements have been adopted. The following diagrams show the arrangements used for taking the pictures, recording the sound, and for reproducing both pictures and sounds, as shown in to-night's demonstration.



THIS FIGURE SHOWS THE PROJECTOR AND THE DIFFERENTIAL GEARING.

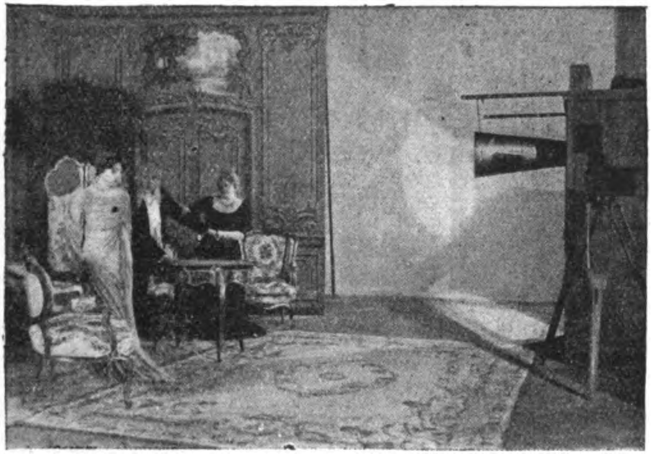
Two motors of identical pattern and of approximately the same power (Fig. 2), for driving the talking machine and the Cinematograph, are actuated from the same source. The armatures of each motor are divided into sections. By means of rings on the elongated axle the corresponding sections of the two armatures are connected together. This arrangement enables the two armatures to act mutually one on the other. Notwithstanding any slight mechanical differences in resistance, perfect synchronism is obtained.

A rheostat introduced into the circuit enables the operator to vary at will the velocity of the motors, even when they are in action.

Experience has shown that the best results are obtained by first setting in action the dynamos and the Phonograph. The Cinematograph is not engaged until a given moment. This can be arranged by placing a clutch between the Cinematograph and its motor.

The automatic engagement apparatus is controlled by a lever connected with the armature of an electro-magnet, which is actuated at a given moment which corresponds with a definitely determined position of the needle in one of the grooves of the disc of the phonograph, which is of the gramophone type.

The next figure (3) shows the Phonograph of a disc pattern, which consists of a turn-table or circular plate moving on a vertical axis, controlled by an ingenious arrangement of .cog-wheels at the end of the axle of the corresponding dynamo. It should also be noted with regard to the recording disc corresponding to that of a Gramophone, that it is a sinusoid which has the same depth throughout. In the Edison method it is a groove of varying depth.



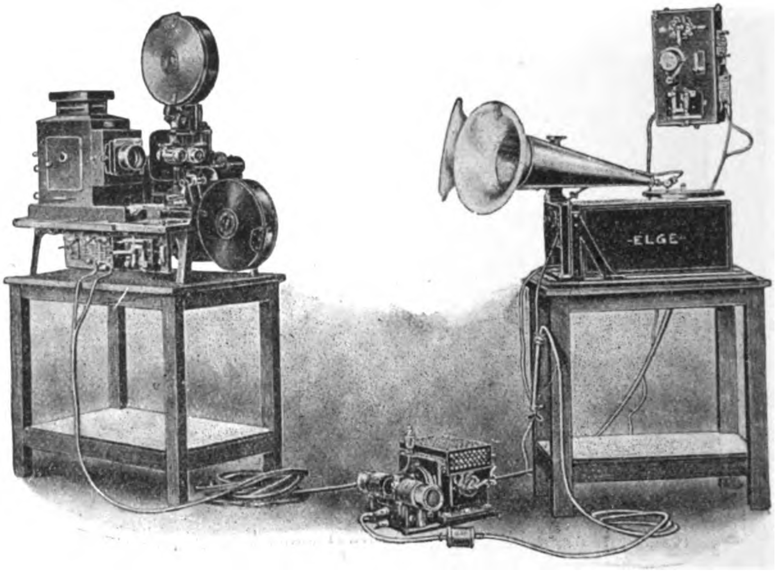
Relation of Phonograph to the Actors in a Set Scene.

There are two contacts by means of which the current can be switched on through the engaging arrangement already mentioned.

The turn-table (Fig. 4) is provided on its under surface with a metal ring interrupted in its continuity, on which both contacts pass.

The upper surface of the turn-table (Fig. 5) carries a projecting finger, which serves to fix the disc of the Phonograph in a position perfectly in relation with the two contacts already mentioned.

If, for example, by any chance there is a discord however small, even a fraction of a second, between the emission of the sound by the talking machine and the movement of the lips of the speaker, there is a special arrangement called the "differential" (Fig. 6) by means of which any want of accord between the Phonograph and camera can be immediately rectified. The differential gearing, which is placed on the shaft between the Cinematograph and its motor and is actuated by means of a special small motor, is provided with a reversing commutator which enables the operator to control the speed of the Cinematograph—either hastening or slowing its movements. The speed of the Phonograph remains constant, so that all correction in speed, in order to synchronise the two machines, is done by accelerating or retarding the speed of projection by means of the Cinematograph. By means of the differential any accidental displacement of the Phonograph needle during the projection can instantly be rectified.



Arrangement for Simultaneous Production of the Cinematograph Film and the Sound Record. The "control board" is above the Phonograph or Gramophone.

Fig. 7 is a schema showing the installation for projection. 1 is a small dynamo with its axis prolonged to the right with its four rings; 2 is the differential gear just described, and which is driven by the small dynamo 2 bis; 3 is the clutch of the electric engaging apparatus 3 bis; while 4 is a spring arrangement for diminishing the shock at the moment the Cinematograph is brought into action; 5 represents the Cinematograph.

Fig. 8 is a photograph of the complete installation for projection as it is arranged for the demonstration in the Royal Institution. In order that the operator may be in close proximity to the Phonograph and to enable him to make sure that everything works well, and to regulate the apparatus, he has before him a rectangular box (Figs. 9 and 10) called "Chef d'Orchestre," but which is practically a "Control Board," fitted with a voltmeter which acts as a speed indicator, a frequency-meter which gives exactly the angular velocity at each instant of the Phonograph – a starting gear with a series of resistances, whereby the Phonograph is set in motion, and a two-way commutator in connection with the differential motor.

Lastly, Fig. 11 shows the complete installation of the whole apparatus, Projector, Phonograph, "Chef d'Orchestre" or Control Board, and a pump for compressed air, by means of which and a special distributor the sound-waves are so intensified in volume that they can be heard in a large auditorium such as the Hippodrome in Paris, when it is filled by six thousand persons.

Some of the original Speaking Films shown at the Royal Institution.

The following speaking films were demonstrated by means of the "Chronophone" by way of showing its applicability to the reproduction of all kinds of vocal sounds. To begin with, vocal sounds produced by animals. (1) A Gallic cock placed on a pedestal, where he crows right lustily so that the whole audience could hear the loud-sounding efforts of Chanticleer, and observe the characteristic movements that accompany his vocalisation. (2) A den of lions with their trainer. The growling of the animals, the dull thud of the iron bar on the floor of the cage are reproduced with startling realism. (3) Illustrates the reproduction of speech and accompanying gestures by a person who is seen speaking through a telephone. (4) Showed a musician playing on a banjo exhibiting the movements of the fingers over the strings, and the fidelity with which musical sounds elicited by the vibrations of strings can be reproduced. (5) Showed a festive gathering of Frenchmen, one of whom gives the toast of "The King," and the company unite in singing "God Save the King." (6) A sailor reproduces in stentorian tones, Kipling's "Ballad of the Clampherdown."

Colour Introduced.

So far, the Cinematograph has been most successfully adapted to reproduce movement in monochrome or in black and white. Many attempts have been made to add the charm and fascination of colour to the pictures, and many processes have been tried and found wanting. The work of W. F. Greene and Frederick E. Ives on still life are well known. Again, to refer once more to the Royal Institution, another novelty was shown by W. F. Greene in January 26, 1906. The trichromic theory of colour vision, so ably supported and elucidated by Professor J. Clerk Maxwell in the Royal Institution and elsewhere, is the basis of all attempts to reproduce colour in the films thrown on the screen. Clerk Maxwell, when demonstrating Young's Theory of Colour Vision, on May 17, 1861, at the Royal Institution, assuming red, green, and blue as primary colours, used troughs containing respectively sulphocyanide of iron, chloride of copper, and ammoniated copper. Clerk Maxwell was fully aware of the necessity for "finding photographic materials more sensitive to the less refrangible rays, whereby the representation of the colour of objects might be greatly improved." The technical difficulties, not to speak of the expense—triple as least that of a monochrome film—of three-colour Cinematography are well known.

Bicolor and Kinemacolor.

W. F. Greene used two screens—red and green. Albert Smith also attacked the problem and he also used only two screens. For a perfect white, however—red, green and blue are required. The patent now known as "Kinemacolor" was introduced to the public in 1908. The effects though beautiful are far from perfect. The pure yellows are wanting, while the true blues and purples cannot be effectively reproduced. The blue, which is lacking, is partly introduced in the green screen, and if the electric arc is used an additional blue tone is introduced. But even this compensates only in a small degree for lack of the blue, while the red and green stand out almost all too vividly.

Mons. Gaumont's Tricolour Method.

For these and other reasons M. Gaumont has reverted to the three-colour theory. The method consists in projecting at the same time on the screen, by means of a cinematograph provided with three objectives, three views of the same subject, each taken through a differently coloured filter-screen. These views are arranged in sequence on an ordinary Cinematograph film. Many attempts have been made in many countries, but so far no single solution is perfect and complete. We have referred to Smith's bicolor method and the kinemacolor and their imperfections.

Difficulties of the Colour Problem.

The following are some of the difficulties of the problem—

1. To obtain a negative film of a sensibility approximately the same for all rays of the spectrum.

2. This sensibility must be such as to permit fourteen views per second as a minimum to be taken.

3. The superposition of the three views must be as perfect as possible on the screen, and reproduced at the same rate at which they were taken.

To facilitate the comprehension of the projection in colours, I show you on the screen—any conventional subject will do (Fig. 12)—e.g., a red triangle, a green square, and a blue circle. Below on the left, the negative film of these figures after development, and on the right side, the same series of three figures as positives. Suppose we place the series of three positives behind the three objectives of the Cinematograph, each one of which is provided with a coloured filter through which the negative was obtained. On uncovering successively the objectives we see on the screen from above downwards, each one of the figures.

The first view obtained with the red filter shows the projection of the red triangle on a black ground, as the filter allows only the red rays to pass.

The second view—a square—obtained through the green filter only allows green rays to pass.

The third view obtained with a blue filter shows the blue circle, the filter allowing only the blue rays to pass. When the three images are superposed, a complete image of the objects in natural colours is obtained.

In practice, however, in order to obtain all tints and tones—and to obtain a pure white—it is indispensable to use filters which allow rays from adjoining parts of the spectrum to pass. Thus, for a red screen, a red-orange filter is used, for green, a green-blue, and for blue, a blue-violet screen is used.

Fig. 2 shows a projection of the designs or figures obtained with the above-named screens ; on the left the negative and on the right the positive. Notice that each view shows grey, which proves clearly that the filter has allowed rays other than the primary colours—red, green and blue – to pass.

Device to Save Expense.

In an ordinary Cinematograph film, each view is 18 mm. high, and is moved at a minimum velocity of 25 centimetres per second with fourteen stops : even with this speed one requires a well-constructed machine in order to have a steady projection and no injury to the films.

The trichromic projection already described, however, requires a speed of projection three times as great, i.e., 75 centimetres per second, which would be very difficult with fourteen stops.

Mons. Gaumont has got over the difficulty by diminishing by one quarter the height of each image, so that three views take the place of two and a quarter, the speed being only about double that of normal.

In this way, at this speed of projection, we may assume—as you shall see for yourselves—that Cinematography in natural colours is practically solved.

Objects projected in Natural Colours.

Amongst the objects projected in natural colours were the marvellous and picturesquely coloured moth *Urania*, and butterflies of the genera *Kallima*, *Morpho*, and *Apatura*. The moth and butterflies themselves were first shown in a strong beam of light, so that the audience could compare the originals with the reproduction. Especially noteworthy was the reproduction of the marvellous iridescence of the wings of the genus *Morpho*. In addition a series of flowers, moving on a rotating disc, were projected in their natural colours.

[W. S.]

WEEKLY EVENING MEETING,

Friday, May 17, 1912.

Sir William Crookes, O.M. LL.D. D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

W. Duddell, Esq., F.R.S. *M.R.I.*

High Frequency Currents.

The subject of High Frequency Currents is an extremely fascinating one, for not only are the effects obtained very beautiful, but also there is an air of mystery about them which lends to their attractiveness. This mystery is in a large measure due to the fact that the ordinary laws governing the flow of electric currents seem to be violated, though on closer inspection this is not really the case.

The subject is now a very large one, and I propose to-night merely to pass in review the different methods of generating high frequency currents and to illustrate their principles experimentally.

Before proceeding it will be well that I should explain what I mean by high frequency currents, and for this purpose I shall have to rapidly survey a certain amount of elementary matter.

If an electric current flows continuously in the same direction through a wire or other circuit, it is said to be a continuous or direct current. If, however, the current flows along the wire first in one direction, then in the other, alternately reversing its direction of flow, it is said to be an alternating current. When the growth and decay of the current and its reversal take place in a regular manner, we speak of the number of times per second that the current goes through a complete cycle of change as its frequency.

This is perhaps best illustrated by considering the motion of a clock pendulum. The little pendulum which I am using to write down its motion on the smoked glass in the lantern has a frequency of two.

In the case of most alternating currents used for electric lighting the frequency ranges between 50 and 100, and in the case of the current supplied to this lecture theatre from Deptford the frequency is 85. These might be thought to be high frequencies, but they are really not at all high. The frequency of the current in the telephone wires—say, for instance, when the electrophone is transmitting a high musical note—might be as high as 1000. The upper limit at which sounds appear to have a clearly musical character is probably of the order of 10,000 per second, and the limit of audibility some 20,000 per second, depending on the individual.

It is possible to produce electric currents having frequencies ranging from 10,000 per second up to many millions, and it is currents between these frequencies that I call high frequency currents. This evening I am going to devote my attention more especially to frequencies in the range between ten thousand and a million, as currents of these frequencies have a large number of practical applications, such as wireless telegraphy, high frequency treatment of diseases, electrical cautery, thermo-penetration, etc.

The methods of producing high frequency currents may be divided broadly into three classes, namely: (1) Alternator methods; (2) methods based on the oscillatory discharge of a Leyden jar; (3) arc methods. Although the alternator method of producing very high frequencies has not had the same general application as the Leyden jar discharge, I will treat it first for two reasons: Firstly, it is somewhat easier to understand; secondly, there seems a probability that the high frequency alternator will be very greatly improved and developed, and soon come into more general application.

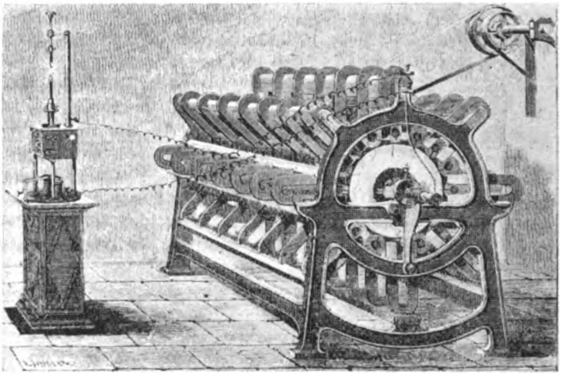


FIG. 1.—ALLIANCE ALTERNATOR.

Alternators were among the first machines invented for producing electric currents on the well-known Faraday principle of magnetic induction. Among the early machines which will illustrate the principle I may mention the Alliance machine, which was largely used in lighthouses, from 1863 onwards. The essential parts were a number of magnets, and wires on bobbins which passed across the poles, in which electric currents were induced (Fig. 1). The frequency of these machines was about 50 per second.

Consider a single wire passing a north pole. A current will be induced in it in a certain direction, according to the well-known principles discovered by Faraday. When the wire passes across the south pole a current will be produced in the opposite direction, so that we have here quite a simple means of producing a current which flows alternately in the wire in opposite directions—that is to say, an alternating current. If a number of magnets are arranged round the machine with their poles alternately north and south, and a wire is caused to move past them, an alternating current will be produced, the frequency depending upon the number of magnets and on the number of revolutions the machine makes per second. For instance, if we have 8 magnets, each of which has, of course, two poles, and if the machine makes 6 revolutions per second, then the frequency will be 48. It will be at once obvious that if we wish to generate a high frequency with a machine such as this, we can obtain it either by using a large number of magnets or a high speed of rotation, or both.

In a little special machine which I have here there are 15 magnets—that is to say, 30 poles alternately north and south—and it runs at 8000 revolutions per minute or 133⅓ revolutions per second, so that frequency in this case is 2000. The peripheral speed of this machine is just over 3 miles per minute—that is to say, the rotating part would roll down to Brighton in about 17 minutes. From this example it will be pretty obvious that if I attempt to obtain high frequencies—that is to say, frequencies well above 10,000—I shall have to run either at enormous speeds or have a very large number of poles. One of the very earliest high frequency machines ever constructed in this country, and following shortly after Tesla's high frequency machine, was made by Messrs. Pyke and Harris for Sir David Salomons, and owing to the courtesy of Messrs. Isenthal, who now own the machine, I am able to show it to you to-night.

The machine ran at 1500 revolutions per minute, but as both the armature and the field magnets are free to move, their relative rate was 3000 r.p.m. or 50 revolutions per second. The number of pairs of poles is 174, so that frequency is 8700.

This machine exhibits very well the peculiar difficulty there is in constructing high frequency alternators, for there are no less than 10 poles in each inch of the circumference of the discs which form the armature and field respectively. When it is remembered that it is not only necessary to get the 10 poles into 1 inch, but also the wires which are between them and the insulation on the wires, it becomes pretty obvious that the constructional difficulties are very great, especially if the machine is required to produce large currents, which naturally require large wires to carry them. As an example of an early machine, this machine possesses many points of interest. Firstly, the method adopted of revolving the two parts in opposite directions is extremely interesting, and has been followed in some of the modern machines. It enables twice the frequency to be obtained from the machine with the same risk of bursting the revolving parts as could be obtained if one part were stationary in the ordinary way and the other part revolved. Secondly, the method of winding—namely, the zigzag winding of the wires on the poles—is of considerable interest. I believe it was first put into a high frequency machine by Tesla in 1889, and it has been adopted in many modern high frequency machines.

At about the same date that Sir David Salomons had his machine constructed, Mr. Parsons constructed for Prof. Ewing at Cambridge University a high frequency alternator in which the high frequency—namely, 14,000—was obtained by using a high speed of 12,000 r.p.m.

At this stage I would explain a second method of constructing an alternator which differs slightly in principle from the first. If, instead of moving the wires across the faces of the magnets, or moving the soft iron cores on which they are wound, the wires be wound on the poles of the magnets and a piece of soft iron moved past the magnet poles, an alternating current will be produced as before with this difference—each time the piece of soft iron bridges two poles and thus forms an easy path for the magnetism, the magnetism will increase through the coils producing a current in one direction. and as it moves away the magnetism will decrease and produce a current in the opposite direction. It will thus be evident that we produce a current first in one direction and then in the other as the piece of soft iron passes each individual pole instead of each pair of poles, so that for a given number of poles and a given speed this type of alternator will produce twice the frequency of the previous type. Alternators depending on this principle are usually called inductor alternators. Many high frequency alternators have been constructed on this principle by using 204 polar projections or teeth and a speed of 35,400 r.p.m. I have obtained a frequency of no less than 120,000 in a machine I constructed in the laboratories of the Central Technical College, but the output of the machine was microscopic.

It is of interest to consider what is the practical limit of frequency obtainable in alternators, and this entirely depends on the limiting speed from the point of view of safety at which the material may be run, and on the spacing of the poles. It is quite well known that for every material there is a limiting peripheral speed above which it will fail. With modern steels and good construction, the safe limit is somewhere in the neighbourhood of 300 metres per second. If we assume that we can get a pole and the wire and insulation into 3 mm., the total number of poles per second will be 100,000, and the limiting frequency would either be 100,000 or 50,000 according to whether the alternator was of the inductor or the ordinary type. If it were possible to build two discs rotating in opposite directions, having the above peripheral speeds, then the above frequencies would be doubled. The limited width available for the pole and copper leads to great difficulties if any considerable output is required.

Turning to modern high frequency machines, we have the alternator designed by Mr. Alexanderson, with which frequencies as high as 200,000 have been obtained. Owing to the courtesy of Mr. Alexanderson, I am

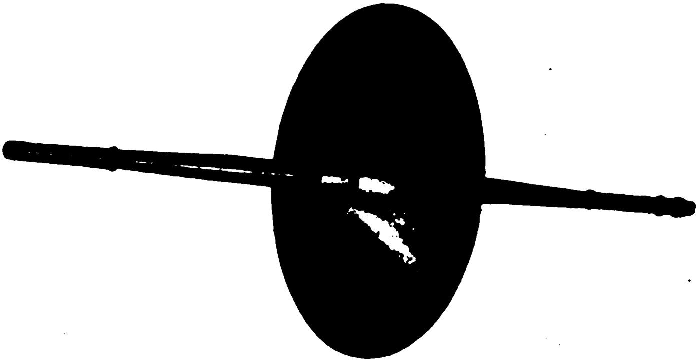


FIG. 2.—DISC OF ALEXANDERSON'S 100,000 ~ ALTERNATOR.

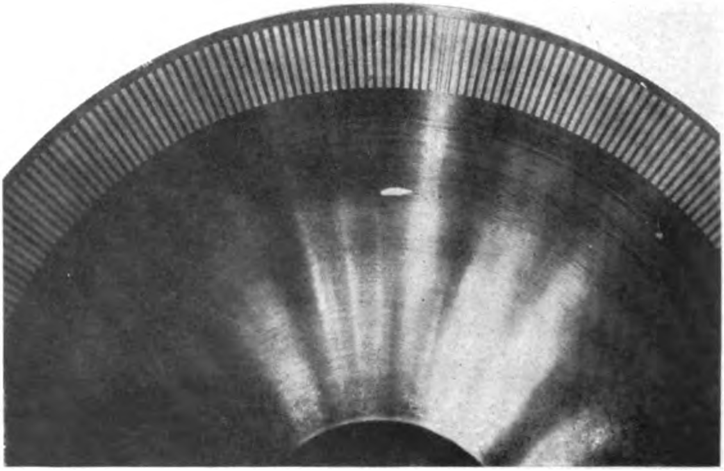


FIG. 3.—DISC OF ALEXANDERSON'S 100,000 ~ ALTERNATOR.

able to show you some photographs of a 100,000 frequency machine and give you some details as to its construction. The alternator is

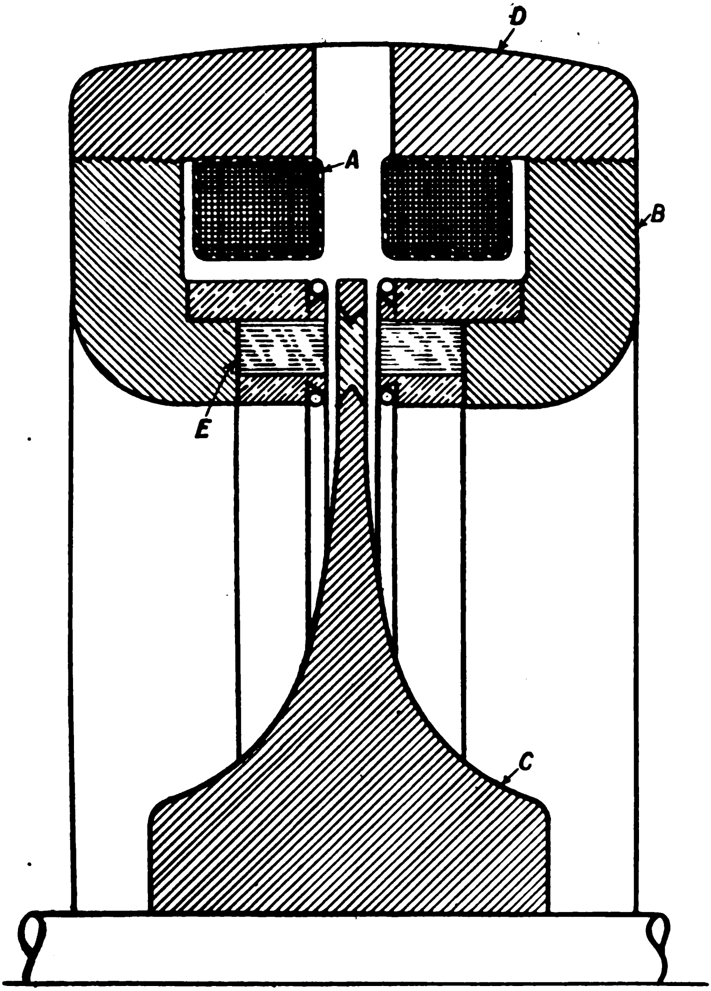


Fig. 4.—Section of Alexanderson's 100,000 ~ Alternator.

of the inductor type. The rotating part consists of a steel disc having 300 slots cut round its edge so as to leave 300 steel teeth. Each of these steel teeth acts in turn to close the magnetic circuit between two of the poles of the stationary part of the machine. In order to reduce the air friction on this disc, which is run at the very high speed of 20,000 r.p.m., the spaces between the teeth have brass plugs riveted into them so as to render the surface of the disc perfectly smooth. Fig. 2 shows the complete disc and its shaft, and Fig. 3 a portion of the disc on a larger scale.

The frame of the machine F, Fig. 4, contains two field coils A. The magnetic lines of force pass through the frame through the part B and the laminated polar projections E. According to whether there is an iron spoke or the non-magnetic material between the polar projection E, the magnetic flux will be maximum or minimum, and will consequently go through a complete cycle of change as each iron spoke passes the pole tips; the wires in which the current is induced are wound zigzag fashion around the pole tips. With 300 teeth and a speed of 20,000 r.p.m., a frequency of 100,000 is obtainable. A view of the complete machine is shown in Fig. 5, the driving motor being on the right, the gearing in the centre and the high frequency alternator on the left. An output of some 2 k.w. is obtainable. In order to get still higher frequencies without rendering the spacing excessively small so that there is no room for the wires between the polar projections on the stator, another form of winding has been adopted in which instead of using 600 polar projections on the stator only 400 are used. This winding is illustrated in Fig. 6.

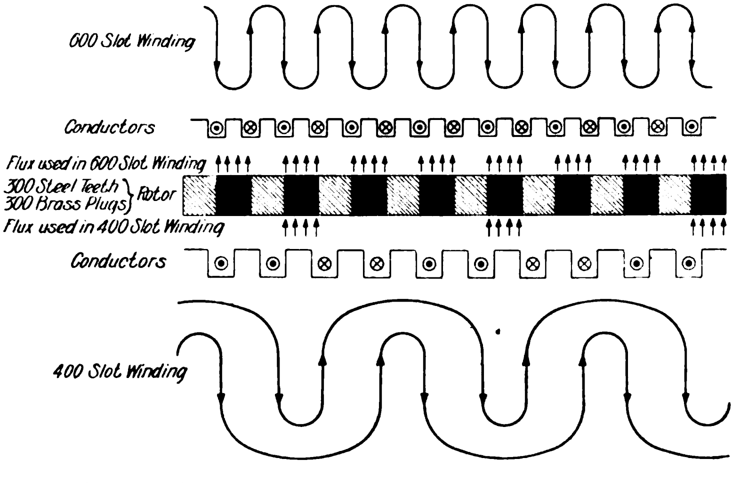


Fig. 6.—Winding Diagram, Alexanderson's Alternator.

With this latter type of winding, using 800 polar projections, a

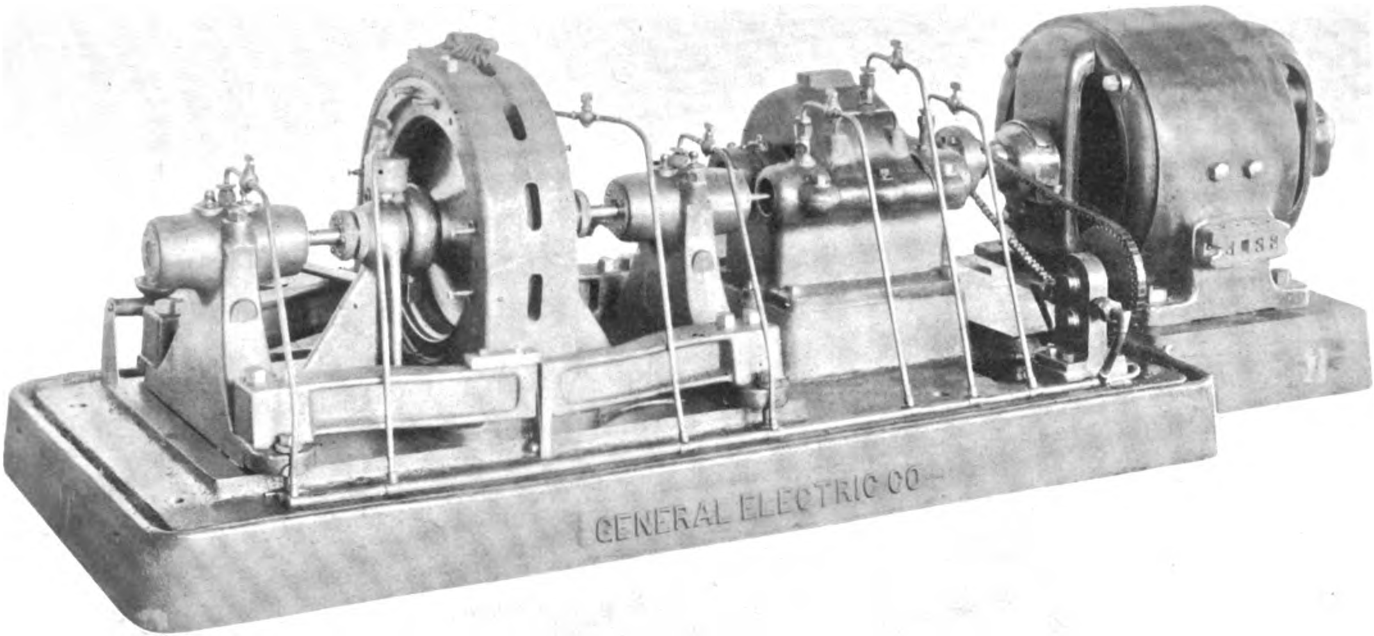


FIG. 5.

frequency of 200,000 has been obtained by Mr. Alexanderson. It is interesting to note that when running at this very high speed the gyrostatic action is so considerable that a long flexible shaft has to be used so that the disc spins in its own plane like a top. Auxiliary bearings are provided close to the disc, which, while allowing sufficient freedom for the disc to spin in its own plane, yet prevent excessive bending of the shaft when the disc passes through its different critical speeds when being speeded up. To give some idea of the high speeds at which these discs run I may mention that the speed of the edge is some 12 miles per minute, so that if they could be used as wheels for a motor car, the journey to Brighton would only occupy about 4½ minutes.

A high frequency alternator on quite a different principle is due to Dr. Goldschmidt. The principle of the machine is at first sight a little difficult to understand. I have mentioned in Sir David Salomon's early machine that the two discs forming the field and armature were rotated in opposite directions. It is possible, however, to produce a rotating field electrically without the disc moving at all. When this is accomplished the frequency of the machine depends as before upon the relative speed of the rotating field and the armature.

Considering for simplicity a machine having only two poles so that it gives one complete cycle per revolution, and suppose I supply the fixed part or stator with an alternating current of 100 frequency and so connect it that it will produce a rotating field (we will assume the alternating current to be three phase for the sake of simplicity), if now I rotate the rotating part in the opposite direction to that of the rotating field I shall have induced in the rotor an alternating current whose frequency will depend on the relative speeds. For instance, if the speed of the rotor is such that with a stationary field it would give 100 frequency, then as my field is actually rotating in the opposite direction at 100 frequency the actual frequency produced will be 200 per second. Using this 200 per second frequency by means of another machine running at the same speed one could increase it to 300, and so on indefinitely. This would, however, lead to a very complicated machine, and Dr. Goldschmidt has arranged matters so that instead of requiring a number of machines the successive additions to the frequency can all go on in the same machine.

The simplest way to explain this is to suppose that the alternating current at 200 frequency obtained from the rotor is passed back into the stator of the same machine so as to produce a rotating field of 200 frequency without in any way interfering with the original rotating field of 100 frequency. This rotating field of 200 frequency will induce an alternating current of 300 frequency in the rotor, which could in turn be transferred back to the stator and produce a rotating field having 300 frequency, and so on.

This is what Dr. Goldschmidt calls the electrical method of transferring the currents from the rotor to the stator. He has also devised a magnetic method which is simpler. Turning again to our two pole machine whose stator is supplied with current at 100 frequency, there will be induced in the rotor a current of 200 frequency. Now, if the rotor be short-circuited there will be produced in it a rotating field having 200 frequency, which will induce in the stator a current having 300 frequency, which in turn can be short-circuited and will induce in the rotor a current having 400 frequency, and so on. I have spoken of the machine as if the rotating field was produced by a three phase current. A single phase field may be looked upon, theoretically, as two rotating fields in opposite directions, one of which will have no effect as far as we are concerned, and therefore a single phase machine will do just as well. Further, it is not really

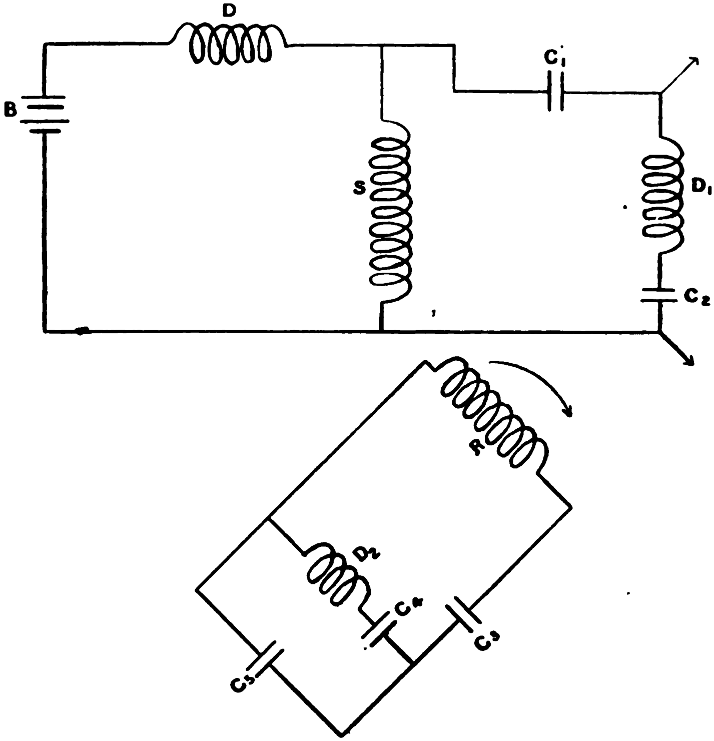


FIG. 7.—GOLDSCHMIDT'S ALTERNATOR.

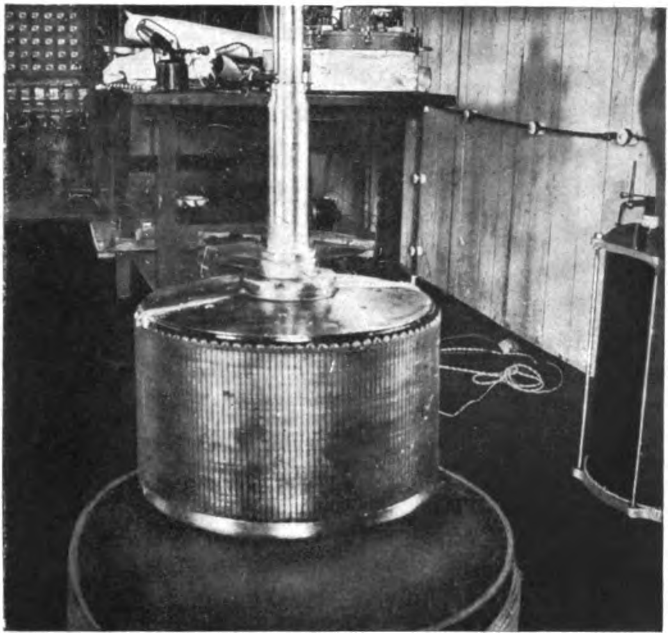


FIG. 8.—ROTOR OF GOLDSCHMIDT'S ALTERNATOR.

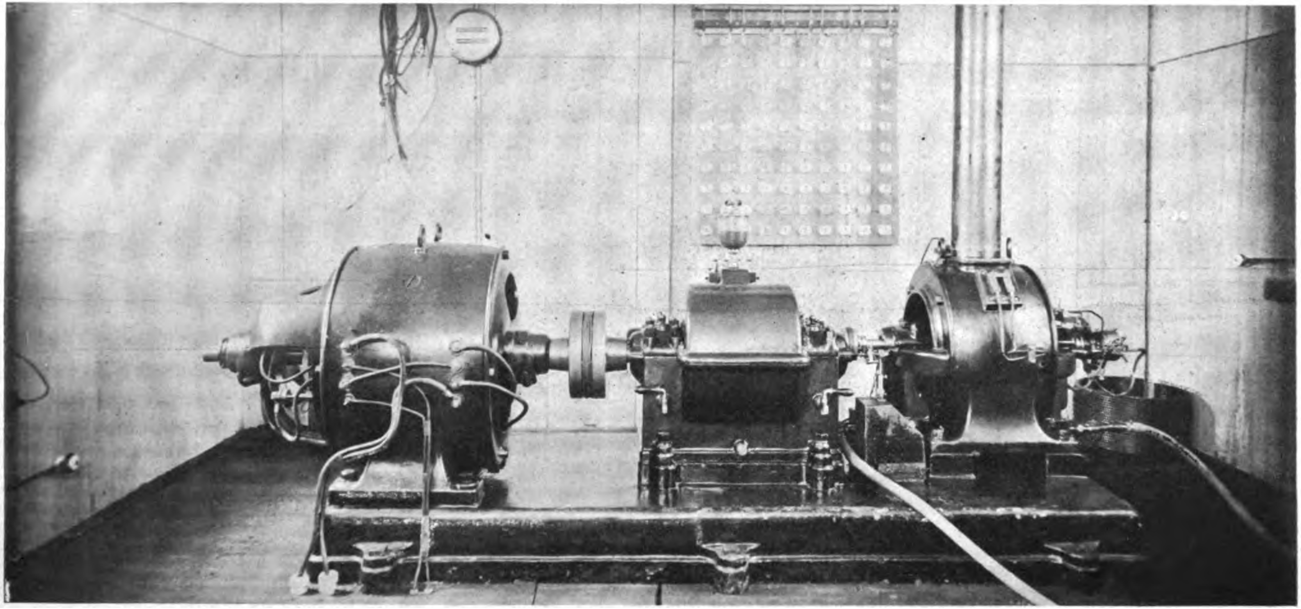


FIG. 9.—GOLDSCHMIDT'S ALTERNATOR COMPLETE.

necessary to supply the stator of the machine with an alternating current. If it is excited with a direct current it will induce an alternating current in the rotor which will react on the stator, and backwards and forwards in the same way as I have already described.

It would seem at first sight as if by this means an indefinitely high frequency could be very easily obtained. There are, however, certain practical difficulties. The actual connections used for a fourfold magnetification of frequency are shown in Fig. 7.

The stator S is excited from the battery B through the self-induction D. This produces in the rotor R an alternating current of the frequency of say 100. This current flows through the condenser C_3, the self-induction D_2, and the condenser C_4. This circuit being in resonance for 100 frequency, consequently the rotor behaves as if short-circuited for this frequency. The field produced by this 100 frequency will react on the stator, and induce in it a current of 200 frequency which passes via the condenser C_1, self-induction D_1, and condenser C_2. This circuit is also adjusted to be in resonance for the frequency 200, and consequently the stator behaves as if short-circuited for this frequency. There is in turn induced in the rotor R a current of 300 frequency which passes via C_3 and C_5, again adjusted to resonance. This induces in the stator a current of 400 frequency which, as circuit $D_1 C_2$ is not in resonance, for this frequency can be taken out to do useful work from the points *a* and *b*. By this method Dr. Goldschmidt has obtained some 12 k.w. at a frequency of 30,000, and at high frequencies a lesser output. Owing to his courtesy I am able to show you some slides of the actual machine. Fig. 8 is the rotor, and Fig. 9 the complete machine.

The present position of the generation of high frequency currents by means of alternators may be resumed by saying that it is now practically possible to generate by means of a machine alternating current up to and over 200,000 frequency. At frequencies of 100,000 several k.w. are obtainable, and at the lower frequencies machines can be built for greatly increased outputs. Where large amounts of power at high frequencies are required such as in wireless telegraphy, the question is now becoming a commercial one as to the efficiency of the different types of alternator, their cost and their reliability. Is it possible to run these machines continuously day in day out and to supply power at high frequencies? If so, how much does it cost per kilowatt hour to produce this high frequency power, taking into account not only the coal consumption, but also the wear and tear on the machine and all the other running and capital charges?

LEYDEN JAR DISCHARGES.

Turning next to the second method of producing high frequency currents—namely, by the discharge of a Leyden jar—it was quite early inferred by Helmholtz and Henry that the discharge of a Leyden jar did not always flow in the same direction, and we owe to Lord Kelvin the mathematical investigation of this subject, and to Feddersen its experimental demonstration. If I charge a Leyden jar, or any other condenser, and allow it to discharge through a very high resistance, a current will flow, which gradually dies away as the jar discharges. If, however, I discharge the condenser through a sufficiently low resistance, it will be found that the current continues to flow in the same direction after the jar is completely discharged, and then charges it up in the opposite direction, the current gradually getting smaller. When the jar is charged to a certain extent in the opposite direction to its original direction it starts to discharge again, this time in the opposite direction. The current again continues to flow after the jar is discharged, and charges it up in the original direction. This cycle of operation is repeated, the energy of the charge in the jar gradually getting less and less, until it dies away. As a simple analogy, a pendulum, or a weight hanging on a spring, when displaced oscillates, the oscillations growing smaller and smaller until they die away. This is so well known that I need not go any further into the matter. The frequency of these oscillations is determined by the capacity of the condenser—that is to say, the quantity of electricity it can hold when charged to 1 volt and the self-induction of the circuit—the capacity of the condenser corresponding to the weakness of the spring, and the self-induction to the mass of the weight in our spring and weight analogy.

It is quite easy to obtain by this method almost any frequency—for instance, anything between 1 and 100,000,000 are comparatively easily obtainable, and I have even demonstrated frequencies as low as ½ in this lecture theatre.

The fundamental difference between these high frequency currents and those generated by alternators is that they die away—that is to say, that the amplitude gradually decreases and comes to zero. The condenser then has to be re-charged, and the process repeats itself. Now, a single discharge of this sort is not of much practical use. It is necessary to charge the condenser and repeat the discharges in rapid succession if any useful work is to be done. Many methods have been proposed for this purpose. Some of the early successful experiments were those of Tesla, shown in this theatre on February 3, 1892.

The condenser is allowed to discharge through a circuit which consists of the necessary self-induction and a spark gap in series. Now, a spark gap possesses the property of acting like a safety valve which will stand a certain steam-pressure, and will then suddenly open and let the steam out freely. The spark gap will stand a certain electrical pressure, and then will open and let the electricity pass. When once the electrical pressure or voltage has been raised to a sufficiently high value to break down the spark gap, the spark gap changes from being an insulator which prevented the flow of electricity to being a comparatively good conductor, and it remains conducting in the ordinary way until the current finally ceases. I shall have some more to say later on the subject of the change of conductivity of the spark gap. Returning to our circuit containing our condenser, spark gap and self-induction, if the condenser be charged to a sufficiently high voltage the spark gap will break down, a spark will take place, and during the time that the gap is conducting oscillations will take place. In order, therefore, to produce the repeated sets of discharges of the condenser, all we have to do is to repeatedly charge the condenser to a sufficiently high voltage, and this is easily accomplished by charging it from a transformer connected to an ordinary low frequency alternator. For instance, with the ordinary 85 frequency current available I can obtain one discharge each time the condenser is charged either positively or negatively—that is to say, 170 discharges per second, each of which discharges consists of a series of high frequency oscillations.

This is the usual method of producing high frequency currents, and is the method used by Tesla for his most brilliant experiments, shown in this lecture theatre twenty years ago. It will be evident. however, that the high frequency currents obtained by this means differ fundamentally from those obtained from an alternator; for instance, with an oscillation frequency of 1,000,000 per second, such as I am using, and 50 oscillations for each discharge of the jar, the whole discharge will last $\frac{1}{20000}$ of a second. As we are only obtaining 170 discharges per second, there is something like a 200th of a second between the end of one discharge and the beginning of the next. which is a comparatively long time, about 100 times as long as the time the oscillations last. Inventors have been struggling, and with some success, to close up these gaps.

It is obvious that if the discharges can be caused to take place with greater rapidity by using a higher frequency alternator to charge the jars, the gaps will be closed up, and this is the method originally used by Tesla, and which is now being used in many of the wireless transmitters, though in this case there is another reason for using a greater number of discharges per second. For instance, the Marconi Company, in their musical spark transmitters, use a frequency of some 300—that is to say, some 600 discharges per second—and the Telefunken Company as many as 1,000 discharges per second.

Mr. Marconi has described in this lecture theatre the method he uses in his trans-Atlantic stations. In this case, instead of charging the Leyden jar or condenser from an alternator, it is charged from a high voltage battery, and the sequence of discharges is produced by means of a special form of spark discharger, which consists of a disc fitted with studs round its edge. When this disc is rotated the studs pass between the balls of the spark gap, and, corresponding to the passage of each stud, a discharge takes place. If the disc is driven at a very high speed a large number of discharges per second are obtainable. Taking the trans-Atlantic case, where the frequency of the oscillations is about 50,000 per second, and, assuming that there are 50 or 60 oscillations in the aerial for each discharge, the total time the oscillations in the aerial last is just over $\frac{1}{1000}$th of a second. As the spark frequency is about 600 per second the oscillations are therefore practically joined up, and there are no completely idle intervals between them.

I now come to another class of method which still depends upon the discharge of a condenser. If I have a body like a pendulum or a spring capable of oscillations, I can make it oscillate by pulling it away from its position of rest and then letting it go. I can also make it oscillate by giving it a blow or shock, and I can make it oscillate by applying to it a number of impulses at properly timed intervals. All three of these methods are employed in the production of high frequency currents. The first is the simple discharge of the Leyden jar without any auxiliary apparatus; the third is the method usually employed in wireless telegraphy for obtaining and maintaining a long series of oscillations in the aerial by tuning it and loosely coupling it to the Leyden jar circuit in which the discharge takes place. The second method also involves a discharge of a condenser. In this case the spark gap is so arranged that it suppresses the oscillations of the condenser—that is to say, it breaks down, allows a large rush of current, and then becomes insulating and stops anything further from happening. To illustrate this I have constructed a little valve which works with water A vertical tube of water represents the condenser, and this is connected to the valve. The height of the water in the tube is gently increased, which corresponds to increasing the quantity of electricity in the condenser, and the pressure of the water, represented by its height, corresponds to the voltage on the condenser. When the pressure of water is sufficient the valve suddenly opens, corresponding to the spark gap breaking down, and allows the water to flow out from the vertical tube, so discharging it. As the valve only works in one direction oscillations cannot take place. We have thus a model of what is called a quenched spark, which goes out directly the condenser is discharged. A discharge of this type can be caused to act either by magnetic induction or otherwise on a circuit consisting of a condenser and a self-induction. The effect of the rush of current when the jar discharges is to give the second circuit a shock and set up oscillations in it.

It might be thought at first that in this method of producing oscillations there was no relationship between the nature of the swing in the first, or discharge, circuit and the frequency of the oscillations in the second circuit. Without going into the theory of coupling, which would take me too far, I would point out that for the best result there is a relationship, and this can be easily seen if one considers the question of starting oscillations in, say, a short and a long pendulum. The long pendulum will be set swinging easier with a blow which comes on and off comparatively slowly, whereas the short pendulum will require a short, quick tap to start it swinging.

This method of shock excitation, due to Max Wien, is employed by the Telefunken Company in their wireless transmitter, and also in their therapeutic apparatus; a special type of spark gap being used consisting of large flat metal plates, a very short distance apart, the object being to suddenly cool the spark to cause it to go out, and the gap to become insulating again at the earliest possible moment.

A spark gap of this type connected to a condenser and self-induction can be supplied in the ordinary way, when the frequency of the spark will naturally depend upon the frequency of the alternator.

If a Leyden jar or condenser be charged from a battery through a high resistance, it will take a certain time to charge up; and if there be also connected to the condenser a discharge circuit containing a spark gap, when the condenser is sufficiently charged a discharge will take place through the spark gap, the condenser will then start to charge again and the operation will be repeated. I employed this method some years ago to work an induction coil. In this case, as I was only using low voltages under 1000 volts, the spark gap was replaced by a vacuum tube, and the self-induction in the oscillatory circuit was the primary of an induction coil. The connections and data are shown in Fig. 10. By suitably adjusting the pressure in the vacuum tube and the resistance in series with the condenser, it is easy to obtain a rapid series of sparks. If voltages of the order of 1000 volts direct current were generally available, this might form a very convenient method of working a coil.

It will be noted that the spark frequency is not, in this case, determined by an alternator, but depends upon the time the condenser takes to charge to a sufficiently high voltage to break down the spark gap. By making the resistance small, and the supply voltage high, the condenser will charge more quickly. It is quite easy to cause the discharges to follow one another very rapidly by this means.

If an attempt be made to employ the successive discharges of a condenser when charged from continuous current, a difficulty is encountered if large powers are dealt with. This difficulty seems to reside in the properties of the spark gap, which tends to become completely conducting, or, what is technically called "arcs," when large powers, say 10 k.w. or over, are employed.

Mr. Galletti set himself to get over this difficulty by employing a number of such circuits, each with their own spark gap, and connecting these together so that they would all help to produce the oscillations.

Of the methods proposed by him I will only refer to one which I have seen in operation. The principle of it is illustrated in Fig. 11. It consists of splitting the condenser into two parts, one of which—C_0—is common to all the discharge circuits. The action of this condenser is not very easy to follow, but it certainly produces the required result, if the conditions are properly adjusted, by causing the discharges in the various circuits to take place in regular sequence. For it is quite easy to show that if the common condenser is not employed, then the circuits tend to all discharge at the same time, so that the addition of extra circuits, instead of filling up the gaps between the discharges, tends to accentuate the discharge, at certain times leaving the intermediate periods idle. By combining a number of circuits and using the high voltage continuous current supplied by the Moutier-Lyons transmission—which was at a voltage of some 40,000 volts when I saw the experiments—Mr. Galletti was able to deal with many k.w., the spark frequency being very high, some 10,000 per second. Corresponding to each of these sparks there is a series of oscillations, so that in this case the oscillations must be

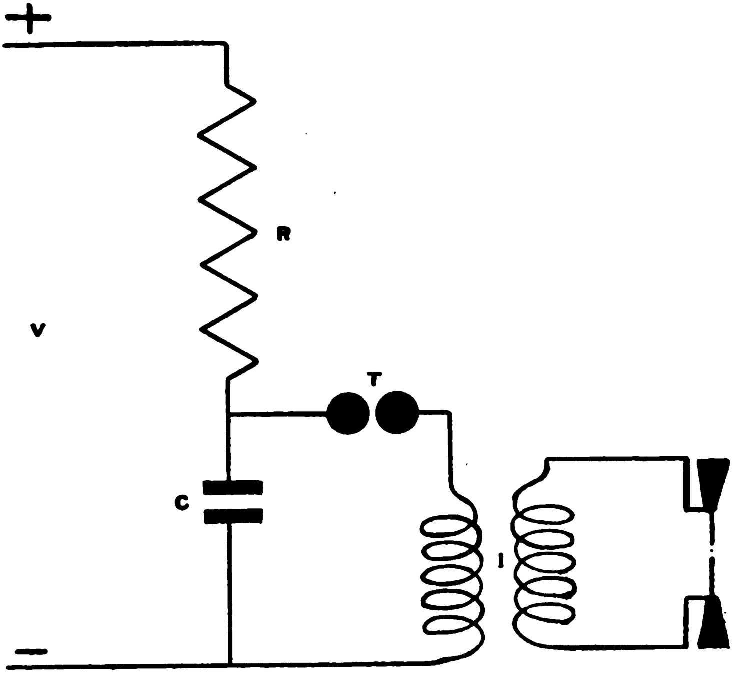


FIG. 10.

I, induction coil; T, vacuum tube; V, about 900 volts;
R, 5000 to 100,000 ohms; C, about 5 mf.

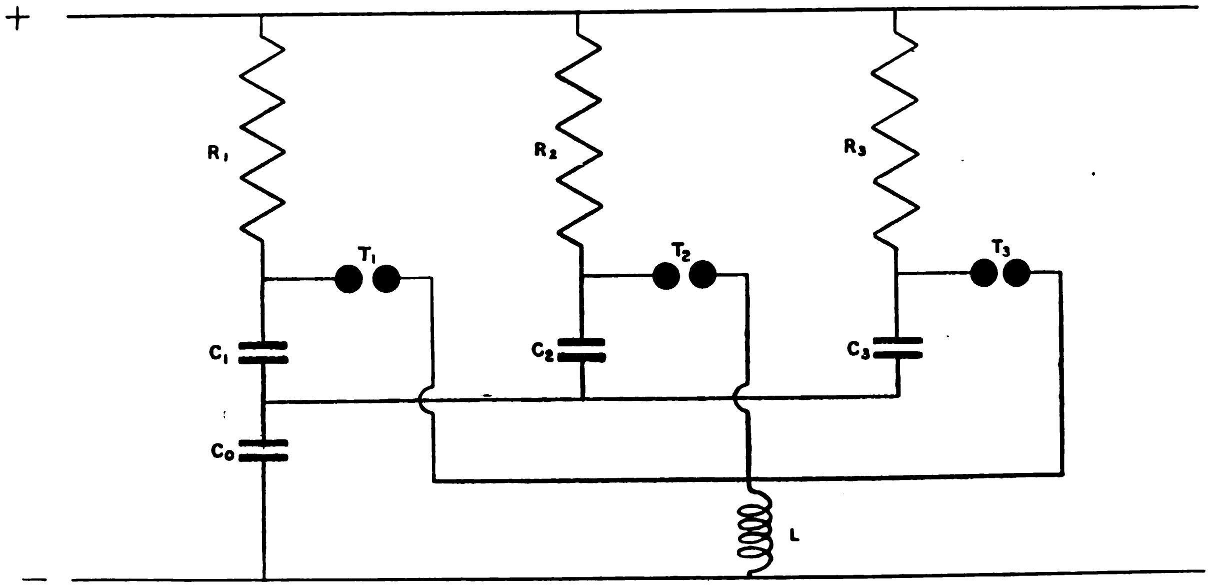


FIG. 11.—GALLETTI CONNECTIONS.

completely joined up, and the result almost the same as if an alternator were employed.

Mr. Galletti informs me that he has been able to employ still larger numbers of sparks per second, up to 100,000. In this case one arrives at the interesting result that the oscillations can be maintained by impulses from the condenser circuit every other oscillation, or even each individual oscillation. This should maintain a true alternating current, such as is produced by an alternator, with no waste intervals.

To experimentally illustrate the principle, I have arranged two circuits according to Mr. Galletti's method. As I have not at my disposal a very high voltage—in fact, I have only 900 volts—I am using vacuum tubes T_1 T_2 in place of the spark gaps, as by adjusting the pressure I can make them discharge regularly at these low voltages.

In order to demonstrate that the two circuits I am using really discharge in regular sequence and not simultaneously, I am using a low discharge frequency of about 1 per second. This is obtained by giving the condensers C_0 C_1 C_2 each a capacity of about 5 mf., and using 100,000 ohms for R_1 and R_2 in series in each circuit. By short-circuiting the common condenser C_0 the sequence of successive discharges is at once destroyed, and the condensers of the two circuits tend to discharge simultaneously. When I reduce the series resistances the discharge rate is greatly increased, until it is no longer possible by eye or ear to realize the frequency. I have, by experiments with the oscillograph, determined that the discharges still take place in regular sequence.

ARC METHODS.

I now come to the third method—namely, the arc method of producing high frequency currents. If an ordinary direct current arc is produced between solid carbon or metal electrodes, it is unstable – that is to say, it cannot be burnt unless a resistance is placed in series with it. The reason for this will be at once evident if we examine the curves in Fig 12, connecting the potential difference and the current. It will be noted that if the current through the arc is increased, due to any cause, the potential difference between its terminals decreases. The result of this decrease in potential difference is to encourage a still larger current to flow. This larger current further decreases the potential difference, and, consequently, still further increases the current, the current tending, therefore, to become infinitely great. If, on the other hand, the current be decreased, the potential difference between the terminals of the arc tends to increase, which has the result of tending to still further decrease the current, causing the current to become zero, so that an

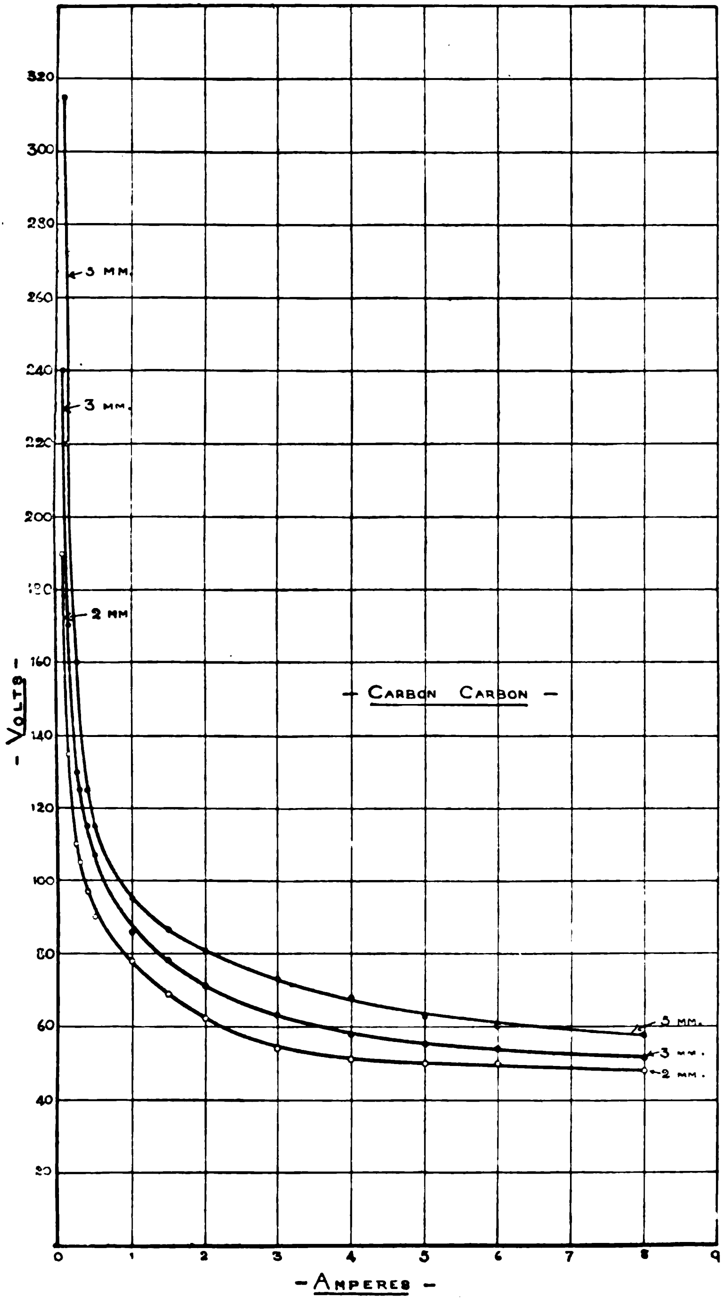


FIG. 12.—CHARACTERISTIC CURVES FOR ARC BETWEEN CARBON ELECTRODES.

arc by itself is essentially unstable. Of course, in practice, the current can be kept at any desired value by putting sufficient resistance in series with the arc, so that when the current is increased the potential difference between the terminals of the arc and the resistance taken together increases, and the circuit as a whole becomes stable. If such an arc be shunted with a capacity and self-induction in series (see Fig. 13) oscillations will be set up in this shunt circuit, the frequency of the oscillations being mainly determined by the periodic time of the shunt condenser circuit, which may be calculated from the well-known Kelvin formula.

When the oscillations are taking place the oscillatory current flows through the arc, and consequently tends to increase and decrease the arc current. Any increase in the current tends to make the vapour column of the arc larger, and any decrease makes it smaller, so that the vapour column of the arc pulsates in size. This pulsation in the size of the vapour column moves the air in the neighbourhood of the arc, and, if the frequency is suitable, produces sound; hence this experiment is known as the musical arc. By varying either the capacity or the self-induction the frequency of the oscillations can be varied, and hence the pitch of the notes given out by the arc, and by this means a tune can even be played on it.

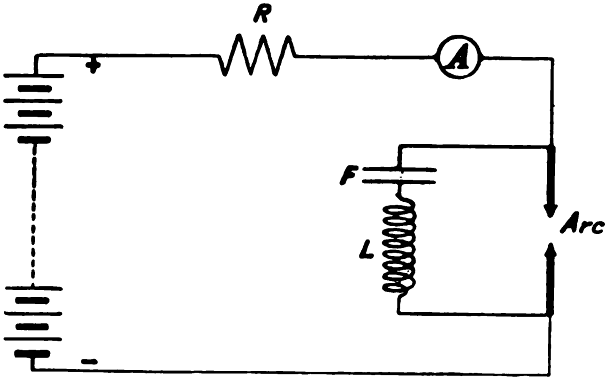


FIG. 13.—MUSICAL ARC CONNECTIONS.

The frequency of the currents produced when using an ordinary arc between solid carbon electrodes is comparatively low, at the most a few thousands per second. The reason for this is that the characteristic of the arc—that is to say, the relationship between the potential difference and the current—depends to some extent on how quickly the current is varied. If the current is varied very quickly, then an increase of current is accompanied by an increase in the

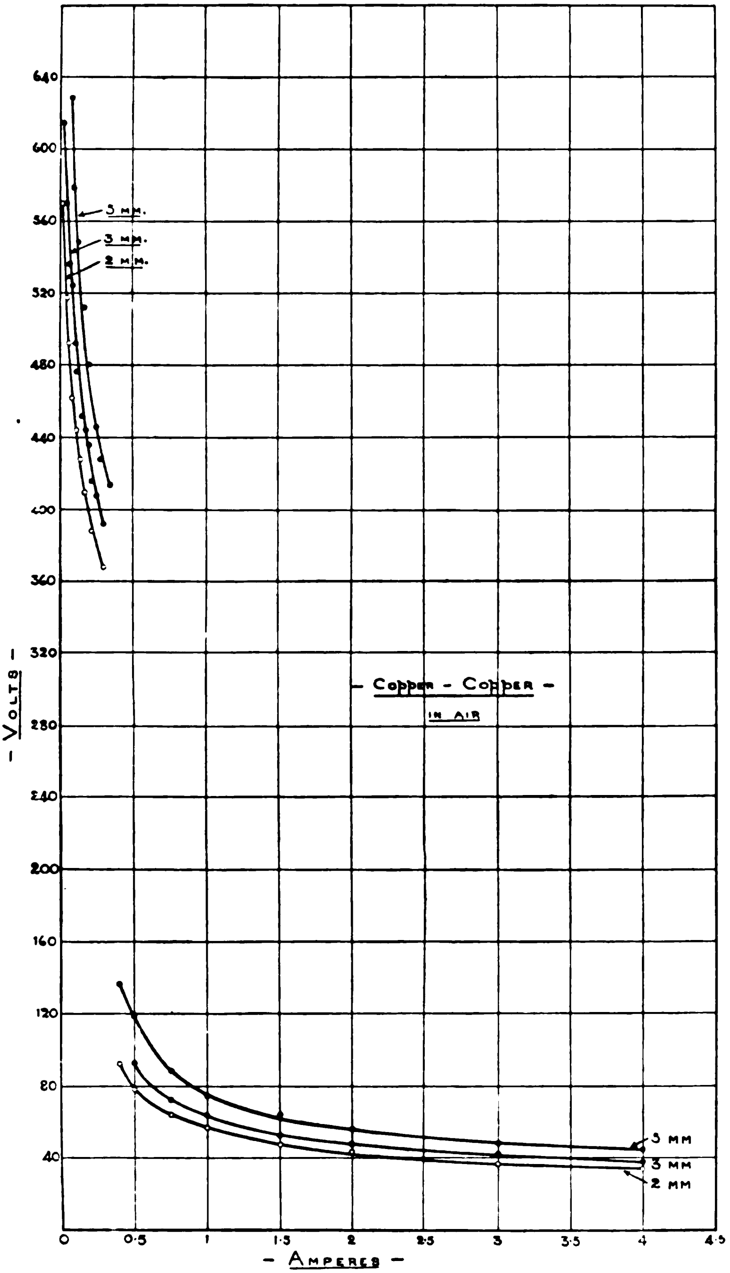


Fig. 14.—Characteristic Curves for Arc between Copper Electrodes.

potential difference; the arc is no longer an unstable conductor, and oscillations cannot be produced.

There are various ways of rendering the arc unstable, even when the current varies rapidly. One of these consists in using very small currents. For instance, metal arcs using currents under one-half of an ampere are extremely unstable, and remain so even under high frequencies (see Figs. 12 and 14, which give some curves I have obtained for arcs between carbon and copper electrodes respectively).

Another method is to energetically cool the electrodes; for instance, to make one of the electrodes consist of a vessel containing water, or squirt a jet of water through the arc. The third method, due to Mr. Poulsen, consists in placing the arc in hydrogen, or some hydrogen compound, such as coal-gas or alcohol vapour. By this means high frequency oscillations can easily be produced up to about a million per second, and at the lower frequencies of, say, 100,000, considerable powers can be dealt with.

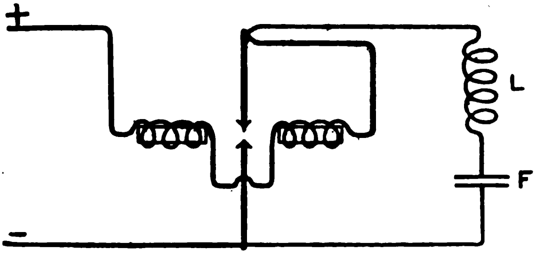


FIG. 15.—POULSEN ARC CONNECTIONS.

To render the arc still more unstable, Mr. Poulsen places it in a strong transverse magnetic field (Fig. 15). With an apparatus of this sort I am informed that some 30 k.w. or more can be dealt with by a single arc, and if several arcs are placed in series large powers can be transformed. Fig. 16 is a photograph of a 12 k.w. arc generator.

If the apparatus is properly adjusted the high frequency currents produced are very nearly true alternating currents, such as are obtained from an alternator.

The only difficulty I find in working the apparatus is to maintain the frequency absolutely constant, as it appears that the frequency depends to a small extent on the current through the arc and on the arc length. In the actual apparatus employed for wireless telegraphy the electrodes are kept in rotation so as to ensure a greater constancy in the arc length.

It must be remembered that if the arc is burnt in coal-gas instead of the electrodes burning away, carbon is actually deposited, and the carbon electrode increases in length.

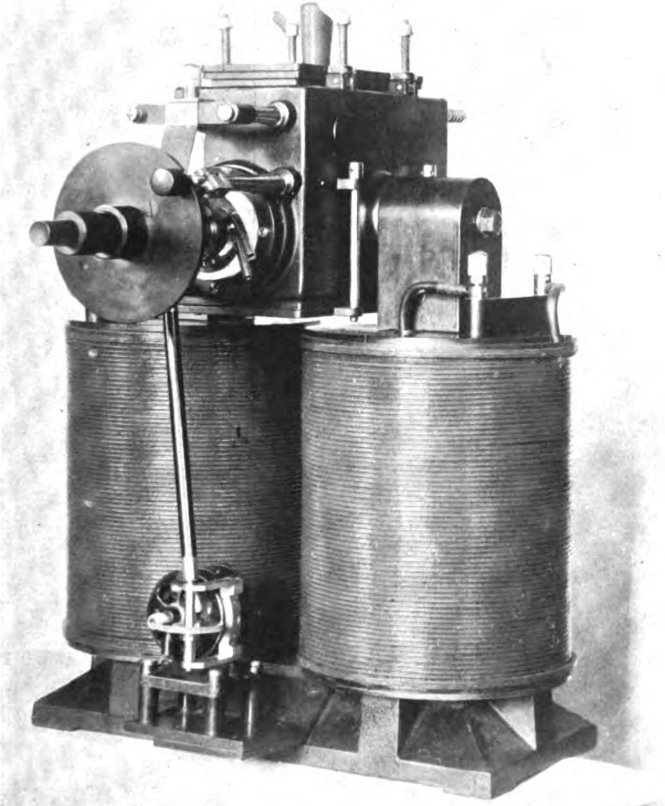


FIG. 16.—12 K.W. ARC GENERATOR.

In using a generator of this sort for wireless telegraphy, messages have been transmitted and recorded from a distance of about 1000 miles at the speed of over 100 words per minute.

A large number of modifications of the arc method have been produced, practically all of which consist in changing the nature of the electrodes or the surroundings of the arc, so as to try and make it still more unstable at high frequencies and large currents. Most arcs seem to lose their instability after the current has passed a certain limit. For this reason there is generally a best working current for any arc. If it is required to deal with more power than one arc can transform, it is better to put the arcs in series.

A trick which is very useful and which enables two arcs to be burnt in parallel as far as the mains are concerned, and to be in series as far as the oscillatory circuit is concerned, is illustrated in Fig. 17. This is at times very convenient, as sufficiently high voltage direct current generators are not always available.

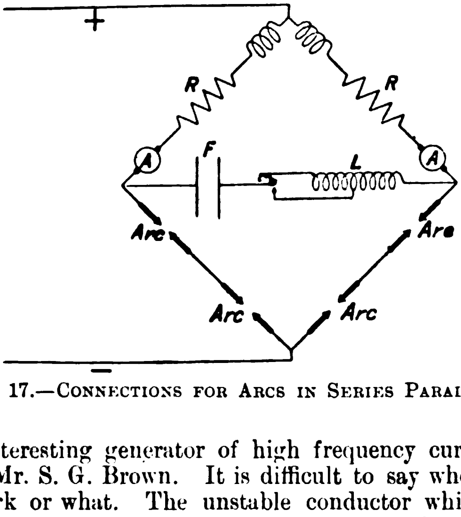


Fig. 17.—Connections for Arcs in Series Parallel.

A very interesting generator of high frequency currents is that invented by Mr. S. G. Brown. It is difficult to say whether it is an arc or a spark or what. The unstable conductor which takes the place of the arc consists of an aluminium wheel, on the edge of which a copper block rests (Fig. 18). The wheel is kept rotating, and the contact is shunted by a condenser and self-induction. This arrangement produces high frequency currents quite easily, but I have never been able to make it deal with any large powers.

In conclusion, it is of interest to review in general terms the present position of the three main methods of producing high frequency currents. The most important application at the moment for high frequency currents is wireless telegraphy. For this purpose frequencies in the range between 30,000 and 1,000,000 are required. A few kilowatts high frequency energy is generally sufficient for practical purposes at the higher frequencies. At the lower frequencies, which are mainly used on long-distance working, large amounts of power are required. In fact one of the great problems in long-distance wireless telegraphy is to produce several hundred kilowatts of high frequency energy with certainty and regularity, and at a reasonable price.

The relative advantages of high frequency alternating current, sometimes called continuous oscillations and groups of oscillations, is not a matter that I can enter into to-night. It is sufficient to point out that if continuous oscillations are employed they can always be cut up into groups by means of an automatic device should this be required. Neglecting the relative advantages of the continuous oscillations and the oscillations which are sent out in groups with spaces between, I have simply to compare the three methods from the point of view of the amount of energy they are able to successfully and regularly handle.

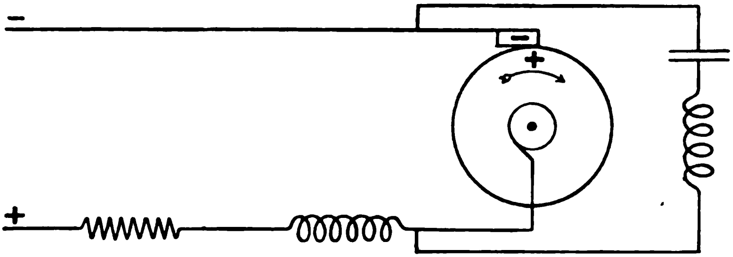


FIG. 18.—CONNECTIONS FOR S. G. BROWN'S WHEEL.

The alternator method, as far as the information at my disposal goes, has never yet been applied to very large powers. There is no doubt that it is from many points of view an ideal method.

The spark method has been successfully employed over a wide range of frequency and up to very large powers, and there is no doubt that it is capable of satisfactory operation. The spark method used with very high frequency sparks and continuous current, as proposed by Mr. Galletti, is still in its experimental stage. With the rough apparatus I saw working a year ago, a considerable amount of power could be dealt with successfully, and there seems no reason why the principle should not be extended so as to deal with still larger powers. The practical difficulties may be great, but I think they should be able to be overcome. This, however, is entirely a matter for experiment.

The arc method is in a much more advanced condition. A large amount of work has been done, and practically all the details have been worked out. There is no doubt that fairly large powers can be successfully generated and utilized.

The position is therefore a very interesting one. No one of the methods seems at the moment to have so many advantages as to make it *the* one method above all others for producing high frequency currents. The final decision as to which is the best will probably in the end be determined by the regularity with which the apparatus works, and by that governing factor in all commercial enterprises—the cost of production.

[W. D.]

WEEKLY EVENING MEETING,

Friday, May 24, 1912.

Sir William Crookes, O.M. LL.D. D.Sc. F.R.S., Honorary Secretary and Vice-President,, in the Chair.

A. D. Hall, Esq., M.A. F.R.S.

Recent Advances in Agricultural Science—The Fertility of the Soil.

From an ordinary commonsense point of view the fertility of the soil is best defined as that property for which a man pays rent—the property which causes some land to let for £2 or £3 an acre, whereas the adjoining land may be dear enough at 10*s*. With the causes of this fertility I do not propose to deal at any great length this evening more than to indicate that it is the outcome of a very complex series of factors, among which we can enumerate the actual supply of plant food in the soil, its mechanical texture as conditioning the movements of water, and the particular micro-fauna and flora inhabiting the soil, for upon these lower organisms depend the facility with which the material contained in the soil will become available for the nutrition of the plant. For the purpose of the present argument it will be sufficient to fix our attention upon the amount of nitrogen in the soil as the main factor determining fertility, because, in the first place, nitrogen is one of the necessary and most expensive elements in the nutrition of the plant, and, secondly, because its amount in the soil is subject to both gains and losses from causes which are more or less under the control of the farmer. The other essential elements which the plant has to draw from the soil—for example, phosphoric acid and potash—are only subject to slight losses by solution in the drainage water, and cannot be added to except deliberately by the action of the farmer; but in the case of nitrogen we have in addition to the small stock of combined nitrogen in the soil the vast store of free gaseous nitrogen with which both soil and plant are in contact. We may take it as settled nowadays that the plant itself can make no use of nitrogen gas, but must draw combined nitrogen in one of its simpler forms, such as nitrates or ammonia, from the soil. Among the bacteria of the soil, however, there are two great groups, one of which is capable of breaking up compounds of nitrogen and setting free the element as gas, whereas the other can take free gaseous nitrogen from the atmosphere and bring it into a combined form. Which of these two groups will be more active depends upon the conditions prevailing in the soil, and goes far to determine both its current fertility and the length of time during which it will be capable of bearing crops.

The question of the duration of the fertility of the land under continual cropping has excited much attention of late, chiefly because the United States has begun to take alarm about the reduced production of some of its most fertile lands, as, for instance, the old prairie lands of the middle West—a reduced production which, amongst other causes, has helped to set in motion a stream of migrants from the United States to the newer lands of the Canadian North-West. In the development of agriculture three distinct stages may be observed. In the first place, we may have a process of pure exploitation of the initial resources of the soil, when the farmer is to all intents and purposes mining in its fertility. This is the process which, in the main, has been going on in America, and, indeed, in all the newer countries which have been opened up to agriculture during the last two centuries. Not all virgin soils are rich, and the system of cropping alternately with wheat or maize which prevails over so much of North America has reduced great areas of the land in the eastern States to such a poverty-stricken condition that it has been allowed to go derelict. In the great plains, however, where the first settler found four or five feet of black soil, containing nearly half per cent. of nitrogen, the land has kept up its productivity almost unimpared for nearly a century. If we suppose the black soil only extended to a depth of three feet, and contained three-tenths per cent. of nitrogen, both limited estimates, there would still be 30,000 lbs. of nitrogen per acre - that is to say, nitrogen enough for five hundred crops larger than the American farmer has been accustomed to win from that land —and yet in less than a century such soils are beginning to show signs of exhaustion. The farming of the kind just described is destructive; but in the older lands of the west of Europe, which have been under cultivation for something like a century, a conservative system has been devised which is capable of keeping up the productive power of the soil, though not, perhaps, to a very high pitch. Perhaps the best example of this may be seen in the Norfolk four-course rotation prior to the introduction of. artificial fertilizers. In this system a turnip crop, which was either consumed on the ground or converted into manure and so returned to the soil, was followed by barley in which clover was sown, and the clover, which also got back to the soil, was followed by wheat. The farming covenants prevented the sale of anything more than barley and wheat grain, and the meat that was produced by the consumption of the turnips and hay. Thus but a small proportion of the nitrogen taken out of the soil by the crop left the farm; the rest was returned and used over again, although considerable losses of gaseous nitrogen occurred during the making of the dung. Both losses, however, were more than replaced by the nitrogen which the clover crop gathered from the atmosphere during its growth. At any rate, we find that under such a conservative system of farming the productivity of the land remained pretty constant at about a level of twenty bushels to the acre from the time of Queen Elizabeth down to the beginning of the nineteenth century. This conservative farming about 1840 began to give place to the third stage in the development—intensive farming, rendered possible by the discovery of artificial fertilizers and the cheap freights which brought foreign fertility in the shape of cheap feeding stuffs to the soil of this country. By these means the average production of the land of the British Isles has been raised from the twenty-bushel level to something over thirty bushels, and the most intensive farmers reach an average level at least 25 per cent. higher. In their case the soil has become practically a manufacturing medium transforming the nitrogen and other fertilizing materials added to it into crops, giving nothing to those crops from its original stock, and indeed up to a certain point gaining rather than losing fertility with each year's cultivation. The inner history of these three stages in agriculture may be followed by a consideration of certain experimental plots at Rothamsted. We may begin with the experimental wheatfield which is now carrying its sixty-ninth successive crop of wheat. One of the plots has been without manure throughout the whole of that period. The production, which fell steadily for the first ten years, has since that time remained so constant that the slow falling off which we still believe to be taking place is disguised by the fluctuations due to season. The average yield is about twelve bushels to the acre, almost exactly the average yield of the wheat lands of the whole world. Unfortunately samples of soil were not taken at the very outset, but if we begin with the earliest analyses that were available in 1865 and draw up a balance-sheet for the nitrogen, we shall find that the removal in the crop is almost exactly balanced by the small amount that comes down in the rain and the decrease that has taken place in the amount of nitrogen in the soil. There are, however, other losses of nitrogen not brought into account; some is washed away by drainage water every year, and a further small unestimated amount gets removed as

EXPERIMENTS ON WHEAT, BROADBALK FIELD, ROTHAMSTED.

Average Produce of Grain, first 8 years (1844–51) and the successive 10-year periods 1852–1911.

Plot.	Manure.	Averages over							
		8 years, 1844–1851.	10 years, 1852–1861.	10 years, 1862–1871.	10 years, 1872–1881.	10 years, 1882–1891.	10 years, 1892–1901.	10 years, 1902–1911.	60 years, 1852–1911.
		Bush.	Bush.	Bush.	Bush.	Bush.	Bush.	Bush.	Bush.
2	Farmyard Manure	28·0	34·2	37·5	28·7	38·2	39·2	35·1	35·5
3	Unmanured	17·2	15·9	14·5	10·4	12·6	12·3	10·9	12·8

weeds. As these losses do not appear in the balance-sheet we must conclude that some recuperative action is at work keeping up the stock, though the process is not sufficient wholly to make up for the removals in the crop. The results of this plot show two principles at work—the tendency of the land under an unchanging system of farming to reach a position of equilibrium when the only variations in the crop are those brought about by seasons; and, secondly, that regeneration of the nitrogen stock in the soil is possible by natural causes alone.

We may now turn to one of the other plots which receives an excess of farmyard manure each year, the manure supplying about 200 lbs. of nitrogen per acre, whereas the crop only takes away about 50 lbs. Naturally the land in this case increased in fertility, but after twenty or thirty years another position of equilibrium was attained at a level of about 36 bushels per acre, after which. despite the continued additions of manure, the crop again did not vary except as the result of exceptional favourable seasons. If we now consider a similar balance-sheet for this plot, we find that the additions of nitrogen are balanced neither by the removals in the crop nor by the accumulation of nitrogen in the soil; indeed half of the nitrogen applied is unaccounted for. The soil has been getting no richer for the last twenty or thirty years, and the greater part of the nitrogen is wasted, doubtless because bacterial action sets the nitrogen free as gas. Here, then, we see another principle illustrated, that in very rich land the wasteful agencies are so speeded up as to prevent any continued accumulation of fertility out of the unused residues of the manures put on. Higher fertility means a higher level of waste, and this explains the rapidity with which the very rich virgin soils lose their fertility when they are put under arable cultivation. In this Rothamsted plot, the soil of which still

BROADBALK WHEAT FIELD.

Nitrogen in Soil, lb. per acre.

In Soil, 1865.	In Soil, 1904.	Gain or Loss in 39 years.	Added in Manure.	Added in Rain.	Removed in Crop.	Unaccounted for.
Plot 3—Unmanured.						
2850	2290	– 560	..	150	600	– 110
Plot 2—Farmyard Manure.						
4470	4970	+ 500	7800	150	1990	– 5460

contains less nitrogen than the less rich virgin soils of the prairies, three times as much nitrogen are wasted every year as is converted into crop, and the same or an even greater rate of wastage must attend the conversion of the rich virgin soils into land growing a succession of cereal crops.

We may now turn to another plot on the same field to illustrate the recuperative actions of which I have spoken. This is a part of the field that has been running wild since 1881, when the wheat it carried was not harvested but allowed to seed itself. A very few years sufficed to eliminate the wheat, which was unable to maintain itself against the competition of the weeds, and the land now carries a miscellaneous vegetation consisting mostly of grass. A soil sample was taken at starting, and when compared with another sample taken twenty-three years later showed that in the interval the land had gained nitrogen at the enormous rate of 92 lbs. per acre per annum. Making every allowance for possible errors in sampling and analysis, the accumulation of nitrogen is in marked contrast to its steady depletion in the equally unmanured arable land alongside. Now, the difference between the two plots lies in the fact that on the land running wild the vegetation is never removed, but allowed to die down naturally. Hence not only is the nitrogen taken out by the crop returned to the soil, but also a large stock of carbonaceous matter assimilated from the atmosphere, and this carbonaceous matter furnishes a bacterium present in the soil, *Azotobacter chroococcum*, with the source of energy which will enable it to fix atmospheric nitrogen. *Azotobacter* is equally present in the soil of the unmanured wheat plot; but, as there the crop is removed and only a little root and stubble left behind, there is but little carbonaceous matter for the *Azotobacter* to work upon, and a correspondingly small fixation of nitrogen, sufficient only, as we have seen, to repair the casual losses by drainage and weeding. This plot gives us a clue to the source of the vast accumulations of nitrogen in the old prairie soils. Vegetation alone, however long continued, cannot increase the stock of nitrogen in the soil; there is only a circulation of the initial stock removed by the plants and then put back when the plant dies *in situ*. But if the conditions are also favourable to the development of *Azoto-*

BROADBALK FIELD, ROTHAMSTED.

Land allowed to run wild. Nitrogen in Soil, lb. per acre.

	In Soil to 27 inches.		Added by Rain.	Gain in Soil per annum.
	1881	1904		
Broadbalk	5910	8110	90	92

bacter, this organism derives from the carbonaceous part of the plant residues the energy it requires for the fixation of nitrogen, and a steady addition to the original stock goes on. We have found *Azotobacter* present in all these rich black soils, from both South and North America, the Russian Steppes, and similar virgin land in all parts of the world, and again we also find an abundance of lime, one of the other necessary factors for the growth of *Azotobacter*. Virgin soils are not necessarily rich; there are miserably poor ones, though they have equally carried some sort of vegetation for hundreds, indeed thousands, of years. They have remained poor because some of the other factors upon which depend the development of *Azotobacter* are lacking. With this far-reaching conclusion in sight, we have naturally tried at Rothamsted whether we could not bring about a similar heaping up of nitrogen in the soil by simply adding to it a carbohydrate containing no nitrogen, such as starch or sugar. In plots the experiment is perfectly successful, and accordingly we selected one of the plots in the barley field which was in a very nitrogen-starved condition, because it had been manured for fifty years only with mineral fertilizers containing no nitrogen, and treated half the plot with sugar at the rate of a ton to the acre, the other treatment of the two halves of the plot being alike. To our surprise, the half receiving sugar gave a miserable crop, much below the non-sugar half, for four years in succession, and a bacteriological examination of the soil showed that *Azotobacter* had not increased in response to the sugar, but that the number of merely putrefactive organisms had gone up greatly. These facts led Dr. Hutchinson to surmise that we had been putting on the sugar at the wrong time of year, in early spring or winter, some time before the barley was sown, when the soil is cold. Now *Azotobacter* is comparatively

HOOSFIELD BARLEY.

Effects of Sugar (or Starch) on the Amount of Produce.
Plot 4 O. Complete Minerals.

Year.	Sugar (or Starch) applied.	Total Produce of Barley.	
		Without Sugar.	With Sugar.
		lbs.	lbs.
1906	Spring	2485	*
1907 †	,,	3578	3249
1908	,,	1820	1404
1909	,,	2563	2261
1910	Autumn	2082	2502
1911	,,	1244	1915

* Very small crop, not weighed.
† Starch applied instead of Sugar in 1907.

inactive at low temperatures, and the sugar was probably being wholly taken by the *Streptothrix*, etc., which are less affected by cold. As these organisms must also obtain nitrogen, they were robbing the barley of the small stock available in the soil, and so bringing about the observed reduction of crop. A change was accordingly made in the time of application of the sugar, which was put on as soon as the barley had been harvested when the soil still retained its summer heat, and the change was immediately followed by an increase in the succeeding barley crops, as compared with the non-sugar plots, that was as marked as the deficiency had been previously. This illustrates the many pitfalls which attend investigations in agricultural science. Under laboratory conditions one can define the issue sharply, but as soon as the experiments are extended to the open ground and living plant, so many extraneous and unsuspected factors come into play, that what is popularly called a conflict between theory and practice often becomes apparent.

We may now take a more complex example from the Rothamsted plots to illustrate what I have called the conservative systems of farming. One of the fields is farmed on a four-course rotation of turnips, barley, clover, wheat, but over half the field the clover is replaced by a year's bare fallow. Further, if we confine our attention to the one plot which never gets any nitrogen, but only mineral fertilizers, it is again divided at right angles into plots from which the turnip crop is wholly removed, and others on which it is returned, as so often occurs in practice when the turnips are eaten off *in situ* by sheep.

NITROGEN PER CENT. IN SOIL OF AGDELL FIELD, ROTHAMSTED.

The plots receive Mineral Manures, but no Nitrogen.

	Fallow.		Clover.	
	Roots Carted off, 13/14.	Roots Returned, 9/10.	Roots Carted off, 15/16.	Roots Returned, 11/12.
1867	0·1224	0·1240	0·1327	0·1380
1874	0·1147	0·1238	0·1241	0·1321
1883	0·1161	0·1228	0·1329	0·1383
1909	0·1159	0·1195	0·1347	0·1498
1852–1903				
Wheat, average	31·2 bush.	32·2	32·2	35·1
Clover, ,,	..	..	41·0 cwt.	47·7
Swedes, ,,	151·0 cwt.	268·0	160·0	187·0
Barley, ,,	22·1 bush.	28·7	24·5	34·5

The above table shows the average yield on these plots and also the changes in the nitrogen content of the soil at different dates.

There are two possible recuperative actions to make up for the crops removed—the *Azotobacter* working upon the carbonaceous matter returned in the turnip crop, and the growth of the clover, for that crop as we know gathers nitrogen from the atmosphere by means of the organisms living in the nodules upon its roots. When neither clover is grown nor are the roots put back the soil is slowly losing nitrogen ; when either occurs singly a fair production is maintained without loss of soil nitrogen ; when both take place during the rotation the average removals from the soil become as high as thirty-five bushels per acre of wheat, thirty-four of barley, and more than two tons of clover hay, yet the soil is if anything gaining rather than losing in fertility, though no extraneous nitrogen is being introduced.

Thus we see that we can maintain indefinitely a production of over four quarters per acre of wheat, and their equivalent in other crops, by natural agencies alone without recourse to external supplies of nitrogen, provided we repair the small annual losses of phosphoric acid and potash, which of course cannot be regenerated from the atmosphere. But such a level of production, though equal to the average of the British Isles, is below that which a modern intensive farmer must attain, and the lesson that we have to bear in mind is that at a higher level, say that of five quarters of wheat, the wasteful actions of which we have spoken are increased out of all proportion. Hence we have to add as manurial nitrogen not merely the difference between that contained in the extra quarter of wheat, but four to five times that amount to repair the waste, and so on to an even greater extent if we still further raise the fertility and the production.

The essential wastefulness of highly intensive agriculture such as must be forced upon the race as the new countries fill up is a serious question, but the prospect of reducing the waste is not entirely hopeless. The losses, as we have seen, are due to bacteria, which attack the nitrogen compounds with liberation of nitrogen gas, the particular bacteria doing this being most active in soils rich in organic matter, until at Rothamsted we only recover in the wheat crop about one quarter of the nitrogen applied in the heavy dressing of farmyard manure. The problem before us is to bring the soil bacteria under control, and we already begin to see in various ways that such control is not impossible. For example, the researches of Drs. Russell and Hutchinson at Rothamsted have already proved that in one simple way we can so rearrange the micro-fauna and flora of the soil as to obtain a much higher duty from the reserves of nitrogen therein contained.

It is too long a story to enter upon now. I can only briefly say that by putting the soil through various processes of partial sterilization, such as heating or treatment with antiseptics, like chloroform or toluene, we can eliminate certain organisms which keep in check the useful bacteria in the soil—i.e. the bacteria which break down the nitrogen compounds to the state of ammonia, a form assimilable by plants. Heating the soil to the temperature of boiling water for two hours will double its productivity, and such a process has been found to be commercially profitable in the case of greenhouse soils. The market growers of cucumbers and tomatoes make up an exceptionally rich soil of virgin loam and stable manure, but in a few years such soil, while still enormously rich on analysis, becomes incapable of growing a profitable crop. The partial sterilization processes of which I have been speaking restore and even enhance its fertility by eliminating the injurious organisms, and we learn from the detailed results that after such treatment a much larger percentage of the soil-nitrogen is recoverable in the crop than normally prevails in untreated soil. At present the processes have not been extended to the open field, but progress is being made in that direction, and give some promise of a method by which ultimately the unseen fauna and flora of the soil will be domesticated, the useful races encouraged, and the noxious repressed, just as the larger flora and fauna have been reduced to our service since the days when primitive man first turned from hunting to agriculture.

[A. D. H.]

WEEKLY EVENING MEETING,

Friday, May 31, 1912.

DONALD WILLIAM CHARLES HOOD, ESQ., C.V.O. M.D. F.R.C.P.,
Vice-President, in the Chair.

PROFESSOR HOWARD T. BARNES, D.Sc. F.R.S.

Icebergs and their Location in Navigation.

ORIGIN OF THE NORTH ATLANTIC ICE.

THE icebergs met with in the North Atlantic each year are almost entirely derived from Western Greenland. The interior of Greenland is covered by a large ice-sheet forming an enormous glacier, which gradually moves outwards, meeting on its journey mountains and islands which form a fringe of varying width. This mountainous belt is penetrated by deep fiords, through which the ice passes towards the sea. As the huge ice-sheets are forced into the sea they are broken off and set adrift as bergs. The "*calving*," as it is called, may take place in a number of ways.

Von Drygalski distinguishes three classes of bergs; those of the first class are the most massive of all, and separate with a sound like thunder from the entire thickness of the glacier front. They result from the buoyant action of the water as the glacier pushes out into the deep water. They usually regain their equilibrium after rhythmic oscillation, and float away in an upright position. Bergs of the second class are broken off under water from time to time. They rise and often turn over before they gain equilibrium, displaying in this way the beautiful blue colour of the lowest layers of ice. Bergs of the third class form almost continuously, and consist of large and small fragments which separate along the crevasses and fall into the sea.

The size of the pieces of ice set adrift varies very much; but bergs 60 to 100 feet to the top of their walls, with spires and pinnacles from 200 to 250 feet high, are most often found. The length of such an average berg would be from 300 to 500 yards. The depth of these masses under water is variously given as from seven to eight times the height, but this is not always the case. It is possible to have a berg as high out of the water as it is deep below the surface, since the submergence depends entirely on mass and not on height. It is possible to find bergs with a pinnacle rising high out of the water, but offering little weight to the mass below. The highest berg in the Arctic which has been recorded had pinnacles 1500 feet high. Bergs are produced all the year round throughout the entire extent of the coastline of Greenland, but the huge masses which push out into the open sea arise either on the west coast between Disco Bay and Smith Sound, or on the east coast south of the parallel of 68°. Besides the icebergs formed from the Greenland glaciers, a few come around Cape Farewell from the Spitzbergen Sea, and some may be traced from Hudson's Bay.

MOVEMENT OF ICE FROM THE ARCTIC REGIONS.

The Labrador current flows southward along the coasts of Baffin Land and Labrador. The average rate is from 10 to 36 miles per day, but occasionally it ceases altogether.* As soon as free the icebergs find their way into the Arctic current, and float gradually southward. The journey is by no means an easy one, and few bergs survive. There are many mishaps, such as grounding in the Arctic basin with ultimate breaking up, stranding along the Labrador coast, where destruction takes place, and falling to pieces entirely in the open sea. Only a small percentage ever reach the Grand Bank and the routes of the transatlantic liners, so many delays attend their journey. It is well known that many bergs seen in any one season may have been produced several seasons before. Taking the Labrador current as 10 miles per day, a berg once formed and drifting freely would make the journey southward in from four to five months. The difference in time of two bergs reaching a low latitude may cover a period of one or two years even when these start on the same day, so devious are the paths into which chance may direct these floating masses. Under-currents affect the largest icebergs, and frequently they are seen to move backward against the wind and surface water. Extensive field ice offers an obstruction to the movements of the bergs, hence the number met with from one season to another must depend on the mildness or severity of the previous summer in the North.

THE LABRADOR CURRENT.

No part of the oceans of the world is of so much interest to mankind as this cold Arctic current. It brings down the cold of the North to temper the heat of the Tropics, and thus tends to equalise the temperature of the world. It is the home and feeding-ground of the world's greatest supply of fish food, and supports more marine life than any other part of the world. It conveys each year southward the greatest menace to the navigator in the form of huge icebergs, and it influences the entire eastern coast of Canada. In spite of all this there has been little study of this current. Why, may we ask, have the Governments of the world neglected to obtain scientific data, and why have they neglected to supply a thorough hydrographic survey of this region? I trust this state of affairs may be soon rectified.

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* U.S. Hydrographic Report, 1909.

DANGER FROM ICEBERGS.

To the navigator the presence of ice is a constant menace. Its movement, often fairly rapid by wind and current, makes its position always uncertain. A ship may see immense fields of ice, which another passing over the same locality a few days afterwards may never encounter. Only those who have stood on the bridge of an Atlantic liner with the officers on a dark night in the ice-track can appreciate the anxiety of those tireless men, who know that collision with even a small floating ice mass means damage to the ship. The small masses called "*growlers*" are often of great danger. They float low in the water, and leave little above to be seen by the look-out. The Arctic ice is of great solidity and is of irregular shape. It presents frequently sharp edges which can cut the plates of a ship, shear off rivets, or drive a hole through the bottom as readily as a steel knife. The game of chance is played by every big ship that speeds through the ice-track at night or in a fog.

FIELD-ICE AND ITS DISTRIBUTION IN THE GULF OF ST. LAWRENCE DURING THE WINTER.

Icebergs are not alone in causing an obstruction to navigation in the Labrador current. Field-ice, which may extend over wide areas, presents great difficulties. This ice is salt water frozen in the bays and inlets along the shore, as especially in the Gulf of St. Lawrence. Immense fields are formed of pieces blown by the wind and massed together in an irregular way. Change of wind and tide causes the fields to float away. When several fields are blown shorewards together they grind and crush together, forming irregular ice many feet thick. Frost and spray soon cements this together into a hard mass, almost impossible to break. Floating again, these agglomerated ice masses, often many miles in extent, are carried out to sea, there to produce great danger to navigation. While the Gulf of St. Lawrence never freezes over entirely, there are to be found all winter floating areas, which take up their position with the direction of the wind. As the spring advances these fields become weaker, and finally disappear. The last to open is the Straits of Belle Isle, where towards the end of June it becomes sufficiently free for ships to navigate.

LIMITS OF REGION OF ICEBERGS.

It has been found that in April, May and June are the greatest number of icebergs. They have been seen as far south as the 39th degree of latitude, and as far east as longitude 38° 30′. In general, it may be stated that floating ice may be met with anywhere in the North Atlantic Ocean, northwards of the 40th degree of latitude, at any season of the year.

SURFACE TEMPERATURE OF THE LABRADOR CURRENT IN WINTER AND SUMMER.

During the winter months the surface temperature of the Labrador current often falls to the freezing point of salt water, about 28° F., but it is more often at 29° or 30° F. As the spring advances the line of low temperature advances farther north, until in July or August the temperature on the Grand Banks towards the Straits of Belle Isle reaches 40° or 45° F., and gradually falls northwards to 29° F. in Hudson Straits. The surface temperature varies considerably, depending on the proximity of ice or land, as will be explained shortly. No measurements have been made north of the Banks in winter or spring, when the Straits of Belle Isle are ice-bound. Reports of the temperature of the ice track are frequently given by sea captains. Results as low as 22° F. have been shown to me, but I believe these to be impossible, and due to some error of measurement arising from the crude method now in vogue on our Atlantic liners.

INFLUENCE OF ICEBERGS ON THE TEMPERATURE OF THE SEA.

There can be no question but that icebergs have an important influence on the temperature of the sea. Composed of frozen fresh water from the north, they melt rapidly when they drift down to the warmer waters of the Banks and when they reach the Gulf Stream. On account of the small conductivity of the water, no appreciable cooling can result from this cause. If it were not for the currents in the sea and the circulation set up by the melting berg, no cooling effect would be appreciable. That there is a small cooling effect has been shown by captains and others, but this has not been made use of for telling the proximity of ice with any success. What is called salt-water ice—that is, ice formed by the freezing of salt water—contains a small trace of salt in its composition and frequently holds salt mechanically, but there is very little difference in the purity of the ice. It is well known that water in freezing expels all the impurities, hence it is erroneous to say that salt-water ice floats under the surface. What is called salt-water ice is really the same as field-ice, and is exceedingly hard to break. Its structure is not uniform, and, composed often of irregular broken pieces, it has no line of cleavage.

PETTERSSON'S THEORY OF ICE MELTING.

Dr. Otto Pettersson has for some time shown experimentally that ice melting in salt water produces three currents (Fig. 1). (1) When the ice melts it cools the salt water, which sinks down by convection. (2) A stream of warmer salt water moves in towards the ice, giving rise to a horizontal current. (3) The melted ice consists of fresh water, which does not mix with the salt water on account

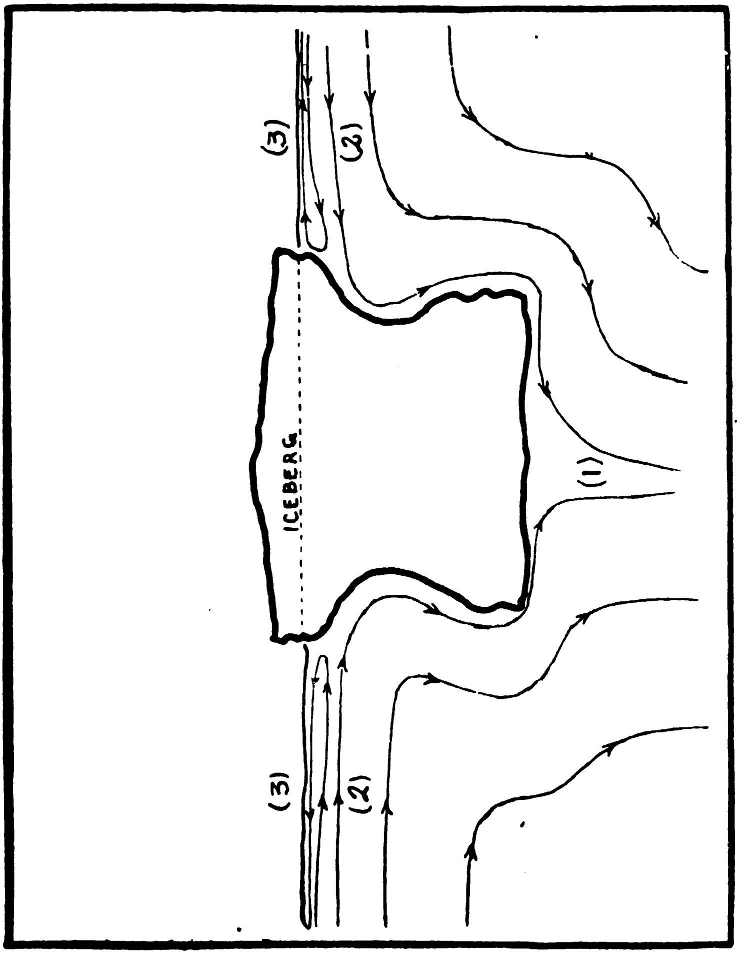

Fig. 1.—Ice Melting in Salt Water.

of the difference of density. This fresh water rises around the ice and spreads out over the surface. The ice becomes surrounded by a layer of fresher water, which tends to remain on the surface. As the ice moves the fresh water moves with it. Waves do not mix it with the salt water, but leave it practically unaffected. Pettersson believes that this circulation has an important influence on the currents in the sea. I have found that the sea becomes diluted by this fresh water over a very wide area. The lower density of this diluted layer prevents the normal vertical circulation in the sea, and causes characteristic temperature effects.*

Icebergs which have been left high and dry on the shore by the tide show the action of the melting. Bergs which become top-heavy and turn over also bear evidence to the underwater current producing the melting. The form of the ice shows a deep furrow running all around where the melting process has proceeded, and this is often the cause of the rolling over of a berg to find equilibrium in some other position. Icebergs are often in an exceedingly unstable state, and the slightest increase in wind or sea disturbance causes them to break up or turn over. This is one of the reasons why captains always go as far away from them as possible.

SIGNS OF THE PROXIMITY OF ICE.

Since the earliest days efforts have been made to find some means of detecting the presence of ice. To those who have had many years' experience in navigating in the ice region the presence of ice is made known by a number of effects. Before ice can be actually seen there is a peculiar whiteness observed around the berg on a dark night, except in the case of dark bergs. This is called by mariners the ice "blink." It is caused by the reflection of the scattered rays of light from the sky from the white surface of the berg. Thus, it is a contrast between the black absorbing water, which reflects none of the light, and the ice, which scatters nearly all. A dark berg is one casting a shadow towards the ship. When the light comes more strongly from any particular part of the sky the iceberg often cannot be seen in certain directions, while clearly visible by the ice blink in others. This I believe to be the reason why the officers on the "Titanic" did not see the berg soon enough to stop. It is stated that on a clear day on the horizon over the ice the sky will be much paler or lighter in colour, and may be distinguished from that overhead.

During foggy weather, ice can sometimes be made out on account of its darker appearance. In this case it is a contrast effect again, but this time it is the shadow of the berg against the white shadowless fog particles.

Icebergs are sometimes detected by the echo from the steam-whistle or fog-horn. They are also frequently heard for many miles by the noise they make in breaking up and falling to pieces. The cracking of the ice or the falling of the pieces into the sea causes a noise like thunder.

The absence of swell or waves is sometimes a sign of ice or land,

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* [Recent experiments have shown this dilution to be very small.—August 27, 1912. H. T. B.]

and the presence of flocks of birds far from land is an indication of ice. The temperature of the air usually falls as ice is approached, and mariners describe a peculiar damp cold as distinguished from the cold caused by a change of wind. I shall discuss the fall in temperature of the sea as ice is approached in what follows.

FAILURE OF PREVIOUS EFFORTS TO MAKE USE OF TEMPERATURE CHANGES IN THE SEA.

Navigators place no reliance on temperature measurements. As a matter of shipboard routine, the temperature of the water is taken; but very little, if any, attention is taken of it. The method is to dip a canvas bucket over the side and bring up a sample of sea water. The quartermaster then inserts a good household thermometer in the water, waits for a few minutes, and then reports the reading to the bridge. The thermometer is usually graduated in two-degree intervals, representing a length of stem about one-eighth of an inch long. The interval of time between the dipping of the water and the report of the reading may be anything from five to ten minutes. In the meantime the ship has sailed some miles beyond the point of observation. It is not surprising, in the light of my results, that no value whatever can be attached to measurements of this kind. As an example I can quote from a standard work on navigation, Captain Lecky's "Wrinkles in Practical Navigation," fifteenth edition :—

"Allied to fog is the question of danger from ice. It is a popular delusion among passengers on board ship, that, by taking the temperature of the sea surface at short intervals, the approach to ice is unfailingly indicated. Unfortunately such is by no means the fact, and reliance thereon invites disaster. More than ordinarily cold water merely shows that the ship is in a part of the ocean where ice may possibly be encountered, and not that it is actually present.

"By kind permission, and on the unexceptional authority of Captains Ballantine, Dutton and Smith, of the Allan Mail Steamship line, all men of high standing in the profession, and well acquainted with ice navigation, it is here stated that no appreciable difference in the temperature of the sea surface is caused by the proximity of even the largest icebergs, and when one considers what a poor conductor of heat water is, their statement can be well believed. . . .

"In a letter to the author, Lord Kelvin says: 'The conducting power of water is so small that there would be absolutely no cooling effect by conduction to a distance from an iceberg; but there might be a considerable effect by the cold and light fresh water running down from the iceberg, and spreading far and wide over the surface of the sea.'

"This seems a reasonable supposition, but it is more than likely that the film of cold fresh water would be broken up by the agitation of the wind and waves, and in any case disturbed and turned over by the plough-like action of a vessel's bow going at speed. Under these circumstances the hydrometer would be no better than the thermometer.

"Again, it is well known that about the Banks the Labrador current is *sometimes* colder when no ice is to be seen than it is when the contrary is the case. In winter its surface temperature even falls to 28° F. Large icebergs have been actually passed at a distance of a quarter of a mile, and the sea-surface temperature tested carefully, without finding a *single degree of difference* from what previously existed when there were none in sight.

"It may be fairly assumed, therefore, that no reliance is to be placed upon the thermometer as an immediate or direct means of detecting the presence of ice, especially when it takes the form of stray bergs. In fog it will simply tell you when the ship has entered the cool current, which may, or may not, be ice-bearing."

Dr. W. Bell Dawson, Director of the Canadian Hydrographic Survey, has made a study of the temperature effect of an iceberg in the Straits of Belle Isle. In his report to the Department of Marine and Fisheries, in 1907, he says :—

"*Icebergs in Relation to Water Temperature.*—On August 7, 1894, an unusually large iceberg was aground in 57 fathoms off Chateau Bay. An instrumental survey made in a boat showed it to be 780 feet long, 290 feet wide, and 105 feet high. The water temperatures on different sides were 38°, 37°, and 37°, at distances ranging from 130 to 1320 feet from it. On that day the water temperature, on a line from Chateau Bay to Belle Isle, was 36½° off the mouth of the bay, 39° in the middle, and 41° off the south end of Belle Isle. It was lowered less than 2°, therefore, in the proximity of the iceberg.

"The next day, August 8, a small iceberg was aground in Chateau Bay. The water temperature in the middle of the bay was 34° and at the mouth 34½°. The lowest temperature close to the iceberg was 33½°, which shows a difference of not more than 1° due to the iceberg.

"In 1906, an iceberg about 140 feet long was aground in 38 fathoms, about 1½ mile from Station P, where it remained for several days. On June 19 it was examined in a boat. The surface temperature in the strait at the time was 35½°, and close around the berg it was found to be the same, except on the west side, where the water tailing from it with the flood was 35°. There was thus only ½° difference of temperature to be found near it."

It is clear that up to the time of the experiments with the microthermometer, in 1910, there was good evidence to show that the ordinary thermometer is useless to detect the small temperature effect of an iceberg. Hence captains are correct in their statement that the ship's thermometer is useless as a means for locating an iceberg.

THE RECORDING MICRO-THERMOMETER.

The development of the recording micro-thermometer has been the result of nearly twenty years' experience in the study of minute temperature changes in the ice-bearing water of the St. Lawrence river. By applying very sensitive electrical resistance thermometers, it has been possible to show that the temperature of the St. Lawrence in winter never varies more than a minute fraction of a degree from the freezing point. The small variations that have been observed and measured are the result of heat exchanges when ice is present, and they accompany the formation or the disintegration of the ice. The delicate poising of the forces of Nature are here wonderfully illustrated. A few thousandths of a degree on either side of the freezing point of the river water produces immense physical effects. Thus the character of a river may be changed in a single night, or the wheels of the largest hydro-electric station completely stopped by a drop of a few thousandths of a degree in the temperature of the water. As a result of this knowledge, it is now possible to apply artificial heat around the wheels and gates, and completely prevent any trouble from the sticking of the ice needles drawn in by the water.

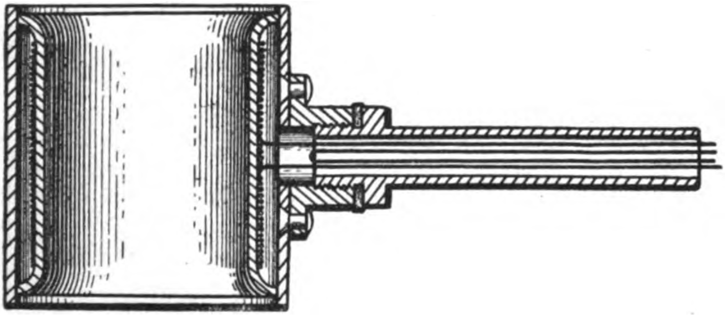

FIG. 2.—BULB OF MICRO-THERMOMETER.

Four years ago I undertook some experiments to study the ice-breaking operations on the St. Lawrence, and to determine the effect of open water conditions on the temperature of the river. During this time I turned my attention to a practical form of electrical resistance thermometer, which could not only easily measure thousandths of a degree but automatically record them on a chart when working from the ice-breakers going at full speed. Following out ideas which I had developed along these lines, I devised an instrument which has proved so satisfactory and trustworthy that I have given it the name of the micro-thermometer.

The thermometer bulb, Fig. 2, consists of concentric cylinders of copper tube about 6 inches long and 4 inches in diameter. On the surface of the inner tube the coil of resistance wire is wound. The outer tube fits closely over this and the ends are soldered together. This makes a winding which affords a large cooling surface and is exceedingly sensitive to temperature changes. The coil consists of 250 feet of large size iron wire silk-covered, and has a resistance of approximately 125 ohms at 0° C. I used iron wire for two reasons, its cheapness and its remarkable steadiness of zero for ice temperature readings. The larger size wire enabled me to apply considerable battery power without appreciable current heating. My experiments with platinum were most unsatisfactory. There was great current heating and changes of zero which as yet I have not been able to explain. The iron wire maintains an invariable zero. When iron is heated over 50° C., however, its zero does change and can only be restored by cooling to a low temperature. It appears that there must be some internal molecular change in the iron above 50° C. which has not as yet been studied.

The connecting wires from the bulb pass to a lead-covered cable of four strands, as in Callendar's compensating method, and then to the recorder, which may be a good Callendar recorder with the galvanometer removed. The galvanometer actually used consists of a jewelled-bearing D'Arsonval, of special design, of about 300 ohms resistance. The bridge wire may be interchanged so as to produce different scales. The one I have found useful is 8 inches long, and gives a range of one degree over its entire length. I have used also one giving half a degree, and the records are very nearly as perfect as with the one degree range. Other wires giving four and eight degree intervals are frequently useful. By my arrangement it is possible to switch from one wire to another very quickly. The one degree scale is essential in very cold water, but for warmer water the coarser scale is necessary.

The thermometer may be connected so as to read the temperature directly, in which case the resistance of the bulb is compensated by a known resistance box. In this way the value of the resistance from previous calibration gives a measure of the temperature. The thermometer bulb may also be connected differentially with a second equal bulb, and only differences in temperature recorded on the chart. This method can be applied to give the difference in temperature between the bow and the stern of a ship. Thus a ship so equipped could not possibly run into ice as long as the bow thermometer was equal to the stern thermometer.

In order to measure the temperature of the river or sea water the thermometer bulb can be trailed by guy ropes alongside, and the lead cable carried through heavy copper pipes up to the deck of the ship. From there the cable can be carried to the chart-house or other convenient location. The thermometer bulb may also be placed in a tank fed from the circulating water in the engine room. This method is desirable on a fast moving ship or in regions where much floating ice is about.

It will be seen that the micro-thermometer is a refined electrical resistance thermometer.

PRACTICAL TESTS OF THE MICRO-THERMOMETER.

Realizing the great menace to navigation in the presence of ice, I was anxious to find whether refined measurements of the temperature of the sea could be used to warn a ship at night or in time of fog.

As a preliminary test of the sensitiveness of the instrument, I had some experiments made in the St. Lawrence river during the time of the ice-breaking work. The C. G. S. "Lady Grey," having cleared out the ice from the channel as far as Lake St. Peter, from Quebec, was detailed to steam slowly up the river towards the unbroken ice sheet in the lake. The edge of the ice was sharp and well defined, and extended out from the shore along the banks of the river. The current of the river flowed from under the ice in a slightly diagonal direction, so that, steaming in the open water, the current flowed in such a way as to pass under the edge of the shore ice. The ship started two miles below the upper edge of the ice, and measurements of temperature were taken at intervals up to the ice. Fig. 3 shows the character of the temperature curve obtained, and illustrates how accurately the micro-thermometer registers even so small a temperature change as one-tenth of a degree per mile. In the lower part of the diagram a map is shown giving the edge of the ice, the ship's course, and the direction of the current. It will be seen that the temperature is everywhere a measure of the ship's distance from the ice sheet taken along the current lines. Thus the temperature curve gives us the contour of the ice.

Having so determined the presence of the ice sheet, even in water less than one-tenth of a degree above the freezing point, I determined to obtain measurements of the sea temperature in the vicinity of icebergs. In July 1910, the Canadian Department of Marine and Fisheries kindly granted me facilities for doing this on the ice-breaking steamer "Stanley," proceeding to Hudson Bay with a survey party. My assistant, Mr. L. V. King, undertook the observations during this trip, and an account of the work was published in my report to the Government.

The thermometer was placed over the side of the ship, immersed to a depth of about five feet, and a record of temperature was made through the Straits of Belle Isle along the Labrador coast to Hudson Bay. The recorder was placed in Mr. King's cabin, where he could observe the effect of ice and land. Several icebergs were passed in the northern journey at a distance of about half a mile, and these were recorded on the chart by a rapid fall of temperature of from one

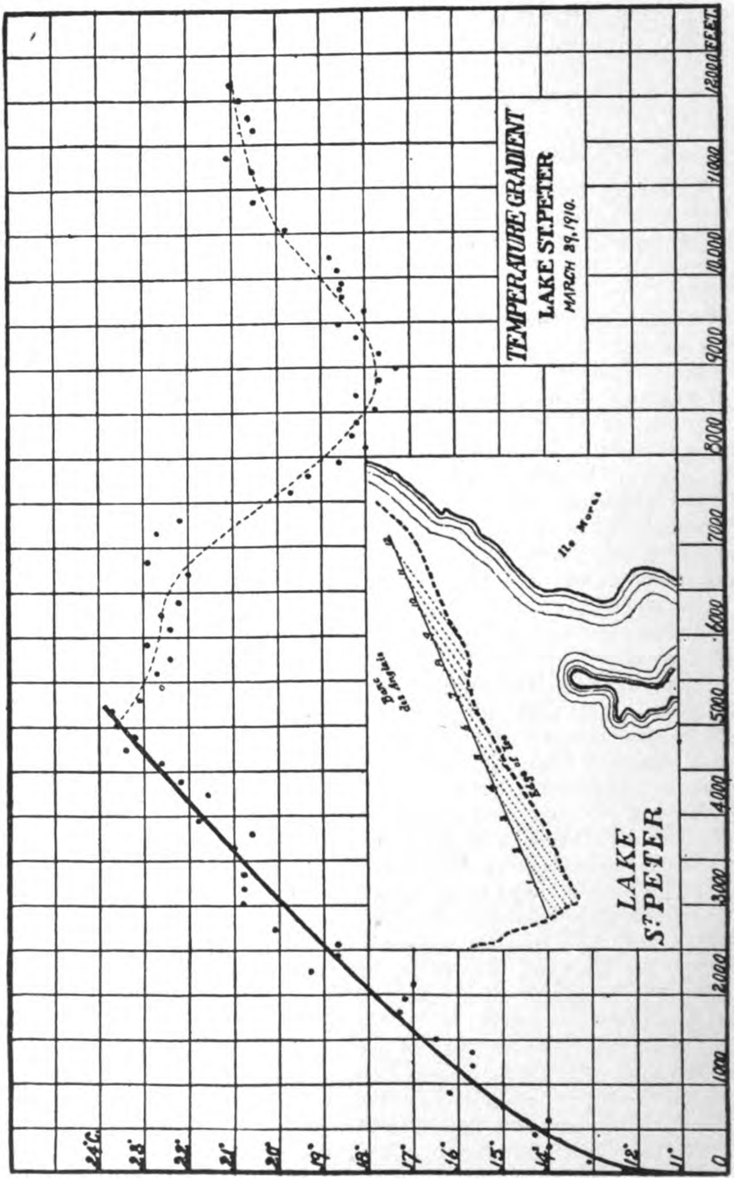

FIG. 8.—CONTOUR OF THE ICE SHEET SHOWN BY TEMPERATURE CURVE.

to two degrees as the bergs were approached. It was found as the ship drew near a berg that a rise of temperature took place first, followed by a rapid fall. On the micro-thermometer the effect was clearly shown, but would have been missed entirely on an ordinary thermometer. I have called this peculiar rise and fall of temperature the *Iceberg Effect.* And it seems to be characteristic, and easily distinguished from the small oscillations of temperature found in the open sea. It is evident that the iceberg effect is caused by the fresh water observed by Pettersson in his tank experiments diluting the sea water, and creating a blanket of lighter water in which the sun's heat is absorbed. In the open sea the warming of the sea by the sun is offset by the vertical circulation, but in the fresher and lighter water this is impossible, and the warmer water remains on the surface.

DISTURBING INFLUENCE OF LAND ON THE TEMPERATURE OF THE SEA.

One of the most interesting results of the Hudson Bay experiments was the effect of land on the temperature of the sea. The coast of Labrador appears to exert an influence in turning up the colder undercurrents of the Arctic stream. Thus, whenever the ship steamed in towards the coast line the temperature was found to fall one or two degrees. The limit of the influence appears to be about five miles. It has been shown by Dr. Dawson that the shoals in the Bay of Fundy influence the surface temperatures, and this is in accord with the present results. Taking this into consideration, it appears that the micro-thermometer may be of great service in telling the presence of land and shoals from a ship at sea. Fig 4 shows the effect of the coast line of Labrador in lowering the surface temperature of the sea.

TRANSATLANTIC EXPERIMENTS.

During my trip across the Atlantic early in May of this year, I obtained a continuous record of the temperature of the sea from Halifax to Bristol. Through the kindness of the Canadian Northern Steamship Company, I installed the micro-thermometer on the "Royal George," and for 3000 miles the instrument faithfully recorded the temperature variations in the sea. In these experiments the bulb of the thermometer was placed in sea water, drawn continuously from the circulating water of the engines at a point within a few feet of the intake mouth situated about 16 feet below the water line. The wires passed through the engine room to Chief Engineer McQuitty's office, where the recorder was situated. The course followed was south of Cape Race to the south of Ireland.

The result of these experiments brings out several points of importance. The iceberg effect was clearly obtained when passing, early in the afternoon, into the ice track. Fig. 5 shows this very well even in water nearly at 32° F. The ship's course was altered here to avoid a large berg, and when abeam of two immense icebergs situated approximately 10 miles on either side of the ship, the microthermometer showed its minimum reading, and as the bergs were left behind the temperature rose rapidly to a maximum, fell off again and then rose steadily. No further ice was encountered until early the following morning, when several icebergs were passed in water measuring approximately 37° F.

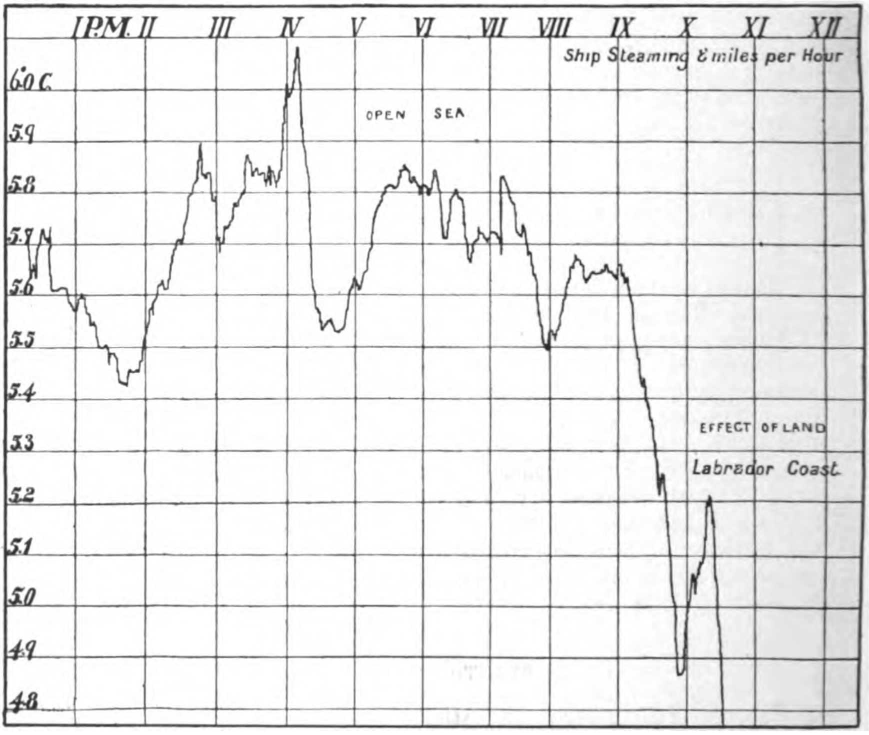

Fig. 4.—Marine Micro-thermogram, showing Effect of Land.

The sudden change of temperature on passing out of the Arctic current into the Gulf Stream was clearly marked. Here a rise of temperature of nearly 10° was recorded in a little over half an hour. The great steadiness of the temperature of the Gulf Stream was remarkable, since for hundreds of miles the variations were not more than a quarter of a degree. The complete absence of any diurnal variation of temperature was clearly marked.

The tests have shown that large variations of sea temperature are caused only by some abnormal condition. Thus land affects it, icebergs produce characteristic disturbances, and current boundaries are clearly shown. The existence of a submarine hot spring was indicated when passing over the great wall of the continental shelf

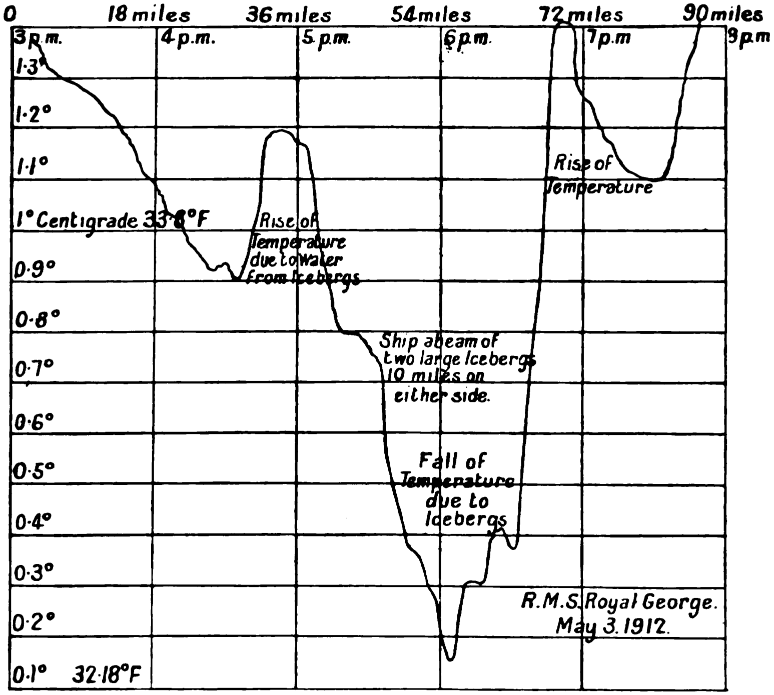

FIG. 5.—MICRO-THERMOGRAM OF THE ICEBERG EFFECT FROM RECORD MADE ON THE MICRO-THERMOMETER ON CANADIAN NORTHERN STEAMSHIP "ROYAL GEORGE."

about 400 miles from the Irish coast. The bottom of the ocean rises quickly here from about three miles to one-third of a mile. Just over this wall the temperature rose rapidly to a sharp peak about one and a half degrees warmer than the surrounding sea and immediately fell again. Fig. 6 shows this disturbance very clearly marked. It shows the peak superimposed on the small variations characteristic of the open sea.

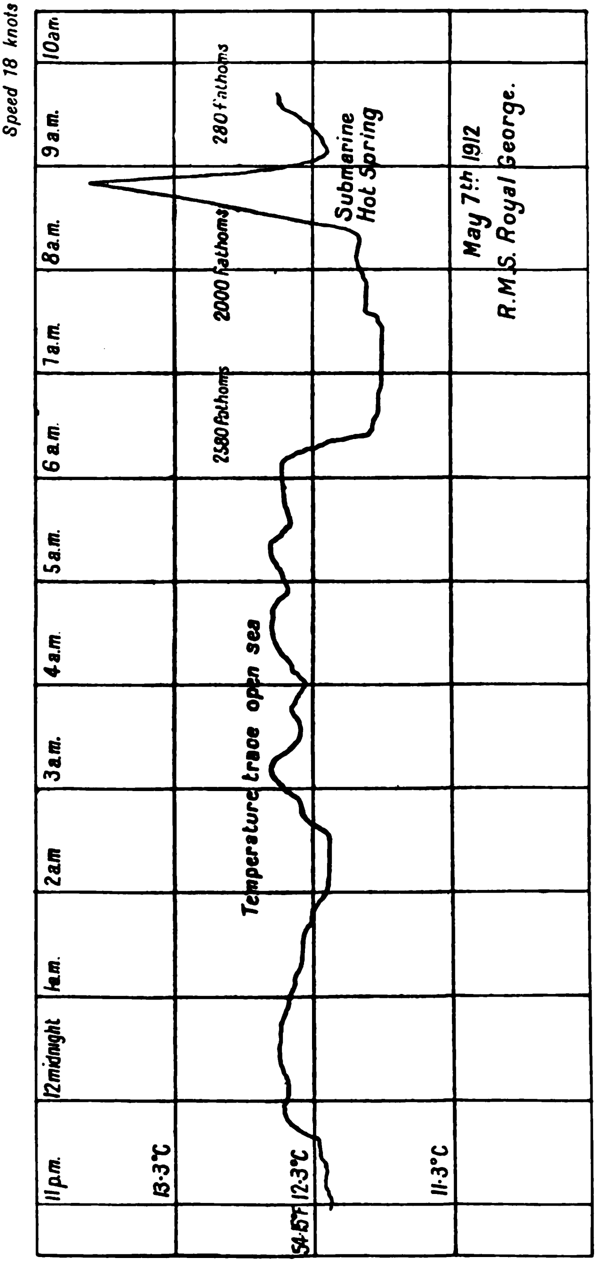

FIG. 6.—MICRO-THERMOGRAM OF THE OPEN SEA, SHOWING DISTURBANCE DUE TO SUBMARINE HOT SPRING OVER THE CONTINENTAL SHELF.

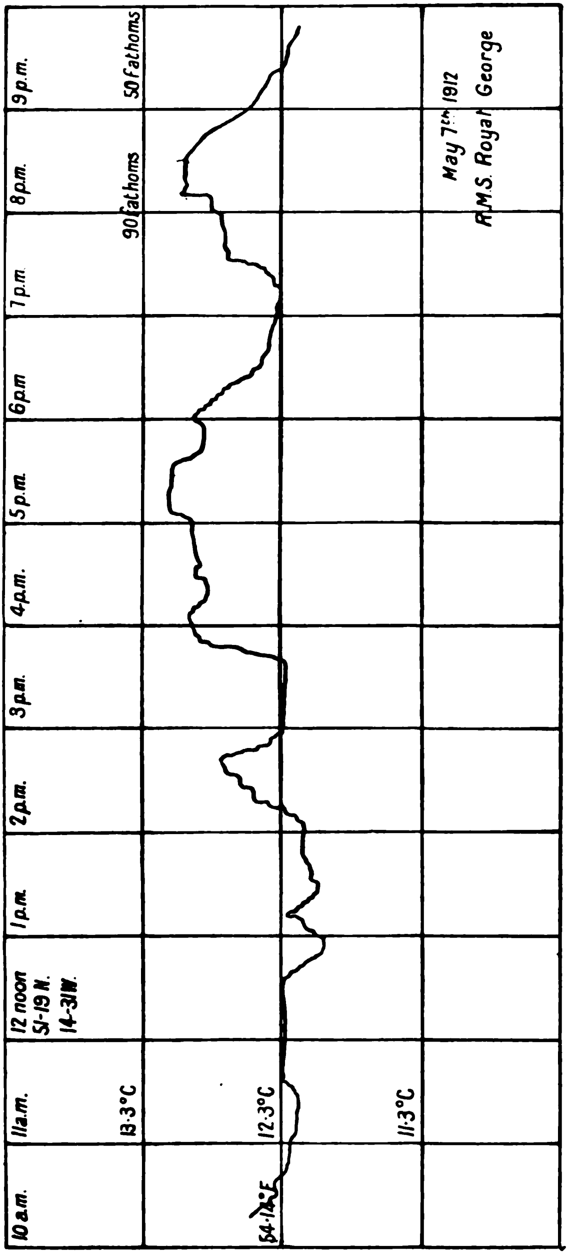

Fig. 7.—Micro-thermogram showing Increasing Temperature Variation due to Approach of Irish Coast.

Nearing the Irish coast the variations of the sea temperature became more marked, and this commenced at a distance of 200 miles from the shore. Fig. 7 illustrates this very well, where the variations may be seen to be much larger than in the previous plate. Fig. 8 shows the effect of drawing near the Fastnet lighthouse, which was passed at a distance of about four miles. Here the temperature rose as in the iceberg effect, and then fell as the ship was abeam of the nearest point of land. The temperature then is seen to rise, as land was left behind, through the Irish Sea. The approach to Lundy Island caused the temperature to rise rapidly again and then fall as the ship passed abeam of the island, about 300 yards. The effect is here more marked than in the case of the Fastnet, but it must be remembered that the ship passed much closer in the former case than in the latter. The proximity of the English coast caused a rapid fall of temperature, as shown in Fig. 8, when the ship was only two miles off the Somersetshire coast, steaming up the Bristol Channel. It is probable that the rise of temperature in both cases is due to the effect of fresh water from the land floating out over the surface of the sea, and diluting it to a considerable distance.

Early in June, since giving this address, I was enabled, through the kindness of the Allan Steamship line, to obtain measurements of the sea temperature on the northly route from Ireland across the Atlantic to Cape Race, and through the Gulf of the St. Lawrence to Montreal. These charts, which I intend to publish later, completely confirmed the previous results. During fog in the ice track, the iceberg effect was clearly obtained, and later a large iceberg was discovered ahead.

The icebergs all produced an effect in the instrument, even those passed at distances ranging from 8 to 12 miles. The temperature of the water through the ice track was between 40° and 41° F. Even when passing within a quarter of a mile of a berg it was only 39°, yet by the iceberg effect—i.e. the sharp rise of temperature to a maximum above the sea temperature—the influence of the berg could be clearly seen. This shows very well that the actual temperature of the water is not a guide as to the proximity of ice, not only because this may be rising and falling, due to other causes, but because in different seasons of the year the Arctic current is at different temperatures. The variation of temperature as ice is approached is, however, unmistakable, and this the micro-thermometer has invariably indicated. Should the ice be found in a locality where variations due to other causes are found, the iceberg effect is so characteristic and sharp that it will be superimposed on the other curve in such a way as to be unmistakable.

Nearing Cape Race the temperature fell rapidly several degrees below the surrounding sea temperature. Fresh-water rivers flowing into the St. Lawrence, where the waters were salt, produced the iceberg effect, thus clearly indicating the real cause of that phenomenon as being due to the water from the icebergs diluting the salt water.

THE PRACTICAL LOCATION OF ICE.

The important question arises as to how an iceberg can be definitely located by means of the micro-thermometer. The exact position of a berg ahead of a ship in a fog is of the greatest importance

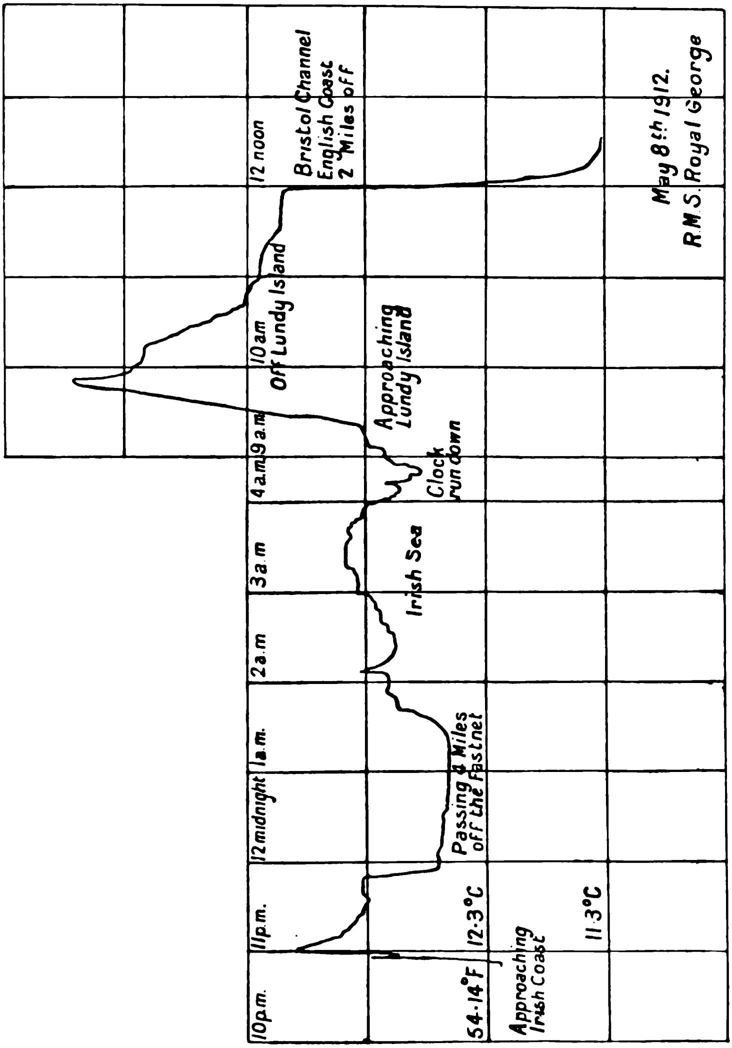

FIG. 8.—MICRO-THERMOGRAM SHOWING EFFECT OF LAND.

to determine. In general, it may be said that an iceberg will make itself felt in the first place by a rapid rise of temperature as it is approached. In the immediate vicinity of the berg the temperature falls quickly. The first warning will be when the temperature begins to mount up the scale above the surrounding sea temperature. In regions where icebergs are in close proximity, safe navigation will be found possible, since no isothermal line can lead to an iceberg.

In conclusion, I wish to impress upon the reader the importance of the fact that the actual temperature of the water in the ice track is no guide to the proximity of even the largest iceberg. The experiences of North Atlantic sea captains alone testifies to the uselessness of individual observations. It is to the small variations of temperature we must look for the infallible guide, and by means of the character of these variations we can determine the presence of ice, land, or currents.

[H. T. B.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1912.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

Miss Margaret Winifred Hughes,
Charles Hesterman Merz, Esq.
Oscar Schwartz, Esq.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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PORTRAIT.

Maull and Fox, Messrs.—Portrait of Professors Brande and Faraday.

WEEKLY EVENING MEETING,

Friday, June 7, 1912.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Sir William Macewen, M.D.(Glasgow), LL.D. F.R.C.S.(Hon.), D.Sc.(Oxon.), F.R.S.

Lord Lister.

Introductory Remarks.

It is said that the Egyptian kings, after death, had to undergo a trial before they were embalmed. Our great men appear to be similarly arraigned, as their character and attainments are brought to judgment by the lesser ones of earth, who bear testimony concerning them, weighing them in their own balance, each to his entire satisfaction.

The reputation of the smaller great men may be affected by this judgment. The reputation of the truly great lies beyond the reach of blame or praise, and lives on in history after all those who have weighed them have been forgotten. Such was Lister.

Unlike the Egyptian kings, however, Lister was tried during life. His struggle with disease and with the mind of his fellow-men, though long and severe, was ultimately successful, and the great good achieved by the adoption of his methods was universally acknowledged. Whilst yet in the autumn of his life he was able to look on at the spread of the antiseptic system over ever-widening areas, and to rest in the consciousness that he had accomplished a great work for the good of mankind.

It would be out of place here to lay before you in their order the honours and titles showered upon Lister in the latter period of his life, or to refer to the impressive ceremony on the occasion of his funeral in the fane of the immortals—Westminster Abbey—save to remind you that, though the Abbey was open to receive his remains, the true man was shown in him, when he directed that his body should be laid where his dust would mingle with the ashes of one he loved, and who had been his constant companion and helpmate during the most active portion of his life.

LISTER'S EARLY DAYS.

HIS FATHER, JOSEPH JACKSON LISTER.

Lister was blessed in his earlier days by excellent environment, well suited to one who was about to follow a scientific career.

His father, Joseph Jackson Lister, was a man of outstanding scientific merit. He left school at 14 years of age, to assist his parent at the wine trade, in London, and though for many years closely tied down to business, he yet contrived, by early rising and otherwise, to gain free hours in which to supplement the education received at school, which, though sound, was insufficient for his needs. He was thus, in many respects, a self-taught man. He possessed extreme accuracy of thought, and was a most methodical worker, skilful with brush and pencil. As a microscopist he was the first to solve the problem of the achromatic lens, whilst many observations on zoophytes and ascidians were made by him—a paper on the former appearing in the "Philosophical Transactions."

Here, then, was a man of grit, who left school at 14 years of age to enter business in London, but who, by dint of his own exertion, found means to extend his scanty education, devoting what time he could to scientific pursuits with accuracy of thought and methodical work. Had it been in one's power to choose a father for Lister, one could not have chosen a man better suited to the purpose.

HIS TEACHERS AND THEIR INFLUENCE.

The influence of Sharpey upon young Lister was great. At University College he was guided by Sharpey to undertake important researches, which were continued by Lister after he had left London. Papers were written by him upon numerous physiological and histological subjects—such as "The Contractile Tissue of the Iris," "An Inquiry regarding the Parts of the Nervous System which regulate the Contractions of the Arteries," "On the Cutaneous Pigmentary System of the Frog," "On the Coagulation of the Blood," "On the Early Stages of Inflammation," etc. There also Graham aided him in the study of chemistry, and furnished his mind with a sound knowledge of its principles. In Edinburgh he studied under Syme, and became a great admirer of Syme's intellect and judgment, as well as of his skill as an operator. This intimacy ripened and lasted throughout the remainder of Syme's life.

All these men were the best he could have been educated under and associated with. The knowledge and experience gained from them admirably equipped him for the life of research which he was about to enter.

It is obviously impossible here to deal with all the periods of Lister's life, and therefore it has been deemed expedient to select one of these, and that the most vigorous of his career, when he evolved the theory of antiseptics, and when he had to defend his thesis.

PRE-ANTISEPTIC DAYS.

In Lister's early surgical days in the Glasgow Royal Infirmary he encountered the same phenomena, which prevented the healing of wounds, in all hospitals throughout the world. Suppuration in wounds was the rule, and very profuse it generally was. Dressing of the wounds had to be done daily, and sometimes several times a day.

The handling of highly-inflamed wounds was a source of pain, and the dressing was anticipated by the patients with an apprehension akin to terror, especially as the exhausting process, with its accompanying high fever, reduced the resisting powers of the individual to a low ebb. The suppurative process invaded the deeper tissues, affecting the blood-vessels, and produced septic thrombosis, from which septic emboli were carried to distant parts. The dissemination of the septic material was soon shown in the high temperature, the violent rigours, the profuse sweats, the sweetish, sickening odour from the breath, the yellow cachexia, emaciation, and final delirium which all too frequently ended in death. Sometimes every patient in a ward who had a serious operation performed upon him would be swept away. The wards would then be emptied, lime-washed, well ventilated, and reopened, soon to be the scene of further pyæmic ravages.

All this was most depressing for the attendants, and many of the young student dressers had at times to retire to the restoring influences of the open air, and there debate within themselves whether it were physically possible for them to continue their work in the midst of such scenes of suffering.

Surgeons and patients alike dreaded operations, owing to their terrible results, and only operations of dire necessity were permitted to be performed. Severe compound fractures were treated by amputation of the limbs, as to attempt to save them was to court disaster. Consequently amputations in those days were common.

INFLAMMATION SUPPOSED TO BE NECESSARY TO WOUND HEALING.

Surgeons were ever at work, attempting to discover the cause of this excessive inflammation, and many were the theses and volumes written on the subject. It was fully recognized that, if one could discover the cause of this excessive inflammation, it would be the first step toward eradicting the serious conditions attending wound healing. The minds of men, however, were obscured by an initial error of fundamental importance, which warped their vision, and for which the doctrine inculcated at the time was responsible. The error lay in the belief that, with the exception of healing by what was known as primary union, inflammation was necessary for wound healing, and that in the process of healing the phenomena of inflammation were always present.

Wound healing was treated in the text-books under the heading of Inflammation. So that, instead of inflammation being regarded, *ab initio*, as a noxious process, it was looked upon as a necessary and beneficent one. It was only when it became excessive that it was regarded as baneful, and efforts were made to lessen it. Varieties of pus were recognized, and students were taught to discriminate between them. Some were thin and offensive, while others were spoken of as good and laudable, the latter being supposed to be the usual accompaniment of the healing wound.

The trend of inquiry was therefore directed toward the elucidation of the phenomena produced by inflammation on the tissues, instead of endeavouring to discover the cause of inflammation and how it could be prevented.

Lister at first joined in the former quest. His papers on "An Inquiry regarding the Parts of the Nervous System which regulated the Contraction of the Arteries" (Phil. Trans. 1858) and "On Early Stages of Inflammation" (Phil. Trans. 1858) were important, and had their bearing on the subject at issue.

SAVIOTTI AND LISTER ON THE NERVE CONTROL ON THE BLOOD-VESSELS IN THE EARLY STAGES OF INFLAMMATION.

Much time was devoted by many observers to the elucidation of the effect of reflex action upon the blood-vessels, in the early stage of inflammation. The investigations of Saviotti * and Lister proved that reflex action, to which alone active hyperæmia had been previously attributed, was not the only factor in the production of increased local blood supply. From observation on the cutaneous pigmentary cells of the frog, it was evident that they were controlled by reflex action, as exhibited when the pigment in them contracted to the centre of the cell, under the influence of a beam of light, passing through the eye of the animal. It was also seen that limited areas could be taken out of the control of this general reflex action by the application of certain irritants applied locally.

In order to account for this latter phenomenon, it was deduced that peripheral nerve ganglia must exist, having control of limited areas, and that when these ganglia were paralysed, they would no longer transmit the general nerve impulses.

Granting this conclusion, it was further deduced that a similar local nerve control might regulate the smaller blood-vessels under topical irritation of the parts and in the earlier stages of inflammation. If these ganglia were paralysed, the arteries would dilate, as is seen in active hyperæmia.

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* Virchow Archiv. vol. i.

One could linger fondly over every one of these earlier papers of Lister, permeated as they are with the spirit of the true enquirer who was endeavouring to coax Nature to reveal her secrets, and it is with difficulty that one can be persuaded to leave them.

Taken alone, they are sufficient to mark their author as an original thinker and investigator.

These communications were interesting and important, yet, though highly appreciated by all who valued science for itself and admired it for the truth it aimed at, they did not directly appeal to those who look lightly upon investigations, the results of which are not immediately productive of direct and tangible benefits.

As far as those earlier papers bore on the phenomena of inflammation, they were overshadowed by the observations of Cohnheim (1867) "On the Vascular, Textural Changes," especially the passage during inflammation of the leucocytes from the blood-vessels into the surrounding tissue, and by Stricker and many others on the histological phenomena displayed by the fixed tissue cells in inflammation.

Though these observations threw great light on the manner in which the cellular elements were converted into pus, they did not explain the causes of inflammation and pus production.

Meanwhile, surgeons were trying chemical substances in an empirical way, as means of arresting excessive inflammation in the wounds, and some of these were attended by an amount of success. They had powerful remedies at hand, if they could have seen how to employ them, and what to use them against. They did not know what to attack—whether it was a something inherent in the tissue or something present in the air.

Some regarded the excessive suppuration as due to a process of putrefaction, and many believed that putrefaction was the result of chemical changes which ensued on exposure of the tissues to the atmosphere.

MICROBES DISCOVERED TO BE THE CAUSE OF PUTREFACTION AND FERMENTATION (CAGNIARD-LATOUR AND SCHWANN).

While darkness still brooded over the realm of Medicine and Surgery, notwithstanding endeavours to reach the light, investigations had been conducted in quite other fields, which were not only important in themselves, but were destined to lead to the revelation of multitudes of hitherto invisible organisms, everywhere existing, and playing a very potent part in the economy of the world.

Over thirty years previously, 1835–37, Cagniard-Latour in papers to the French Academy * recognized that alcoholic fermentation was due to the presence of a living organism. He found that grape-juice contained numerous globular bodies which he considered to be of vegetable nature, and which reproduced themselves by budding. These were always present when fermentation occurred, and in their absence fermentation did not take place.

This microscopic "fungus," which he found in yeast, appeared to be the essential agent in the production of fermentation, and he attributed the resolution of sugar into alcohol and carbonic acid to its influence.

In the following year Schwann of Berlin † published the results of an investigation into the causes of putrefaction, in the course of which he also independently discovered the yeast plant.

What was of equal importance, he demonstrated that a putrescible fluid, such as a decoction of meat, could be freely and indefinitely exposed to the action of pure air—air free from dust and organisms—without putrefaction ensuing in it.

Those views of Schwann that putrefaction is due to the action of living organisms, and those of Cagniard-Latour showing that fermentation is caused by the yeast plant, did not, for more than thirty years, yield the fruit which, viewed from present knowledge, might have been expected from them. The bearing which these discoveries might have had on other fields was hindered by the fact that they were not generally known—the dissemination of scientific knowledge being slower in those days—and also by the fact that there were many who refused to accept the existence of organisms as the cause of fermentation or putrefaction. They chose to believe, with Baron Liebig, that chemical action was alone sufficient to cause the changes which were seen in fermentive and putrefying fluids, and that the mere exposure to air was sufficient to produce the effect.

Liebig held that the ferment was a nitrogenous substance undergoing putrefaction, and that it converted the oxygen of the air into carbonic acid.

The demonstration of Schwann, which showed that putrefaction could be prevented as long as the putrescible body was exposed to pure air only, was either unnoticed or was not believed.

CAGNIARD-LATOUR AND SCHWANN'S OBSERVATIONS CONFIRMED BY PASTEUR, 1858.

Pasteur, when in the University of Lille, had abundant opportunity of studying alcoholic fermentation, as alcohol was the staple

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* Annales de Chimie et de Physique, t. lxviii. 2nd series, p. 206, 1838. Comptes Rendus, t. iv. p. 905, 1837.

† Poggendorf Annalen, xli. p. 184, 1837.

article of manufacture in that town. He became thoroughly convinced of the correctness of the observations and deductions previously made by Schwann and by Cagniard-Latour. He verified and extended those observations showing that fermentation was due to micro-organisms, and confirmed the observations of Schwann that pure air had no effect in producing putrefaction.

Pasteur demonstrated these facts by many diverse and original experiments on fermentable and putrescible substances. He showed (1) that sterilized grape-juice did not ferment in the presence of pure air, however long it might be kept, and that while thus exposed none of the ferments of wine were produced in it; (2) that blood could be kept indefinitely exposed to pure air without undergoing putrefaction—it yielded neither bacteria nor vibrios.

AN ORGANISM FOUND TO BE THE CAUSE OF A DISEASE BY DAVAINE, 1850.

In 1858 Pasteur had demonstrated that the whole process of fermentation disproved the theory of spontaneous generation. He showed that putrescence is a form of fermentation due to the presence of micro-organisms. He also reasoned from analogy that the relation of micro-organisms to disease was highly probable, and that the changes taking place in the secretion of a wound were probably due to a somewhat similar process to that of fermentation. The probability of micro-organisms being the cause of disease was greatly increased by a momentous discovery from a totally different quarter, an organism having been constantly found invading the tissues and blood-vessels of animals which had died of splenic fever. This was the *Bacillus Anthracis*, discovered by two observers, Davaine * and Rayer in 1850, though it was ten years later before the complete identification of the relationship of this germ to the disease was definitely established.

Chronologically this was the first pathogenic Bacillus discovered.

Therefore, at this time, fermentation and putrefaction had both been demonstrated to be due to the action of living organisms, and one of the infectious diseases had been shown to be due to another micro-organism, whilst it had also been demonstrated that neither putrefaction nor fermentation could ensue from mere exposure of susceptible substances to the action of pure air.

Pasteur had further advanced his belief that the changes taking place in the secretion of a wound were probably due to a process somewhat similar to that of fermentation.

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* Davaine. Recherches experimentales sur la Maladie Charbonneuse, par H. Toussaint. Paris: Asselin & Co.

OTHER THEORIES OF FERMENTATION.

Those observations and conclusions of Schwann, Cagniard-Latour, Pasteur and Davaine were not generally known, and, where known, were not generally accepted—other theories being still in the field.

Besides the chemical theory of fermentation and putrefaction, the believers in heterogeneous and spontaneous generation were still many. Pouchet,* in 1859, made a systematized attempt to prove the possibility of spontaneous generation, and even after the antiseptic theory had been formulated, spontaneous generation was still advanced by Bastian † and other observers who tried to demonstrate that vital force and living matter may arise *de novo* under the action of ordinary physical forces.

TYNDALL, HUXLEY, AND RAY LANKESTER AGAINST SPONTANEOUS GENERATION.

Such writings had the effect of confusing the issue and diverting men's minds from the truth, and it was in no small measure due to the powerful help of Tyndall, Huxley, and Ray Lankester that the error was conclusively refuted.

Dr. E. Ray Lankester ("Nature," Jan. 30, 1870) stated that he had performed numerous experiments with turnip solution, made under the conditions given in Dr. Bastian's book. No life was developed, a result contrary to that obtained by Bastian.

Prof. Huxley (" Nature," Oct. 13, 1871) stated that he had seen Dr. Bastian's experiments and preparations and expressed his belief that the organisms which Bastian got out of his tubes were exactly those which he put into them.

Tyndall, the illustrious predecessor of Sir James Dewar at the Royal Institution, submitted the question to fresh investigations. He had gone over the ground on which Bastian took his stand and was able to expose many of the errors by which experimenters were misled. One very beautiful and convincing experiment was introduced by Tyndall. He observed the fact that in a box, the sides of which were coated with glycerine, all the particles of dust floating in the inside air fell and adhered to the glycerine in the course of a few days. The air is then *optically* pure. A transmitted ray of light tells the moment when this purity is obtained. Tyndall proved that to an eye, rendered sensitive by remaining in darkness for a few minutes, the course of the ray is visible only so long as there are floating particles of dust capable of reflecting or diffusing light. On the other hand, the course of the ray becomes invisible to the eye as soon as the air has deposited all its solid particles.

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* Pouchet, Heterogenie ou Traité de la Géneration spontanée basé sur des nouvelles experiences. Paris, 1859.

† Bastian, The Beginnings of Life, 1872. The Evolution and Origin of Life.

When this deposition has occurred, any organic infusion may be introduced into the box and kept there without undergoing the least putrefactive or fermentive change, and without producing bacteria.

Tyndall, in a letter to Pasteur on the subject, concludes: "For the first time in the history of science we are justified in cherishing confidently the hope that, as far as epidemic diseases are concerned, medicine will soon be delivered from empiricism and placed on a real scientific basis. . . ."

LISTER PROMULGATES AND INTRODUCES THE PRACTICE OF THE ANTISEPTIC TREATMENT OF WOUNDS.

While Professor of Surgery in Glasgow, Lister was constantly speculating on the cause of inflammation and the cause of putrefaction in wounds, and during a discussion with friends, it was suggested to him that Pasteur's papers on fermentation might be of use in elucidating what seemed to be somewhat kindred processes. These papers of Pasteur came as a revelation to Lister, especially as he had not been cognisant of the observations made about thirty years previously by Schultze (1836), Schwann (1837), and Cagniard-Latour (1838), which had really laid the foundation of the germ theory and modern bacteriology.

The perusal of Pasteur's work threw a flood of light on the subject of decomposition in wounds, and Lister at once accepted the theory, and began a search for a something which would prevent the entrance of living organisms into wounds, believing that if such were found the healing of a wound would proceed "just as if it were subcutaneous."

About this time creosote—the active agent of which was carbolic acid—was used for disinfecting sewage, and Lister secured a sample of carbolic acid from Dr. Anderson, Professor of Chemistry in Glasgow University. He tried it in August 1865, with results which justified his hypothesis.

As in the case of many other discoveries, more than one person had come to the same conclusion quite independently.

Jules Lemaire had been similarly impressed with Pasteur's observations, and had tried to eradicate the germs from wounds by means of chemical antiseptics, and especially by carbolic acid. He seems to have recognized the true basis of antiseptc surgery, the germ theory of fermentation. He published his researches in 1863.*

In the wards of the Glasgow Royal Infirmary, which, previously, in common with other hospitals, had been the home of septic diseases, with their terrible issues, the introduction of the antiseptic treatment by Lister acted like the magician's wand, dispelling the horrors which previously accompanied wound-healing and creating an atmosphere of sweetness and health.

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* W. Cheyne, Antiseptic Surgery, p. 356, London, 1882.

DIFFICULTIES IN ACCEPTING ANTISEPTIC THEORY AND PRACTICE.

The new treatment and the theory, on which it was founded, were received at first—save by a few faithful pupils—with scepticism and coldness, and later on with open hostility.

The works of Schwann and Cagniard-Latour and Pasteur were not generally known. Germs in purulent wound secretions were not then demonstrated, and Lister was boldly called upon to show those organisms in such secretion before founding a theory and practice upon the assumption of their presence. This desirable demonstration was not obtained until later (1880–81), when Billroth and Ogston demonstrated the presence of organisms in pus taken from acute abscesses. Yet the deduction arrived at by Lister at that time, from the experiments of many able and reliable scientists, was not only permissible, but was the only one to which the data then available inevitably pointed.

Subsequent investigations with which all are now conversant abundantly proved the correctness of the conclusion.

The usual fate meted out to innovators or disturbers of settled doctrines was shared by Lister. He and his theory were virulently assailed both from within the hospital and from without. Some colleagues, some governors and a host of free-lances all joined in the fray, the most ignorant being ever the loudest. He was despitefully used, and had to bear the derision and cackle of fools. A scoffer has not necessarily a high standard of intelligence, and at best he does but devil's work. Fortunately such ephemera, troublesome and annoying as they are, die before the light.

GERMANY READILY ACCEPTS ANTISEPTIC TEACHING.

Lister's teaching in this country was at first of no avail. It fell upon ears unprepared to receive it. Except by his own students in Scotland and a sprinkling of them in England the antiseptic treatment passed unheeded over Britain, yea, even over the land of Pasteur it passed to other nations, especially to that country where the scientific education of its people, their earnestness of purpose, thoroughness of method and their desire to see under the surface, enables them to appraise quickly any theory and practice having a scientific basis.

Another reason for the rapid spread of antiseptics among the surgeons of Northern Europe was that they bestirred themselves "to go and see" the practice first-hand. Having read that a new theory and practice were enunciated, they became sufficiently interested to go straightway and see how the method was carried out. Thereafter they returned to their homes with a precise knowledge and a truer conception of the theory and practice than they otherwise could have had.

German surgeons were able, from their laboratory training, to devote the necessary care and attention to the carrying out of the detail of antiseptic treatment which made its practice a success. The influence of Danish and German testimony, corroborative of the value of the antiseptic treatment, made itself felt, and did much to render its adoption universal.

PYOGENIC ORGANISMS DISCOVERED, 1880; ORGANISMS THE CAUSE OF DISEASE.

The discovery of pyogenic organisms as the cause of suppuration in wounds was of great importance, as it demonstrated the correctness of Lister's theory and gave a tangible basis for the practice. It placed both the ætiology of suppuration and its treatment upon a scientific basis. Empiricism was in great measure overthrown, and henceforward a rational ætiology for disease and its treatment was sought. Microbiology, then in its infancy, received a great impulse, and fresh fields were opened from which an ever-increasing harvest has been reaped. From the burning plains and pestilential swamps of the Tropics, to our own slums, with their three great D's—Dirt, Damp and Darkness, which we fondly harbour in our midst—disease after disease has been traced to its micro-organismal cause. Those diseases which remain will doubtless yield their secret to steady investigation, and would do so all the more readily if submitted to a properly constructed investigation department under scientific control.

Tuberculosis, which for centuries was regarded as an hereditary disease, was shown to be germ-borne, common to man and to the lower animals, and to be inter-communicable between them. Cancer and sarcoma and the varieties included under these terms are doubtless also germ diseases, the germs of which are probably to be found near our every-day life, if our eyes were open to perceive them. The need of a scientific experimental investigation department under scientific control is all the more apparent as the Government has at last ventured to advance measures intended to mitigate one of the communicable diseases—Tuberculosis.

Yet what they do with the one hand they undo with the other.

For instance, in the old days the light that entered our houses was taxed, and the windows became smaller; to-day the powers that be tax the air contained therein, and for every cubic foot of air enclosed additional charge is made. In order to escape or to lessen this burdensome assessment, many huddle themselves and their families into dwellings of the smallest compass, where they inhale pre-breathed air, with the resultant lowering of vitality, germ-dissemination, disease, and death. Then we appoint commissions to find out the cause of the deterioration of the race!

Every man who is born has an inalienable right to as much fresh air as he is able to consume; but the "powers that be" say, "God may give you that right, but we shall tax you for using it." It is true that they do not as yet tax us for the amount of air we inhale out of doors, possibly because they do not know how to estimate the individual consumption. Yet the governing bodies are full of humanity and have the best intentions. When the ravages of tuberculosis can no longer be hidden, as it stares them in the face, they are moved to grasp at the first thing that appeals to them, and they say to the affected, "Come, let us help you; we shall put you in sanatoria." What happens there? The patient has his birthright restored to him in being able to breathe the fresh air which God has meted out so freely, and for the use of which he was previously taxed.

Would it not be better to begin at the other end?—better to stop producing tuberculosis than merely to alleviate or to cure it once it has developed?

MODIFICATIONS IN ANTISEPTIC TREATMENT.

Antiseptic treatment underwent many modifications. What was essential in the early days of its introduction became no longer necessary as the advance of knowledge brought clearer conceptions and paved the way for radical changes in the form of treatment. It became apparent that though strong antiseptics introduced into wounds destroyed organisms, they at the same time exercised an irritating influence on the living tissue, lowering its vitality, decreasing its resisting power, and increasing its secretions. To that extent the free use of antiseptics in the interior of wounds was detrimental.

Besides being harmful, they were unnecessary, as healthy living tissue of the interior of the body is free from germs, and pure air is innocuous.

EVOLUTION OF ASEPTIC TREATMENT.

As microbiology yielded its secrets, the bearing of germs and their products on the phenomena of disease ever became the clearer. The primitive conception of germs acting upon the human body just as they would in a laboratory test-tube was soon dispelled, and the multiplicity of the defensive reactions established by the living tissues for their own protection was recognized. It was seen that the microbic products excited the tissues to anti-bacillary action, and the elements of immunity, as we now understand it, were established.

The anti-bacillary phagocytic action of the living healthy tissue was demonstrated by the beautiful experiments of Metchnikoff, when it was seen that a certain number of organisms, brought into contact with the living tissue, could be destroyed therein by living cells. It also became obvious that the healthy living tissue in the interior of the body was inherently free from germs, and when wounded was capable of healing rapidly, and would do so if its vitality were preserved, and if germs emanating either from the abundant flora of the skin or from elsewhere could be prevented from being brought into contact with it.

This was effected by sterilizing the skin and the instruments and all material brought into contact with the wound, without allowing antiseptics to invade the interior of the tissues.

It is upon such lines that aseptic treatment was introduced. Aseptic surgery was a natural evolution of antiseptic surgery—the one paved the way for the other.

The surgery of the present day involves the performance of painless, almost bloodless, operations, the wounds healing, as a rule, under a single dressing of the slightest description. Any material introduced into a wound for the arrest of hæmorrhage, or for bringing the parts together, is of a kind which the living cells are able to remove after its function has been performed. When the patient recovers from the effects of the anæsthetic his trouble is over. The film which covers the wound drops off of itself as soon as the phagocytes have completed their work of removing the deep part of the catgut stitches.

The air of cheerfulness in a surgical ward is now pronounced, the difficulty often being to persuade the patients to remain quiet for a time sufficient to allow the internal parts to heal.

ADVANCES CONSEQUENT TO THE INTRODUCTION OF ASEPTIC SURGERY.

The introduction of aseptic surgery and the extension and more correct appreciation of bacteriological knowledge have enabled surgical procedures upon the human body to be greatly extended. The dangers arising from risk of wound infection being averted, many new devices have been practised for reaching the internal organs and for removing therefrom the products of disease.

Regions of the body hitherto considered too dangerous to be operated upon have now been successfully entered, and it soon became apparent that wherever diagnosis showed the presence of a serious pathological lesion, there the surgeon could follow, and where practicable eliminate it.

Since the introduction of asepsis and the consequent acquisition of extended and more definite pathological knowledge, the field of greatest surgical activity has been the abdomen—Abdominal Surgery, as we now know it, has been created. Tens of thousands—possibly millions—of human lives have been saved in this field alone, and the amount of pain and discomfort alleviated has been enormous.

Surgeons of all countries have contributed to this beneficent result, and have vied one with another in restoring health and comfort to the community, thus adding greatly to the economic prosperity of the nations.

Compound fractures, so fatal in pre-Listerian days, were not only robbed of their fatality, but surgeons became emboldened to make compound fractures for the rectification of mal-formation of the limbs.

Compound fractures, under the heading of Osteotomies, have been performed aseptically in thousands, occasionally as many as 10 osteotomies having been performed on one patient at one time with an almost uniformly excellent result—the bones healing aseptically. A portion of bone which has been fractured and displaced may be removed, placed in aseptic solution, pared, re-arranged and returned to its proper place in the body, where it will live and grow, and become restored to its functional use. Defects in the bone of one person may be made up by grafting on a portion of bone removed from another. A transplanted bone may be divided into little pieces, and a mosaic work of new bone may be placed in another animal to restore defects.

Asepsis, along with better knowledge of the physics of the pleura, has enabled surgeons to penetrate into the lungs and to remove therefrom pathological products, with a gratifying amount of success. Portions of lung have been removed, and several times the whole of one lung has been successfully taken away—the patients still continuing to enjoy life, working for their own living, and one, at least, for that of his family.

Aseptic Surgery has enabled operations upon the brain to be safely undertaken, and Brain Surgery has kept pace with the localization of cerebral function. Its further development rests with the increase of precise data on that subject. Direct experiment on the brains of lower animals—thanks to Ferrier, who was one of the first to perform them—furnished excellent data on the localization of the motor functions, but information as to the localization of the higher intellectual functions must be gathered by patient clinical observation.

The discerning eye and the discriminating sense guiding the educated finger with its softness and lightness of touch have, under asepsis, carried out many operative procedures on diseased brains, where the tangled skeins of that delicate fabric have been unravelled.

Considering the delicacy of the organ, and the fact that in many instances life has been sapped at the governing centres of energy by the pathological lesions, operations on the brain have been very successful, many of them veritably snatching the patient from the brink.

The consummation of all that surgical activity has been attained by the introduction of aseptic practice, surgery having been re-developed since the introduction of Listerian principles and treatment.

PERSONAL TEACHING AND DEMONSTRATION VERSUS BOOKS.

It is fashionable nowadays to decry University teachers and professors, many regarding them as an effete remnant of antiquity. It is contended that all that is required is to issue a paper or a book and allow the students to read at their own fire-sides instead of compelling them to attend lectures and demonstrations in a University.

It is true that formerly the teaching extended only as far as the teacher's voice could carry, but now one can write in one's own laboratory, and, if the message be important, it will be borne to the limits of the civilized world, and thus it is possible to instruct an audience of unlimited size.

There is, however, a difference between teaching by books and *viva voce* teaching and demonstration. Some things may be explained by means of clear writing and may be understood by correct reading of what has been written, but there are other things difficult of comprehension in detail without the aid of practical demonstration. More especially is this the case when one has not the opportunity of personal contact with the introducer or with one who has seen his practice and followed his methods. No matter how well a statement may be written, impressions are drawn from it which differ according to the preparedness and previous experience of the mind of each individual reader. Personal observation produces a much more vivid impression and generally corrects individual misconstruction.

As Professor of Surgery in Glasgow Lister followed the Scottish method, teaching the principles of surgery in the University, and afterwards demonstrating his methods in the wards of the Infirmary. His lectures in the University and his observations in the wards were complementary to each other, and gave a groundwork more thorough than could otherwise have been obtained. Those who had been so taught found his methods simple and easy of execution, and were often astonished at seeing others less fortunate falling into serious errors in their attempt to carry out the antiseptic practice after reading Lister's papers alone.

There were many earnest men—professors of continental universities, amongst others—who were well qualified to read correctly what had been written, yet who, having read, were not satisfied, but straightway desired to be brought into personal contact with the Professor, in order that they might hear his teaching from his own lips and see the practice carried out by his own hands.

Professor Saxtorph, of Copenhagen, was amongst the first of the many distinguished visitors to the Glasgow Royal Infirmary to see Lister's practice and to study his methods. After a few days he remarked that the seeing of the practice persuaded him of its feasibility, and that it then seemed much easier than it did when he had only read Lister's papers. So it was with many others.

LISTER AS A SCOTTISH AND AS A LONDON PROFESSOR.

Lister's teaching was more rapidly propagated among the students he had in Scotland than among the London students. The position which he occupied as a Scottish Professor aided in this, as it was different to that held by him as Professor of Surgery in a London Hospital.

In London in those days, the bulk of students desired, naturally, to take the membership of the College of Surgeons, and most teachers at that time taught to the requirements of the Board of Examination, otherwise their prelections were not specially sought after. In London, Lister was teaching a new doctrine, not yet generally homologated, and his wards were attended by few students compared to the numbers that surrounded him in the Scottish Universities. On the other hand, Lister had less time to devote to the teaching of students, as London was more accessible to foreign visitors, and many of his days were devoted to demonstrations for their benefit.

As a Scottish Professor, Lister's position offered the greatest advantage for the dissemination of his doctrines. He could teach his own students what he believed to be true, and, if necessary, teach them in advance of the time, as the teaching and the examination were both under his supervision. Hence Glasgow students were the first to become imbued with the spirit and to thoroughly grasp the principles of antiseptics, which they carried into practice. Scottish students thronged his wards and lecture-theatres in the infirmaries, an eager, critical and ultimately an enthusiastic crowd, bringing inspiration to their teacher whose principles and practice they afterwards bore to the ends of the earth, even before many examining Boards were prepared to accept his teaching.

LISTER'S INFLUENCE ON THE SCOTTISH STUDENTS.

Lister's presence in the Scottish Universities was of the utmost value. By him teaching was maintained at a high level; he used the Universities to stimulate thought, and therein aided them to perform their highest function. It was an inestimable blessing to a

University to have such a man in it, and a priceless privilege to the students—to those of them who could appreciate it—to be allowed to stand silently by and watch the habit of mind and see how the brain worked. He was a man in earnest, and therefore he taught. His teaching was supported by direct appeal to nature. He accumulated data by observation and experiment, from both of which careful deductions were drawn. As a thinker, Lister did good by laying bare the difficulties he encountered in carrying out his projects, and his modes of overcoming these difficulties. In this way he stimulated and propagated the thinking faculties of the student. He showed his methods and thereby paved the way for others to follow.

His view of physiology in relation to life and of pathology in relation to disease was philosophic. A philosophic pathologist differs from the practitioner, as much as the astronomer, who formulates the laws of the stars, differs from the mariner who sets his course by deductions derived therefrom. Lister was, therefore, in some respects alone, as a man of that kind must be.

In Glasgow Lister not only promulgated the theory of antiseptic surgery, but he worked out and thoroughly established its utility in practice, leaving behind him a body of enthusiastic disciples. After spending, as Regius Professor of Surgery, nine of the most active years of his life, and those fullest of scientific fruition, Lister passed quietly from Glasgow without public recognition of his services, the general body of citizens being unaware that a great scientific achievement had been wrought in their midst. It was long afterwards, "when all the world wondered," that Glasgow became alive to what it had possessed—and lost.

THE STUDENTS' APPRECIATION OF LISTER.

As to the manner in which Lister was viewed by the Glasgow students, the following is an extract from a letter written me by a friend and fellow-student, which so well expresses my own views that I give it in his words :—

"We students were all very much impressed by the personality of Lister. His mild expression and his grave demeanour gave him benign dignity which could not fail to command respect. Even the impediment in his speech—which in another man might have been a source of annoyance to his hearers, seemed in his case only to add to the weight of what he said ; and as he spoke slowly not a word of his lecture was lost. You remember how his students more or less unconsciously fell into a way of speaking, which was a manifest echo of the master's voice. This affectation on the part of the students was simply an indication of the hero-worship which pervaded Lister's class, for there is no doubt we all idolized him.

"I understand it has been said of Lister that he was not a good lecturer, and that he was not a brilliant operator. You and I can laugh at such statements. Lister's lectures were all that could be desired. His subject-matter was always interesting—generally intensely so; his thoughts were clear and well defined, and he conveyed them to his hearers in choice and vivid language which left no doubt as to his meaning. As to his operating slowly, did he not tell us that the advent of anæsthesia by chloroform had rendered it unnecessary and undesirable to hurry through the work? Lister was thinking out and developing the antiseptic system at that time, and we were privileged to listen day by day as he informed us of his difficulties and how he proposed to overcome them; and so we watched the progress of those early stages which laid the foundation for the final triumph. . . . Above and beyond all petty details rises the towering personality of the man while the mind dwells fondly on the grandeur and beneficence of his achievements." (J. W. Allan).

From another of Lister's Glasgow students, and one who was his house-surgeon in the Royal Infirmary, Dr. Jas. Coats (now Colonel Coats), who was among the first to practise antiseptic surgery in private, an interesting letter of reminiscences has been received, from which the following is culled:—

"One day when Lister was visiting his wards in the Glasgow Royal Infirmary, there was a little girl whose elbow-joint had been excised, and this had to be dressed daily. Lister undertook this dressing himself. The little creature bore the pain without complaint, and when finished she suddenly produced from under the clothes a dilapidated doll, one leg of which had burst, allowing the sawdust to escape. She handed the doll to Lister, who gravely examined it, then asking for a needle and thread he sat down and stitched the rent, and then returned the dolly to its gratified owner."

On one occasion on which Lister visited my wards in the Royal Infirmary, after he had been for some time in London, we were walking together from a ward in one part of the building to a ward in another, by means of a gangway of wood and glass, when Lister remarked: "Macewen, do you find this bridge a convenience to your work, for if so, you have to thank me, as I was instrumental in getting it put up?" I replied, "Yes, it is a convenience, but it is nothing compared to the greater gangway you provided, by which the patients after operation cross directly from the wards into the midst of life and health." I received a kindly look, a suppressed smile, and a pressure of the arm. . . .

In Edinburgh, though his system was met by some with determined opposition, it was adopted more or less thoroughly by others, and by many of the younger men enthusiastically. The students, though doubtful at first, began to observe his results, and soon became admirers of Lister and his work.

When Lister entered the clinical theatre of the old Infirmary to deliver before a crowded audience his last lecture there, he was presented with a farewell address from the students. As he rose to reply, the air was rent with a rousing cheer that shook the building to its foundation. A cheer such as only British students—at rare moments—know how to give. It is spontaneous, and bursts like a blast from the throat of a whirlwind. Lister was fairly overcome. One who was near him, as a quiet observer, saw that he first became pale, and then a blush covered all his visible anatomy to the tips of his fingers. In a few moments he recovered, and said: "Gentlemen, I can recall my reception in the surgical theatre in Munich, on my visit to Nussbaum, where I was greeted with a German 'Hoch.' It was to me almost overpowering in its enthusiasm, but it was as nothing compared to this." (Dr. Young.)

That spontaneous outburst issuing from four hundred throats made amends for much. It was the laurel crown offered by the students. That rousing cheer reverberated through his whole being, and left such deep impression as doubtless would be with him to the end.

Allow me to thank the President and the Council of the Royal Institution for permitting an old student to place a stone on the cairn of a beloved master.

[W. M.]

WEEKLY EVENING MEETING,

Friday, June 14, 1912.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

A. HENRY SAVAGE LANDOR, ESQ., *M.R.I.*

Unknown Parts of South America.

[NO ABSTRACT.]

GENERAL MONTHLY MEETING,

Monday, July 1, 1912.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

John Somerville Highfield, Esq.
Walter Judd, Esq.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Journal, Vol. VII. Part 2. 8vo. 1912.
Memoirs of Department of Agriculture: Entomological Series, Vol. II. No. 9; Vol. IV. No. 1. 8vo. 1912.
Astronomer-Royal—Report to the Board of Visitors of the Royal Observatory, 1912. 4to.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXI. 1° Semestre, Fasc. 10-11. 8vo. 1912.
Aeronautical Society—Short History of the Society, with List of Fellows. 8vo. 1912.
American Geographical Society—Bulletin, Vol. XLIV. Nos. 5-6. 8vo. 1912.
Astronomical Society, Royal—Monthly Notices, Vol. LXXII. No. 7. 8vo. 1912.
Boston Public Library—Sixtieth Annual Report. 8vo. 1912.
Boston Society of Natural History—Memoirs, Vol. VII. 4to. 1912.
Proceedings, Vol. XXXIV. Nos. 9-12. 8vo. 1910-11.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIX. Nos. 15-16. 4to. 1912.
British Astronomical Association—Journal, Vol. XXII. No. 8. 8vo. 1912.
Canada, Geological Survey—Mineral Production, 1910. 8vo. 1912.
Chemical Industry, Society of—Journal, Vol. XXXI. Nos. 11-12. 8vo. 1912.
Chemical Society—Proceedings, Vol. XXVIII. No. 403. 8vo. 1912.
Journal for June, 1912. 8vo.
Chemistry, Institute of—Proceedings, 1912, Part 3. 8vo.
Register of Fellows, etc., 1912. 8vo.
Cirencester, Royal Agricultural College—Scientific Bulletin, No. 3, for 1911. 8vo. 1912.
Chicago, John Crerar Library—Seventeenth Annual Report, 1911. 8vo. 1912.
Cracovie Academie des Sciences—Bulletin, 1912: Classe des Sciences, A, No. 4; B, No. 3. 8vo.
Editors—Agricultural Economist for June, 1912. 8vo.
American Journal of Science for June, 1912. 8vo.
Athenæum for June, 1912. 4to.
Canada for June, 1912. 8vo.
Chemical News for June, 1912. 4to.
Chemist and Druggist for June, 1912. 8vo.

Editors—continued.
Concrete for June, 1912. 8vo.
Dyer and Calico Printer for June, 1912. 4to.
Electrical Engineering for June, 1912. 4to.
Electrical Industries for June, 1912. 4to.
Electrical Review for June, 1912. 4to.
Electrical Times for June, 1912. 4to.
Electricity for June, 1912. 8vo.
Engineer for June, 1912. fol.
Engineering for June, 1912. fol.
Ferro-Concrete for June, 1912. 8vo.
Gardener's Chronicle for June, 1912. 8vo.
Horological Journal for June, 1912. 8vo.
Illuminating Engineer for June, 1912. 8vo.
Journal of the British Dental Association for June, 1912. 8vo.
Law Journal for June, 1912. 4to.
London University Gazette for June, 1912. 4to.
Model Engineer for June, 1912. 8vo.
Motor Car Journal for June, 1912. 8vo.
Musical Times for June, 1912. 8vo.
Nature for June, 1912. 4to.
New Church Magazine for July, 1912. 8vo.
Nuovo Cimento for May, 1912. 8vo.
Page's Weekly for June, 1912. 8vo.
Physical Review for May, 1912. 8vo.
Power for June, 1912. 8vo.
Power User for July, 1912. 8vo.
Florence, Biblioteca Nazionale—Bulletin for June, 1912. 8vo.
Franklin Institute—Journal, Vol. CLXXIII. No. 6. 8vo. 1912.
Geographical Society, Royal—Journal, Vol. XXXIX. No. 6. 8vo. 1912.
Year Book and Record, 1911. 8vo.
Geological Society—Abstracts of Proceedings, Nos. 926-927. 8vo. 1912.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Ser. III. A. Tome I. Liv. 3-4. 8vo. 1912.
Life-Boat Institution, Royal National—Journal for May, 1912. 8vo.
Annual Report, 1912. 8vo.
Literature, Royal Society of—Transactions, Vol. XXXI. Part 2. 8vo. 1912.
Report and List of Fellows, 1912. 8vo.
London County Council—Gazette for June, 1912. 4to.
Madrid, Real Academia de Ciencias—Revista, Tomo X. Nos. 6-7. 8vo. 1912.
Manchester, Municipal School of Technology—Annual Report of the Godlee Observatory, 1911. 8vo. 1912.
Meteorological Society, Royal—Record, Vol. XXXI. No. 123. 8vo. 1912.
Microscopical Society, Royal—Journal, 1912, Part 3. 8vo.
Monaco, Institut Océanographique—Bulletin, Nos. 228-232. 8vo. 1912.
National Church League—Church Gazette for June. 1912. 8vo.
Navy League—The Navy for June-July, 1912. 8vo.
New Jersey Geological Survey—Annual Report, 1911. 8vo. 1912.
New York, Society for Experimental Biology—Proceedings, Vol. IX. No. 4. 8vo. 1912.
New Zealand, Registrar-General—New Zealand Statistics, 1910, Vol. II. 4to. 1911.
Onnes, Dr. H. Kamerlingh—Communications from the Physical Laboratory of the University of Leiden, No. 126. 8vo 1912.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for May, 1912. 4to.
Paris, Société Française de Physique—Journal de Physique for May, 1912. 8vo.

Parsons, Hon. R. C., M.A. M.R.I. (*the Author*)—The Tiltometer Chemical Injector. 8vo. 1912.

Pharmaceutical Society of Great Britain—Journal for June, 1912. 8vo.

Photographic Society, Royal—Journal, Vol. LII. No. 6. 8vo. 1912.

Physical Society of London—Proceedings, Vol. XXIV. Part 4. 8vo. 1912.

Post Office Electrical Engineers, Institution of—Papers, Nos. 37–38. 8vo. 1912.

Quekett Microscopical Club—Journal, Ser. 2, Vol. XI. No. 70, April, 1912. 8vo.

Radcliffe Observatory, Oxford—Observations, Vol. L. 1906–10. 8vo. 1912.

Rockefeller Institute for Medical Research—Studies, Vol. XIV. 8vo. 1912.

Royal Colonial Institute—United Empire, Vol. III. No. 6. 8vo. 1912.

Royal Irish Academy—Proceedings, Vol. XXIX. A, No. 6; Vol. XXX. C, Nos. 1–3; Vol. XXXI. (Clare Island Survey) Parts 40, 41, 43, 44, 56. 8vo. 1912.

Index to Publications, 1786–1906. 8vo. 1912.

Royal Society of Arts—Journal for June, 1912. 8vo.

Royal Society of Canada—Transactions, Third Series, Vol. V. 8vo. 1912.

Royal Society of London—Proceedings, A, Vol. LXXXVI. No. 590; B, Vol. LXXXV. No. 578. 8vo. 1912.

Philosophical Transactions, A, Vol. CCXII. No. 485; B, Vol. CCIII. No. 294. 4to. 1912.

St. Petersburg, Imperial Academy of Sciences—Bulletin, 1912, Nos. 10–11. 8vo.

Comptes Rendus de la Commission Sismique Permanente, Tome IV. Liv. 3. 4to. 1912.

Sanitary Institute, Royal—Journal, Vol. XXXIII. No. 6. 8vo. 1912.

Saxon Academy of Sciences, Royal—Abhandlungen: Phil.-Hist. Klasse, Band XXIX. No. 5; Math.-Phys. Klasse, Band XXXII. No. 5. 8vo. 1912.

Berichte: Math.-Phys. Klasse, 1911, Nos. 7–9, 1912, Nos. 1–2; Phil.-Hist. Klasse, 1911, Nos. 6–10, 1912, Nos. 1–2. 8vo.

Selborne Society—Selborne Magazine for July, 1912. 8vo.

Smith, B. Leigh, Esq., M.R.I.—The Scottish Geographical Magazine, Vol. XXVIII. No. 6. 8vo. 1912.

Smithsonian Institution—Miscellaneous Collections, Vol. LIX. Nos. 1, 6, 7, 10, 12, 13. 8vo. 1912.

South Africa, Union of—Agricultural Journal for May, 1912. 8vo.

South African Association for the Advancement of Science—Journal, Vol. VIII. No. 10. 8vo. 1912.

Statistical Society—Journal, Vol. LXXV. Part 7. 8vo. 1912.

Tariff Reform League—Monthly Notes on Tariff Reform for June, 1912. 8vo.

Thompson, W. P., Esq. (*the Author*)—Handbook of Patent Law of all Countries. 15th edition. 8vo. 1912.

United Service Institution, Royal—Journal for June, 1912. 8eo.

United States Department of Agriculture—Experiment Station Record, Vol. XXVI. Nos. 3–6. 8vo. 1912.

United States Patent Office—Gazette, Vol. CLXXIX. Nos. 1–3. 8vo. 1912.

Upsala, Meteorological Observatory—Über das Erscheinen der Seebrise an der Schwedischen Ostküste. Von S. Grenander. 8vo. 1912.

Vienna Imperial Geological Institute—Jahrbuch, 1912, Heft 1. 8vo.

Warsaw, Academy of Sciences—Comptes Rendus, 1912, Part 2. 8vo.

Travaux, Classe des Sciences, No. 6. 8vo. 1912.

Weir, James, Esq., M.R.I (*the Author*)—The Energy System of Matter. 8vo. 1912.

Western Australia, Agent-General—Monthly Statistical Abstract for Feb. 1912. 4to.

Yorkshire Archæological Society—Journal, Vol. XXII. Part 85. 8vo. 1912.

Extra Series, Vol. II. Index to Paver's Marriage Licences, 1567–1630. 8vo. 1912.

GENERAL MONTHLY MEETING,

Monday, November 4, 1912.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Dr. J. H. McBride,
Miss Jane Worth,

were elected Members of the Royal Institution.

The Honorary Secretary reported the decease of Professor Henri Poincaré on July 17, 1912, and the following Resolution, passed by the Managers at their Meeting held this day, was unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record (at this their first Meeting since his death) their sense of the loss sustained by the Institution and the world of science in the decease of Professor Henri Poincaré, D.Sc. (Paris and Oxford), Membre de l'Académie Française, et de l'Académie des Sciences, Membre du Bureau des Longitudes, Professeur Honoraire à la Faculté des Sciences, Professeur Honoraire à l'Ecole Polytechnique, Commandeur de la Légion d'Honneur, Foreign Member of the Royal Society of London, and an Honorary Member of the Royal Institution.

Professor Poincaré evolved the series of mathematical functions known as the Fuchsian Functions; he made highly original and important contributions to the dynamical theory of tides, on the figures of equilibrium of Rotating Masses of Liquids, and the theory of the motions of planets and satellites He was the author of elaborate treatises on various departments of Physics, Mathematics and Philosophy, among which are the following :—" Cours de Physique Mathematique," " Les Méthodes de la Mécanique Celeste," " Thermodynamique," " Electricité et Optique," " La Science et L'Hypothèse," " La Valeur de la Science," and " Science et Methode."

The Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Madame Poincaré and the family in their bereavement.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—Geological Survey: Records, Vol. XLII. Parts 1–2. 8vo. 1912.
Memoirs of Department of Agriculture: Botanical Series, Vol. IV. No. 6.; Chemical Series, Vol. II. Nos. 4–5; Entomological Series, Vol. IV. Nos. 2–3. 8vo. 1912.
Agricultural Journal, Vol. VII. Part 3. 8vo 1912.
Survey of India: Professional Paper, No. 13. Theory of Isostasy in India. By H. L. Crosthwait. 8vo. 1912.

Secretary of State for India—Agricultural Research Institute, Pusa: Bulletin, No. 30. 8vo. 1912.
Report on Improvements in Indian Agriculture. 8vo. 1912.
Kodaikanal Observatory, Bulletin, No. XXVI. 4to. 1912.
Annnal Report of the Board of Scientific Advice for India for 1910–11. 8vo. 1912.

Astronomer-Royal—Greenwich Observations, 1910. 4to. 1912.

Photo Heliographic Results, 1910. 4to. 1912.

Report of H.M. Astronomer at the Cape of Good Hope, 1911. 4to. 1912.

Annals of Cape Observatory, Vol. X. Parts 1–2. 4to. 1912.

British Museum Trustees (Natural History)—Catalogue of Lepidoptera Phalænæ, Vol. XI. and Plates. 8vo. 1912.

Monograph of the Mycetozoa. By A. Lister. 2nd ed. 8vo. 1911.

A Revision of the Ichneumonidæ. By C. Morley. Part 1. 8vo. 1912.

Index to Hand List of Birds, Vols. I.–V. 8vo. 1912.

Guide to Bible Exhibition. 8vo. 1911.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXI. 1º Semestre, Fasc. 12; 2º Semestre, Fasc. 1–7. Classe di Scienze Morali, Vol. XXI. Fasc. 1–4. 8vo. 1912.

Rendiconto, 1912, Vol. II. 8vo.

Allegheny Observatory—Publications, Vol. II. Nos. 17–18. 4to. 1912.

American Academy of Arts and Sciences—Proceedings, Vol. XLVIII. Nos. 1–4. 8vo. 1912.

American Geographical Society—Bulletin, Vol. XLIV. Nos. 7–9. 8vo. 1912.

American Philosophical Society—Proceedings, Vol. LI. Nos. 203–205, General Index to Proceedings, Vols. 1–50. 8vo. 1912.

Transactions, Vol. XXII. Part 2. 4to. 1912.

Aristotelian Society—Proceedings, Vol. XII. 1911–12. 8vo. 1912.

Asiatic Society, Royal—Journal for July–Oct. 1912. 8vo.

Association of Accountants—Journal, Nos. 18–19, Vol. V. July, 1912. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LXXII. No. 8. 8vo. 1912.

List of Fellows, 1912. 8vo.

Bankers, Institute of—Journal, Vol. XXXIII. Parts 7–8. 8vo. 1912.

Batavia, Royal Magnetic and Meteorological Observatory—Verhandelingen, Nos. 1–2. 8vo. 1911–12.

Belgium, Royal Academy of Sciences—Bulletin, 1912, Nos. 4–7. 8vo.

Memoirs in 8vo, 2e Serie, Tome III. Fasc. 5. 1912.

Berlin, K. Technische Hochschule—Programm, 1912–13.

Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1912, Nos. 22–38. 8vo.

Birmingham Natural History and Philosophical Society—Proceedings, Vol. XII. No. 5. 8vo. 1912.

List of Members and Annual Report, 1911. 8vo. 1912.

Boston Public Library—Bulletin, Vol. V. Nos. 2–3. 8vo. 1912.

Botanic Society, Royal—Botanical Journal, Vol. II. No. 4. 8vo. 1912.

British Architects, Royal Institute of—Journal, Third Series, Vol. XIX. Nos. 17–20. 4to. 1912.

Kalendar, 1912–13. 8vo.

British Astronomical Association—Journal, Vol. XXII. Nos. 9–10. 8vo. 1912.

List of Members, 1912. 8vo.

Buenos Aires—Monthly Bulletin of Municipal Statistics for March–April, 1912. 4to.

Burrard, Colonel S. G., C.S.I. R.E. F.R.S. (the Author)—On the Origin of the Himalaya Mountains. 8vo. 1912.

Cambridge Observatory—Annual Report of the Observatory Syndicate, 1911–12.

Cambridge Philosophical Society—Transactions, Vol. XXI. Nos. 17–18; Vol. XXII. No. 1. 4to. 1912.

Proceedings, Vol. XVI. Parts 7–8. 8vo. 1912.

List of Fellows, etc. 8vo. 1912.

Canada, Department of Marine—Report of the Meteorological Service, 1908. 8vo. 1912.

Canada, Department of Mines—Coals of Canada, Vols. I.–II. 8vo. 1912.

Bulletin, No. 6. 8vo. 1912.

Memoirs, 24E, 28. 8vo. 1912.

Mica. By H. S. de Schmid. 2nd ed. 8vo. 1912.

Canadian Institute—Transactions, Vol. IX. Part 2. 8vo. 1912.

Carnegie Foundation for the Advancement of Teaching—Bulletin, No. 6. 8vo. 1912.

Carnegie Institution—Contributions from Mount Wilson Solar Observatory, Nos. 59–60. 8vo. 1912.

Chemical Industry, Society of—Journal, Vol. XXXI. Nos. 13–20. 8vo. 1912.

Chemical Society—Journal for July–Oct. 1912. 8vo.

Proceedings, Vol. XXVIII. No. 404. 8vo. 1912.

List of Fellows, 1912. 8vo.

Chicago, Field Museum of Natural History—Publications: Anthropological Series, Vol. VII. No. 4, Vol. X., Vol. XI. Nos. 1–2; Report Series, Vol. IV. No. 2; Zoological Series, Vol. X. Nos. 5–6, Vol. XI. 8vo 1912.

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List of Publications, New Series, No. 2. 8vo. 1912.
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List of Fellows, 1912. 8vo.

GENERAL MONTHLY MEETING.

Monday, December 2, 1912.

His Grace The Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Peter Boswell Browne, Esq., M.Inst.C.E.
H. B. Grylls, Esq.
G. Kennett-Barrington, Esq., B.A.
William H. Player, Esq.
Spencer William Richardson, Esq., M.A. D.Sc.
Major George Owen Squier,
H. Hawkins Turner, Esq.
Francis W. Willcox, Esq.

were elected Members of the Royal Institution.

Professor Jean Gaston Darboux, D.Sc., Sécretaire pérpetuel de l'Institut (Paris).
Professor Gösta Mittag-Leffler, Ph.D. (Stockholm),

were elected Honorary Members of the Royal Institution.

The following Lecture Arrangements were announced :—

Professor Sir James Dewar, LL.D. Ph.D. D.Sc. F.R.S. *M.R.I.*, Fullerian Professor of Chemistry. Six Lectures (adapted to a Juvenile Auditory), Experimentally Illustrated, on Christmas Lecture Epilogues: Alchemy, Dec. 28; Atoms, Dec. 31, 1912; Light, Jan. 2, 1913; Clouds, Jan. 4; Meteorites, Jan 7; Frozen Worlds, Jan. 9.

Professor William Bateson, M.A. D.Sc. F.R.S., Fullerian Professor of Physiology. Six Lectures on The Heredity of Sex and some Cognate Problems. On *Tuesdays*, Jan. 14, 21, 28, Feb. 4, 11, 18.

Professor H. H. Turner, D.Sc. D.C.L. F.R.S., Savilian Professor of Astronomy, University of Oxford. Three Lectures on The Movements of the Stars: 1. The Nebular Hypothesis; 2. The Stars and their Movements; 3. Our Greater System. On *Tuesdays*, Feb. 25, March 4, 11.

SETON GORDON, Esq., F.Z.S., Author of "The Charm of the Hills." Two Lectures on BIRDS OF THE HILL COUNTRY. On *Thursdays*, Jan. 16, 23.

PROFESSOR B. HOPKINSON, M.A. F.R.S., Professor of Mechanism and Applied Mechanics, University of Cambridge. Two Lectures on RECENT RESEACH ON THE GAS ENGINE. On *Thursdays*, Jan 30, Feb. 6.

SIR SIDNEY LEE, D.Litt. LL.D., Editor of Dictionary of National Biography. Three Lectures on THE DAWN OF EMPIRE IN SHAKESPEARE'S ERA. On *Thursdays*, Feb. 13, 20, 27.

W. B. HARDY, Esq., M.A. F.R.S., Fellow and Tutor of Gonville and Caius College, Cambridge. Two Lectures on SURFACE ENERGY. On *Thursdays*, March 6, 13.

HENRY WALFORD DAVIES, Esq. Mus.Doc. LL.D., Organist and Director of the Choir at the Temple Church. Three Lectures on ASPECTS OF HARMONY: 1. CHORD PROGRESSION: ILLUSTRATED FROM HANDEL, BEETHOVEN, BRAHMS. 2. ADDED DISSONANCE: ILLUSTRATED FROM SCHUMANN AND WAGNER. 3. THE NEW WHOLE TONE CHORD AND ITS PREDECESSORS: ILLUSTRATED FROM BACH AND DEBUSSY. (With Musical Illustrations.) On *Saturdays*, Jan. 18, 25, Feb. 1.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.Sc. F.R.S. *M.R.I.*, Professor of Natural Philosophy, Royal Institution, etc. Six Lectures on THE PROPERTIES AND CONSTITUTION OF THE ATOM. On *Saturdays*, Feb. 8, 15, 22, March 1, 8, 15.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Journal, Vol. VII. Part 4. 8vo. 1912.

Lords of the Admiralty—Nautical Almanac, 1915. 8vo. 1912.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXI. 2° Semestre, Fasc. 8. Classe di Scienze Morali, Vol. XXI. Fasc. 5–6. 8vo. 1912.

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British Ornithologists' Union—List of Members, 1912. 8vo.

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Southern Vancouver Island. By C H. Clapp. (Mem. 13.) 8vo. 1912.

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Carnegie Institution—Contributions from the Mount Wilson Solar Observatory, No. 61. 8vo. 1912.

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- Athenæum for Nov. 1912. 4to.
- Author for Dec. 1912. 8vo.
- Canada for Nov. 1912. 8vo.
- Chemical News for Nov. 1912. 4to.
- Chemist and Druggist for Nov. 1912. 8vo.
- Concrete for Dec. 1912. 8vo.
- Dyer and Calico Printer for Nov. 1912. 4to.
- Electrical Engineering for Nov. 1912. 4to.
- Electrical Industries for Nov. 1912. 4to.
- Electrical Review for Nov. 1912. 4to.
- Electrical Times for Nov. 1912. 4to.
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- Engineering for Nov. 1912. fol.
- Ferro-Concrete for Nov. 1912. 8vo.
- Gardener's Chronicle for Nov. 1912. 8vo.
- Horological Journal for Nov. 1912. 8vo.
- Illuminating Engineer for Nov. 1912. 8vo.
- Journal of the British Dental Association for Nov. 1912. 8vo.
- Journal of Physical Chemistry for Nov. 1912. 8vo.
- Law Journal for Nov. 1912. 4to.
- London University Gazette for Nov. 1912. 4to.
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- New Church Magazine for Dec. 1912. 8vo.
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WEEKLY EVENING MEETING,

Friday, January 19, 1912.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Professor Sir James Dewar, M.A. LL.D. D.Sc. F.R.S. M.R.I.
Fullerian Professor of Chemistry, R.I.

Heat Problems.

[ABSTRACT.]

The approach to the absolute zero has rendered the problem of the determination of extremely low temperatures a very important one. Within certain limits resistance thermometers or thermo-couples give satisfactory results; but now that a temperature within 3° of the absolute zero has been attained, other methods must be resorted to, and the only reliable one is by the measurement of changes at constant volume due to temperature in the pressure of a gas under less than one-third of an atmosphere. The limits of temperature measurable by this method vary with different gases; helium, for example, giving accurate records to within 3° of the absolute zero.

Gas Thermometers.

A gas thermometer for lecture illustration is shown in Fig. 1. A is a copper sphere of a capacity of about 100 c.c., filled with pure hydrogen at a pressure of 273 mm., while immersed in melting ice. The free space, which is the volume of the uncooled narrow tubes required for connection to the manometer, is in this case so small that it may be neglected. A is placed in a metallic vacuum vessel B, containing liquid oxygen, and connected by a thick-walled narrow lead tube to a mercury manometer C. The apparatus is mounted on a movable frame D, so that the height can be adjusted for projection of the mercury levels. The change of pressure due to cooling the hydrogen in A is shown by the alteration in the levels of the mercury in the manometer. Liquid oxygen reduces the pressure to 90 mm., corresponding to 90° Abs., and if the liquid is exhausted through E, a pressure of 65 mm. is obtained; i.e. the temperature of the sphere is now 65° Abs., because with this sphere filled as described the difference of mercury-levels in millimetres is practically equal to the temperature of the sphere in degrees absolute.

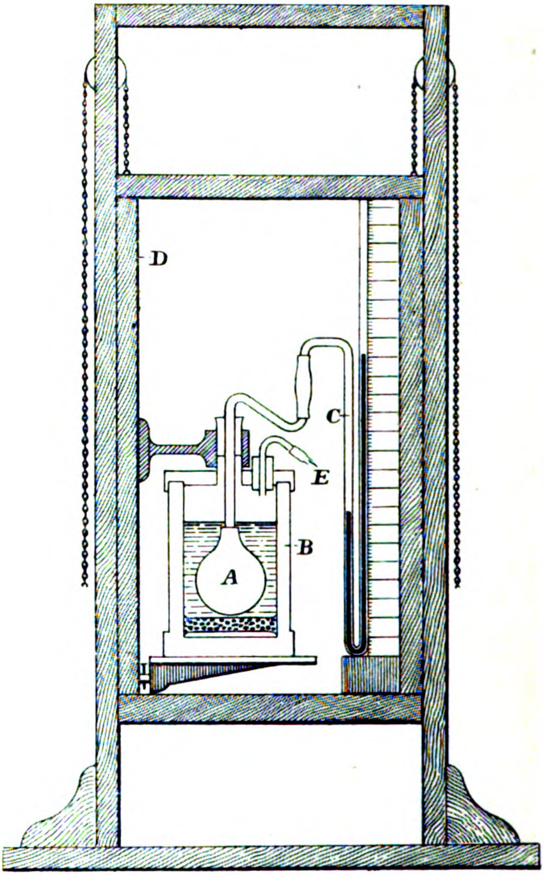

FIG. 1.

The following table gives the practical limits of gas thermometers filled with different gases at various initial pressures.

TABLE I.—PRACTICAL LIMITS OF GAS-THERMOMETERS.

Gas	Initial Pressure at 0 Deg. Cent. Millimetres of Hg.	Limit of Temperature to which Applicable. Deg. A.
CO_2	$\frac{1}{2} \times 273$	171·4
O	$\frac{1}{2} \times 273$	69·2
H	$\frac{1}{2} \times 273$	11·72
H	$\frac{1}{10} \times 273$	10·40
Helium	38×273	2·95
Helium	$\frac{1}{2} \times 273$	1·91
Helium	$\frac{1}{10} \times 273$	1·71

The lowest temperatures which can be attained with various cooling agents are of considerable interest. Gay Lussac showed that it was possible to freeze mercury by the evaporation of ice, and the experiment, arranged for projection on a screen, can be performed in the following manner (Fig. 2). A flat glass bulb, A, containing the mercury, is supported on a quill tube in the flask C, containing sulphuric acid. This vessel is connected through a side tube in the neck by means of a 3-way cock D to an exhaust pump, and a bulb of charcoal E. At each end of A some strands of wet cotton wool are fastened. The charcoal having been cooled in liquid air, the exhaust pump is turned on to C for a short time; and the

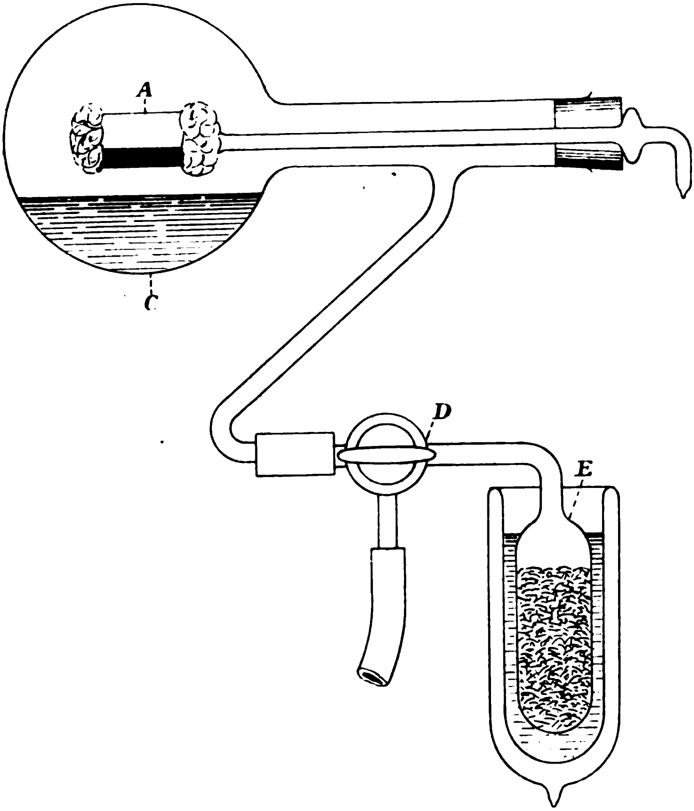

Fig. 2.

charcoal is then brought into play by a quarter turn of D. The consequent reduction of pressure on the ice in the cotton wool on A, and therefore of its temperature, is such that in two minutes the mercury at each end of the tube freezes, the centre portion remaining liquid ; in four minutes the mercury becomes solid, which may be seen when the bulb is rotated. By this method a temperature of − 70° C. can be reached. Experiments made with various bodies as cooling agents give the following limits of temperature :—

TABLE II.—LIMITS OF TEMPERATURE ATTAINABLE WITH VARIOUS COOLING AGENTS.

Solid	Absolute Temperature attainable. Deg.	Pressure in mm. of Hg.	Fraction of T_c
Ice	203	0·001	0·32
CO_2	132	0·63	0·43
O	51	0·19	0·33
N	37	0·7	0·30
H	9	0·59	0·29
Helium . . .	1·8	0·26	0·33
"X"	0·4	—	—

In this table X is a hypothetical body as volatile compared to helium, as helium is to hydrogen. With the aid of such a substance it may be inferred that a temperature of within ½° of the absolute zero should be reached. It will be seen from the table that the temperature attained was almost invariably the same fraction of the critical temperature of the body, a fact which further emphasizes the

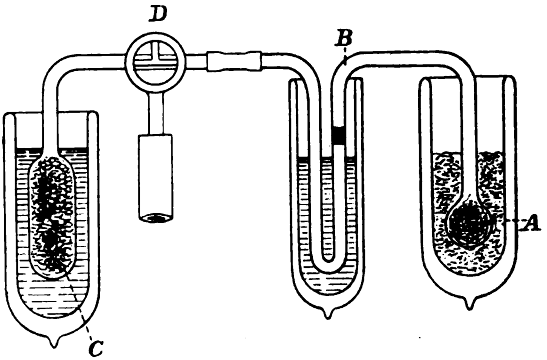

FIG. 3.

importance of the critical point. Other relations between the properties of a body and its critical constants are of interest, and are given in the following table :—

TABLE III.—CORRESPONDING TEMPERATURE RELATIONS.

Liquid coefficient of expansion varies as	$1/T_c$
Liquid coefficient of compressibility varies as	$1/P_c$
Liquid internal latent heat varies as .	T_c
Capillary constant varies as . .	P_c
Gas inversion point varies as . .	T_c
Volume of gas molecule varies as .	T_c/P_c
Maximum density varies as density at	T_c

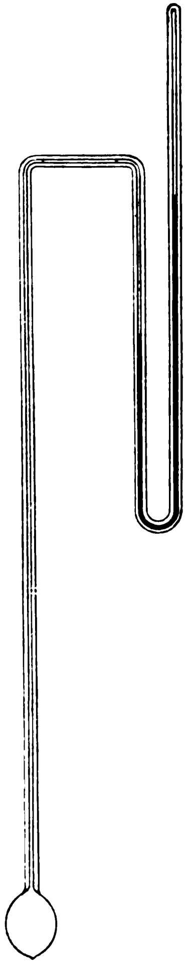

FIG. 4.

The vaporization of ice at $-78°$ C. can be demonstrated by the apparatus shown in Fig. 3. A is a small bulb packed in solid CO_2, filled with ice in the form of frozen water-saturated cotton wool, which gives a large surface. This vessel is connected to the U-tube B immersed in liquid air. The tube and the bulb are highly exhausted by an air pump connected at the 3-way cock D, and then by charcoal C cooled in liquid air. The deposit is first seen in the form of a ring in the limb of the U-tube nearest the bulb A. Vaporization is very slow, and takes almost an hour before the deposit is sufficient to be easily visible.

With gas thermometers working under small gas pressure reliable results can be obtained, provided the differences in the height of the mercury columns are read by means of a cathetometer, and proper precautions taken in filling with the gas. A simple form of gas thermometer with the volume of the bulb as small as 5 or 6 c.c. is shown in Fig. 4. In small apparatus, where a more bulky instrument could not be used, this form works in a satisfactory way, giving readings within 0·5°. when filled with helium or hydrogen under an initial pressure of 273 mm. at the melting-point of ice. The following diagram (Fig. 5) shows the type of the graphical curves given by such thermometers ; exhibiting a slight deviation from the theoretical straight line connecting absolute temperature and pressure. The various substances used in the calibration of such thermometers are marked on the curves. The deviations from the theoretical line are much exaggerated in the diagram. They are due to the variation in the volume of the free space in different thermometers. Thus, if P and p are pressure readings of the manometer at T° and 273° Abs. respectively, then $T^\circ = 273\frac{P}{p} - \frac{P(p - P)}{c}$ where c is a constant

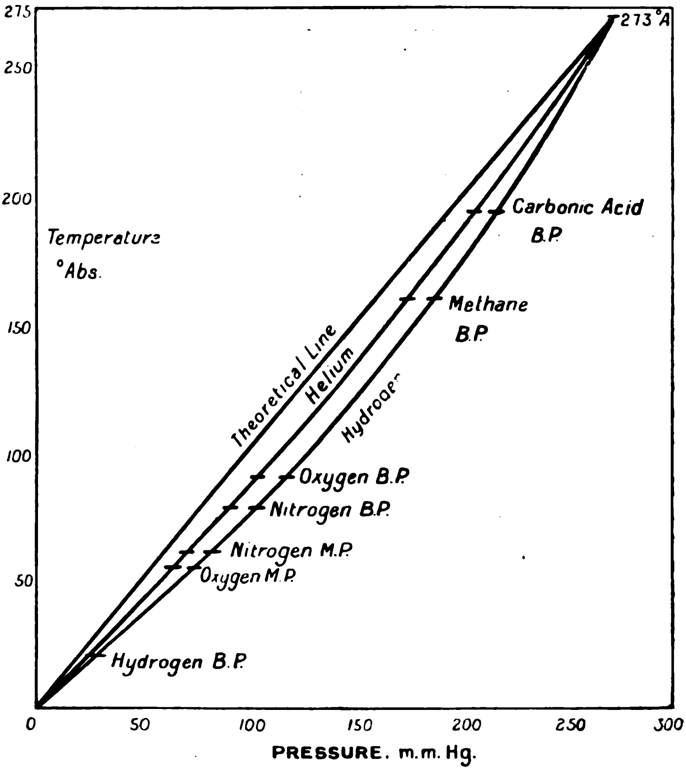

Fig. 5.

varying inversely as the ratio of the volume of the capillary to that of the bulb of the thermometer. Helium under a pressure of 273 mm. may be regarded as reliable as low as 3° Abs. If the pressure be reduced to 27·3 mm. at 273°, the limit of temperature is about 1°·7

Insulation by Powders.

In all low temperature work heat isolation is of the utmost importance. The double-walled glass vacuum flasks now so generally used for this purpose were first exhibited on this Lecture Table nineteen years ago. The efficiency of these vessels is much increased by silvering the inner wall to form a reflecting surface. Experiments made by filling the annular space with powdered charcoal or silica showed that while with some flasks the loss of heat could be reduced to about one-third, as a rule the results obtained were not better than with the use of the silvered vessels. This may be shown by the following experiment (Fig. 6). Three vacuum vessels, A, B, C, containing liquid air, are sealed to a common

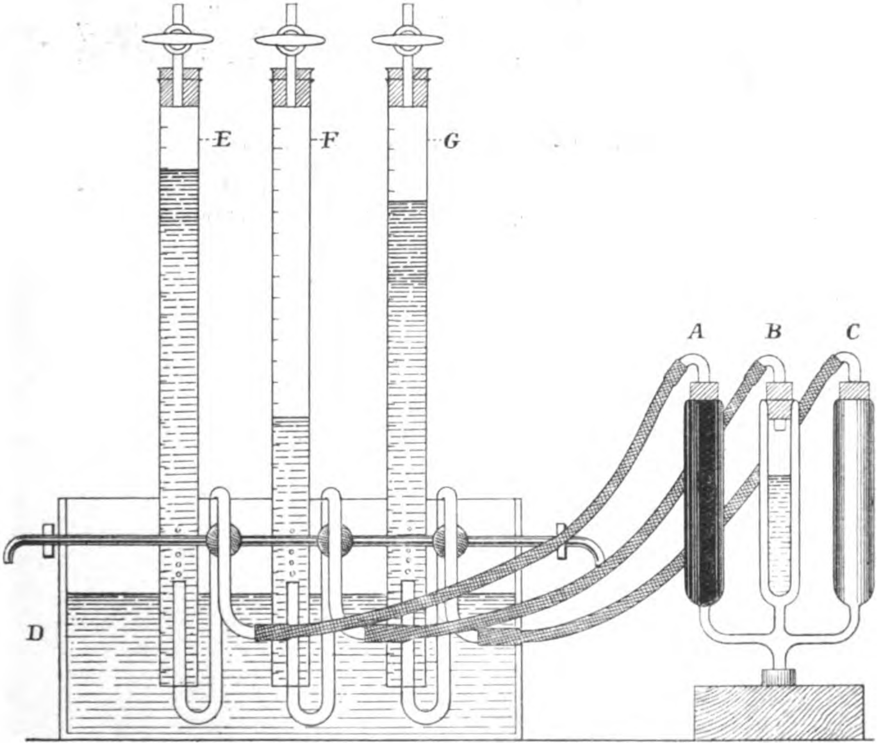

FIG. 6.

exhaust. B is of the ordinary unsilvered variety; A and C contain powdered charcoal and silica respectively in the annular space. The three tubes are fitted with glass delivery-tubes leading through rubber corks to a glass tank D filled with coloured water, containing three uniform collecting-tubes, E, F, G. The evaporated air from A, B, C is delivered simultaneously into these collecting-tubes. In about five seconds F, connected with the plain tube B, is almost full of air. The rate of evaporation from the three tubes may be judged by the relative volumes of air in E, F, G, the proportion found being about 2 : 10 : 3. While the manufacture of vacuum vessels is restricted to glass, probably no increased efficiency will be obtained by the use of powders as isolating agents, but if such flasks were made of fused silica the method might have considerable advantages. Silica flasks would withstand very high temperatures, which would, however, render the use of silver as a reflecting surface an impossibility; although nickel might be used. If such a flask could be made quite impervious to gases, which is extremely doubtful, it would be especially suitable for the isolation of red hot bodies.

LIQUID AIR AND HYDROGEN DROPS.

In a Friday Evening Discourse on "Liquid Hydrogen Calorimetry,"* the mode of manipulation of liquid gases for calorimetrical experiments was discussed. One fact which renders this simple method of calorimetry more difficult than it appears is the mobility

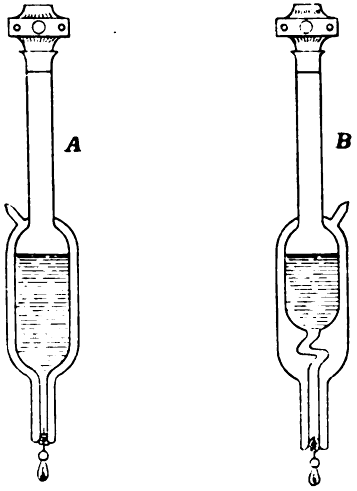

FIG. 7.

A shows the simple construction which can be used with the instrument made in quartz; if glass is used as in B, then a spiral has to be sealed in the exit tube, to take up the contraction caused by cooling to low temperatures.

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* Proc. Roy. Inst., xvii. p. 581.

of liquid air, and more especially of liquid hydrogen. When any solid is dropped into such calorimeters violent ebullition ensues, and either can be carried over with the evaporating gas, in the form of fine drops or spray, into the collecting jar, and so falsify the results. The possibility of this can be proved by showing that drops of liquid air will retain their state through a fall of from 30 ft. to 100 ft. in air. To illustrate this, special dropping pipettes were supported from the ceiling of the Lecture Room. The construction of the nozzle is shown in Fig. 7; it is filled with a wad of tightly packed cotton-wool, pierced with a coarse needle. By exercising a slight pressure on the liquid air above—a screw clip on a short length of rubber tube serves for this—the rate of dropping is easily regulated. Larger drops are obtained when a pin with a round glass head replaces the needle. The drops fall on a wet blackened board, and the impact is plainly audible and made visible by the surface of the board being obscured by a white mist as the drops rapidly evaporate. Such drops pass through a soap-bubble without bursting it. Hydrogen is remarkable in this respect, as the surface tension of liquid hydrogen is only

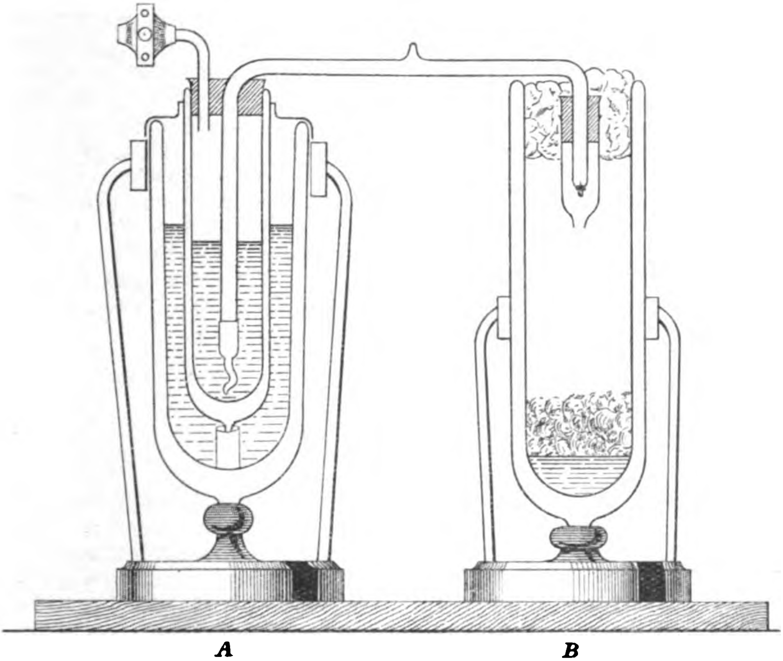

FIG. 8.

one-seventh that of liquid air, and its density one-fifteenth, while its temperature is only 20° Abs., i.e. one-fourth that of liquid air; still, small drops of the liquid are easily traceable through a fall of 8 feet, by the trail of frozen air caused by their passage. The formation of the liquid hydrogen drops requires a special form of silvered glass siphon (Fig. 8, A). The siphon is connected to a large silvered vacuum vessel containing the liquid hydrogen, fitted with a screw-cock for regulating the pressure, which controls the rate of dropping. The nozzle contains a loosely packed wad of wool, and to prevent fouling by condensation of air, it is surrounded by a piece of glass tube 4 inches long and 1½ inch in diameter, drawn out below to ½ inch, thus preserving an atmosphere of hydrogen about the nozzle. When the drops of liquid hydrogen are started, a succession of tiny vortex rings of frozen air are visible from the nozzle to the floor as they fall gently through the air. If the siphon be fitted to a vacuum vessel about 18 inches long (Fig. 8, B), so that the drops can fall on some liquid air previously placed in the vessel, a cloud of solid air quickly rises from the surface, rapidly settling again when the drops are shut off. If a slow rate of dropping is maintained, the liquid hydrogen drops are easily seen, producing little jets of condensed vapour as they dart rapidly about in the spheroidal state on the surface of the liquid air.

LIQUID AIR CALORIMETER.

Vacuum jacketed vessels have led to the construction of a simple form of gas calorimeter for the measurement of specific heats. The lecture form of the apparatus (Fig. 9) consists of a silvered vacuum vessel A containing liquid oxygen, fitted with an air-tight cap; a small delivery-tube B leads to an inverted gas jar C. The cap has a central vertical tube closed by a rubber cork D. A is supported in a metal vacuum pot E containing liquid air. An aluminium cup F rests on asbestos wool in the bottom of A. Normally there is no evaporation from A, but if a fragment of the substance of which it is desired to ascertain the specific heat, is dropped into F by momentarily removing D, a quantity of liquid oxygen is evaporated as gas, collected in C, and the amount measured, from which the specific heat of the body can be calculated. A sphere of lead, of weight proportional to its atomic weight, is taken as a standard, this element maintaining a nearly constant atomic heat, following the law of Dulong and Petit, for a considerable range of temperature. Pieces of carbon, aluminium, copper and bismuth, in atomic proportion to the lead sphere, all liberate smaller quantities of gas, especially carbon, where the value is found to be much below the average.

In all instances, however, the atomic heats of the elements fall below the normal value as the absolute zero is approached.

The mean specific heats of a few substances, for temperatures ranging from 20° to 80° Abs., made with the liquid hydrogen calorimeter, are given in the following table :—

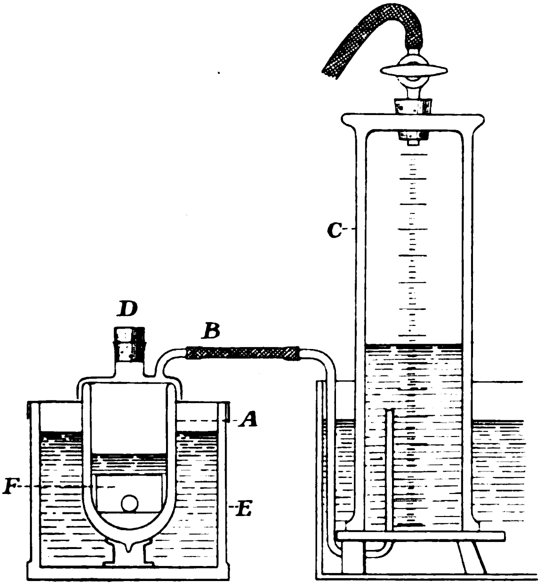

FIG. 9.

TABLE IV.—MEAN ATOMIC SPECIFIC HEATS BETWEEN 20 DEG. AND 80 DEG. ABSOLUTE.

Diamond	0·05
Graphite	0·17
Magnesium	1·71
Aluminium	1·12
Ice (molecule)	1·94

At the mean temperature of 50° Abs. it will be seen that the diamond has a very small specific heat, and all the other bodies in Table IV show a considerable decrease as compared with determinations made at ordinary temperatures. The theoretical curve of the change of atomic specific heat due to temperature on Einstein's theory, is shown (Fig. 10). The problem of specific heats has been extensively studied by Nernst, who has shown that they would probably disappear before the absolute zero is reached. This result has given rise to various theoretical speculations as to its cause, and in all probability the solution will be found in the development of the electron theory. At the temperature of boiling helium Kamerlingh Onnes has found that the electrical resistance of gold, mercury, and platinum vanish. Thus, pure metals lose both specific heat and electrical resistance below 4° Abs., suggesting that in this as yet unexplored region existing theories fail. The properties of matter at the absolute zero remains a purely speculative problem.

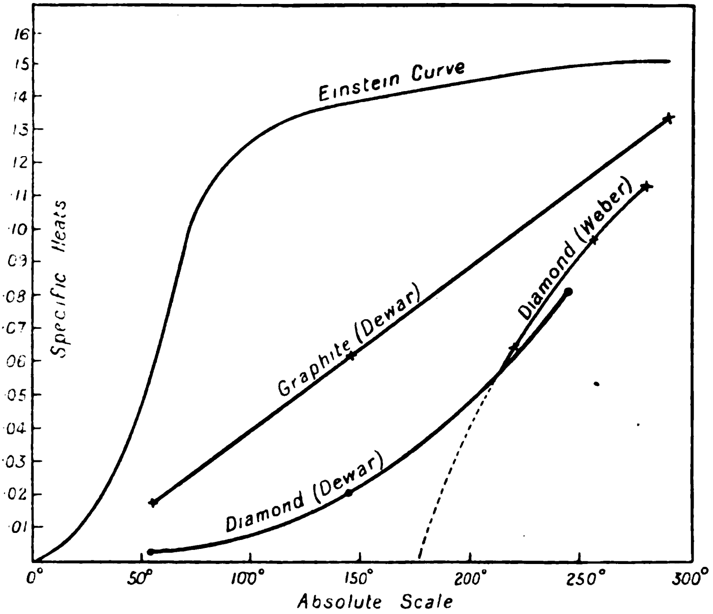

FIG. 10.

[J. D.]

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WITH

ABSTRACTS OF THE DISCOURSES

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VOLUME XX

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ROYAL INSTITUTION OF GREAT BRITAIN.

WEEKLY EVENING MEETING,

Friday, January 17, 1913.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.Sc. F.R.S. M.R.I., Professor of Natural Philosophy, Royal Institution.

Some Further Applications of the Method of Positive Rays.

THE method to which I shall refer this evening is the one I described in a lecture I gave here two years ago. The nature of the method may be understood from the diagram given in Fig. 1. A is a vessel containing the gases at a very low pressure; an electric discharge is sent through these gases, passing from the anode to the cathode C. The positively electrified particles move with great velocity towards the cathode; some of them pass through a small hole in the centre, and emerge on the other side as a fine pencil of positively electrified particles. This pencil is acted on by electric forces when it passes between the plates L and M, which are connected with the

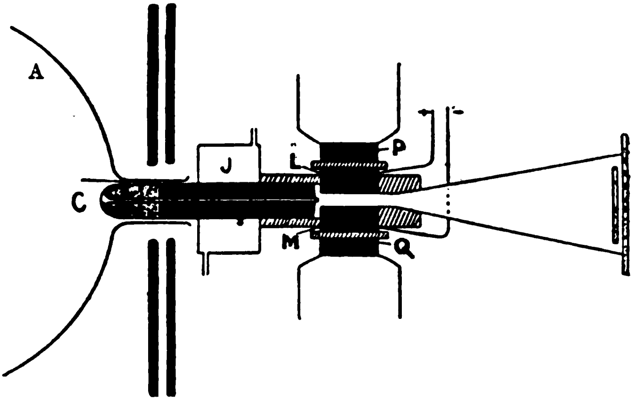

FIG. 1.

terminals of a battery of storage cells, and by a magnetic force when it passes between P and Q, which are the poles of an electro-magnet. In the pencil before it passed under the influence of these forces there might be many kinds of atoms or molecules, some heavy, others light, some moving quickly, others comparatively slowly, but these would all be mixed up together. When they are acted on by the electric and magnetic forces, however, they get sorted out, and instead of travelling along the same path they branch off into different directions. No two particles will travel along the same path unless they have the same mass as well as the same velocity; so that if we know the path of the particle we can determine both its mass and its velocity. In chemical analyses we are concerned more with the mass than with the velocity, and we naturally ask what is the connexion between the paths of particles which have the same mass but which move with different velocities. The answer is that all such paths lie on the surface of a cone, and that each kind of particle has its own cone; there is one cone for hydrogen, another for oxygen, and so on. Thus one cone is sacred to hydrogen, and if it exists there must be hydrogen in the vessel; so that if we can detect the different cones produced from the original pencil, we know at once the gases that are in the tube. Now, there are several ways of identifying these cones, but I shall only refer to the one I have used in the experiments I wish to bring before you this evening. These moving electrified particles, when they strike against a photographic plate, make an impression on the plate, and a record of the place where they struck the plate can be obtained. Thus, when a plate is placed in the way of the particles streaming along these cones, the sections of these cones by the plate (parabolas) are recorded on the photograph, hence we can identify these cones by the parabolic curves recorded on the photograph, and these parabolas will tell us what gases are in the vessel.

The first application of the method which I shall bring before you this evening is to detect the rare gases in the atmosphere. Sir James Dewar kindly supplied me with two samples of gases obtained from the residues of liquid air; the samples had been treated so that one might be expected to contain the heavier gases, the other the lighter ones. I will take the heavier gases first. The photograph for these is shown in Fig. 2. When the plate is measured up it shows a faint line corresponding to the atomic weight 128 (xenon); a very strong line corresponding to the atomic weight 82 (krypton), a strong argon line 40 (argon), and the neon line 20. There are no lines unaccounted for, and hence we may conclude that in the atmosphere there are no unknown gases of large atomic weight occurring in quantities comparable with those of xenon or krypton. This result gives an example of the convenience of the method, for a single photograph of the positive rays reveals at a glance the gases in the tube. I now turn to the photograph of the lighter constituents shown in Fig. 3; here we find the lines of helium, of neon (very strong), of argon, and in addition there is a line corresponding to an atomic weight 22, which cannot be identified with the line due to any known gas. I thought at first that this line, since its atomic weight is one-half that of CO_2, must be due to a carbonic acid molecule with a double charge of electricity, and on some of the plates a faint line at 44 could be detected. On passing the gas slowly through tubes immersed in liquid air the line at 44 completely disappeared, while the brightness of the one at 22 was not affected.

The origin of this line presents many points of interest; there are no known gaseous compounds of any of the recognized elements which have this molecular weight. Again, if we accept Mendeleef's Periodic Law, there is no room for a new element with this atomic weight. The fact that this line is bright in the sample when the neon line is extraordinarily bright, and invisible in the other when the neon is comparatively feeble, suggests that it may possibly be a compound of neon and hydrogen, NeH_2, though no direct evidence of the combination of these inert gases has hitherto been found. I have two photographs of the discharge through helium in which there is a strong line, 6, which could be explained by the compound HeH_2, but, as I have never again been able to get these lines, I do not wish to lay much stress on this point. There is, however, the possibility that we may be interpreting Mendeleef's law too rigidly, and that in the neighbourhood of the atomic weight of neon there may be a group of two or more elements with similar properties, just as in another part of the table we have the group iron, nickel, and cobalt. From the relative intensities of the 22 line and the neon line we may conclude that the quantity of the gas giving the 22 line is only a small fraction of the quantity of neon.

Let me direct your attention again to the photograph of the heavier gases in the atmosphere. You will notice that the parabolas corresponding to many of the elements start from points which are all in the same vertical line; this indicates that the atoms or molecules which form these parabolas all carry the same charge. Several of these lines, however, do not follow this rule; you will notice, for example, that the neon line has a prolongation which comes nearer than the normal line to the vertical line drawn through the undeflected spot. Measurement of the photograph shows that the neon line begins at a distance from this vertical line which is only half the normal distance; this shows that some of the neon atoms in the positive rings possess two charges of electricity; the majority of them, however, only possess one. If you examine the argon line you will find that it comes even nearer to the vertical than the neon line: in fact, it begins at a distance from the vertical only one-third of the normal distance; this proves that the argon atom can have as many as three charges of electricity. If now you examine the krypton line you will find that it comes nearer to the vertical line than even the argon; its least distance is one-fourth of the normal distance, showing that the krypton atom may have as many as four charges. The mercury line comes so close to the vertical line that it is only on large photographs that it can be seen that there is in reality an interval; this interval is only one-eighth of the normal interval, showing that mercury may acquire eight positive charges, i.e. that it may lose eight corpuscles. The mercury atom when it is on this line must have only the normal charge, i.e. it must have regained all but one of the corpuscles it previously lost; if it had retained two positive charges it would have been on the line corresponding to the atomic weight 200/2 or 100; if it had retained 3, or 4, 5, 6, 7, 8, on the lines corresponding to the atomic weights, 200/3, 200/4, 200/5, 200/6, 200/7, 200/8 respectively. All these except the last have been detected on the plate. The lines corresponding to the multiple charges on krypton, argon, and neon have also been detected. It appears, then, that in a vacuum tube a mercury atom, for example, may be ionized in two ways; in the one way the atom loses one corpuscle, in the other it loses eight. I would suggest that these two types of ionization may result from the two different types of collision which the atom must experience. The first type is collision with a corpuscle; since the corpuscle is an exceedingly small body moving with a very great velocity, it can pass freely through the atom, and the collision it makes with the atom is really a collision with a corpuscle inside the atom; this may result in the corpuscle it strikes acquiring such a great velocity that it is able to escape from the atom; this type of collision will result in the detachment of a single corpuscle. The second type of collision is when the atom collides with another atom and not with another corpuscle; the result of this collision may be that the atom suffers a sudden change in its velocity. This change is not at first shared by the corpuscles, so that these just after the collision may have a very considerable velocity relative to the atom. If there are several corpuscles which are comparatively loosely attached to the atom, these may all be detached from it and leave it with a positive charge corresponding to the number shaken out. It is this type of collision which we regard as giving the multiply-charged ions, and we see that the magnitude of the charge is a measure of the number of corpuscles in an atom which are readily detachable from it. We have seen that the greater the atomic weight the greater the charge it can acquire, the maximum charge being roughly proportioned to the square root of the atomic weight, hence the heavy elements have a larger number of detachable corpuscles than the lighter ones.

Another application of the method I should like to bring before you is the use of it for the discovery and investigation of a new substance. I have in previous lectures said that sometimes there appeared on the plates a line corresponding to a particle with an atomic weight 3; this must either be a new element of a polymeric modification of hydrogen, represented by H_3. The other possibility that it is a carbon atom with four charges is put out of court by the fact that it frequently occurs when the carbon line is exceedingly faint, and when there is not a trace of a carbon atom with even two charges, though the doubly-charged carbon atom occurs readily under certain conditions. In addition to this, the carbon atom parabola never approaches the vertical near enough to allow of its having four charges. I thought the study of the substance producing this line would be of interest, and I have for some time been working at it; and although the research is by no means completed, I have obtained some results which I should like to bring before you.

At first I was greatly hindered by not knowing the conditions under which the line occurred; although it appeared from time to time on the plates, its appearance was always fortuitous, and sometimes for weeks together the plates would not show a trace of the line. The line sometimes appeared, but why it did so was a mystery, and I could not get it when I wanted it. I began an investigation, which proved long and tedious, to find the conditions under which the line appeared. I tried filling the discharge-vessel with all the gases and vapours described in the books on chemistry without success. At last I tried bombarding various substances with cathode rays. Under this treatment the substances give off considerable quantities of gas the greater part of which is hydrogen, carbonic acid or carbon monoxide. When I came to analyse by the positive rays the gases given off in this way, I found that with a large number of substances these gases contained the substances giving the three lines, so that I was now in a position to get this line whenever I wanted it, and investigate the properties of the gas to which it owes its origin. The question of the gases absorbed and given off by solids is an extremely interesting one, and a considerable number of investigations have been made on it. In all these, as far as I know, the method has been to heat the solid to a high temperature, and then measure and analyse the very considerable amount of gas which is driven off by the heating. As far as I know, no experiments have been made in which the gases were driven off by bombardment with cathode rays. This treatment, however, will cause the emission of gas even when ordinary heating fails to do so.

Belloc, who has recently published* some interesting experiments on this subject, after spending about six months in a fruitless attempt to get a piece of iron in a state in which it would no longer give off gas when heated, came to the conclusion that, for practical purposes, a piece of iron must be regarded as an inexhaustible reservoir of gas. There are some interesting features about the emission of gas from a heated solid. If the body is kept for a long time in a vacuum at a high temperature, the emission of gas becomes too small to be detected; if after this treatment the temperature is raised considerably, there will be a further copious emission of gas, which again diminishes as the heating continues. After it has fallen to zero, all that is necessary is to raise the temperature again and you will get a fresh supply of gas; and as far as my experience goes, after you have got all the gas you can out of the solid by heating it, you have only to expose it to cathode rays to get a fresh outburst. This effect of increased temperature in renewing the stream of gas from the solid seems to me to be too large to be accounted for merely by an increase in the rate of diffusion of the absorbed gas from the interior to the surface; it seems to be more analogous to the case of the emission of the water of crystallization from some salts. There are some salts, for example, copper sulphate, which when heated lose their water of crystallization in stages; thus, if the temperature is raised to a certain value, some of the water of crystallization comes off, but the rest remains fixed, and you may keep the salt at this temperature for ever without getting rid of all the water of crystallization; on raising the temperature, however, fresh water of crystallization is given off.

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* Ann. de Chimie et de Physique [8], xviii. p. 569.

Something of this kind seems to take place in the case of gases absorbed in metals, and there seem to be indications that there is some kind of chemical combination between the gas and the metal. This absorbed gas may influence the behaviour of the substance. For example, an ordinary carbon filament gives off, when raised to a white heat, large quantities of negatively electrified corpuscles; but Pring and Parker * have shown that when great precautions are taken to get rid of the absorbed gas, the emission of these corpuscles falls to less than one-millionth of their previous value. It is in the gases given off by certain metals when they are bombarded by cathode rays that I have found an unfailing source of the substance, which I shall denote by X_3, giving the line corresponding to the atomic weight 3. The arrangement I have used for investigating the presence of this gas is shown in Fig. 4. A is a vessel communicating with the bulb B in which the positive rays are produced by two tubes, one of which is a very fine capillary tube, while the other one is five or six millimetres in diameter; taps are inserted so that one or both of these vessels can be closed, and the vessels A and B isolated from each other. A is provided with a curved cathode such

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* Phil. Mag., xxiii. p. 192.

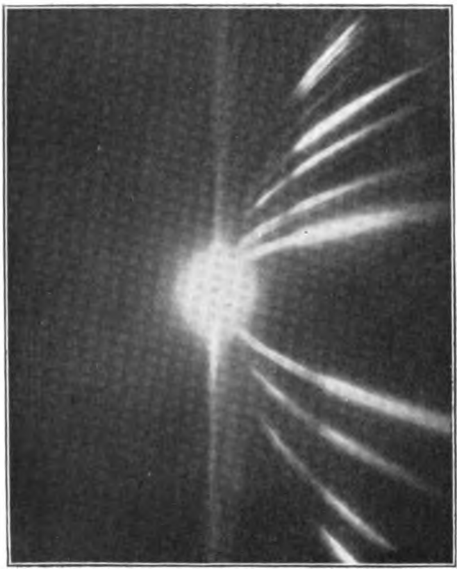

FIG. 2.

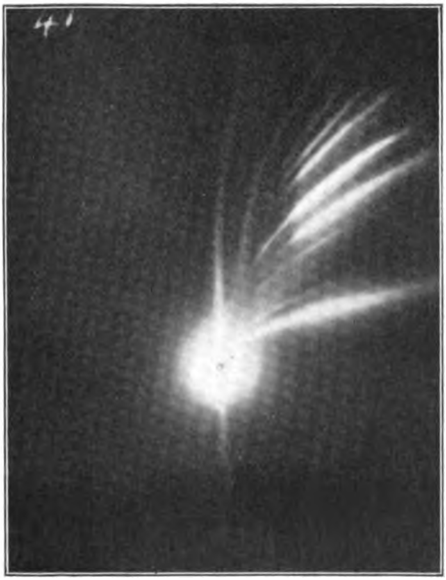

FIG. 3.

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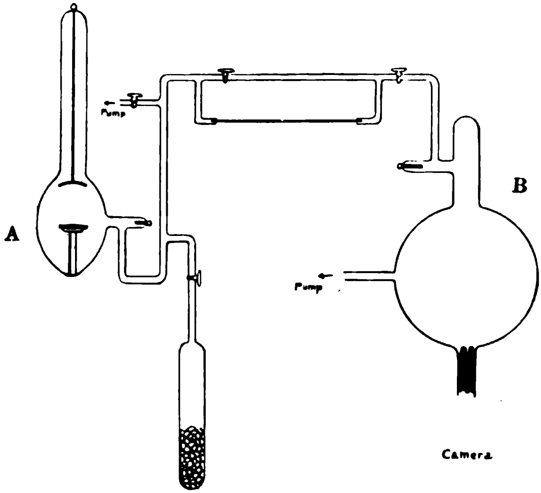
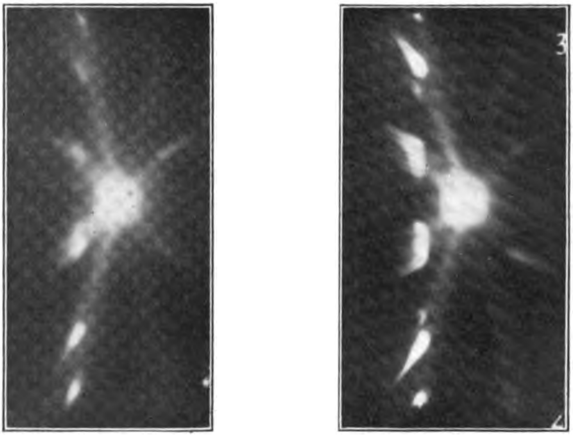


Fig. 4.



a Fig. 5. *β*

as are used for Röntgen ray focus tubes, and the cathode rays focus on the platform on which the substance to be bombarded is placed. [It is not absolutely necessary to focus the cathode rays in this way, but it makes the supply of the gas X_3 more copious.] After the metal or other solid to be examined has been placed on the platform, the taps between A and B being turned so as to cut off the connexion between them, A is exhausted until the vacuum is low enough to give the cathode rays; the discharge is then sent through A, and the cathode rays bombard the solid. The result of this is that in a few seconds so much gas, mainly CO_2 and hydrogen, is driven out of it that the pressure gets too high for the cathode rays to be formed, and unless some precautions to lower the pressure were taken the bombardment would stop. To avoid this, a tube containing charcoal cooled by liquid air is connected with A, and this absorbs the CO_2 and enough of the hydrogen to keep the vacuum in the cathode ray state. To see what new gases are given off in consequence of the bombardment, a photograph is taken while the connexion between A and B is cut off. After this is finished, and when the bombardment has gone on for about four hours, the tap is turned and a little of the gas from A is allowed to go into B; another photograph is taken, and those lines in the second photograph which are not in the first represent those gases which are liberated by the bombardment, and which have escaped being absorbed by the charcoal. I have here a slide (Fig. 5) representing the result of bombarding nickel. There are two photographs, one (α) before turning the tap and the other (β) after; in the second you see the three line very distinctly, while it is absent from the first, showing that the gas giving the three line has been liberated by the bombardment. I have got similar results to these when, instead of nickel, iron, copper, lead, zinc have been bombarded. I have tried two specimens of meteorites kindly lent to me from the Mineralogical Museum, Cambridge, and found there the three line. Nearly every substance I have tried gives, the first time it is bombarded, the helium line as well as this line due to X_3; if, however, the same substance is bombarded a second time, the helium line is in general absent (occasionally it is still to be detected, though exceedingly faint); and on the third bombardment is invisible in all the substances I have tried except monazite sand, where it is given off in exceedingly large quantities as long as the bombardment continues. It is remarkable that monazite sand, which contains so many elements, gives no trace of the three line when bombarded.

I have also obtained the X_3 line and also the helium line when the tube A was replaced by one containing a Wehnelt cathode; with this the current of cathode rays through the tube was much larger than with the other cathode, though the velocity of the rays was smaller. The Wehnelt cathode gives the line without placing pieces of metal in the tube, so that in this case nothing is bombarded by the cathode rays but the glass walls of the tube ; the strip of metal forming the cathode is, however, bombarded by the positive rays.

The three line when present at all continues even though the bombardment is very prolonged. In some cases the bombardment has been prolonged for twenty hours, and at the end of that time the line seemed almost as bright as at the beginning ; indeed I could not feel certain that there was any difference. This might lead one to suspect that X_3 was manufactured from the lead or other metal by the bombardment rather than stored up in it, and this view might be regarded as receiving some support from the fact that very little of the X_3 is liberated by heating. The following experiment is an illustration of this. I took a piece of lead, and instead of bombarding it with cathode rays I placed it in a quartz tube connected with vessel A, and heated the tube to a bright red-heat for several hours. Large quantities of CO_2 and hydrogen were driven off by this process; this was absorbed by charcoal, and the residual gases, which had accumulated in A, were admitted into the vessel B ; the X_3 line and helium line could just be detected, and that was all. I then gave the lead a second heating, this time raising the temperature until the quartz was on the point of softening. The lead was boiling vigorously ; the heating was kept up for about three hours. In this time about three-quarters of the lead had boiled away. I then let the gases which had been given off at the second heating into the vessel B, and took another photograph ; no trace of the line due to X_3 or helium could be detected. The fraction of the lead which had not been boiled away was now placed in A and bombarded by cathode rays. It now gave the three line quite distinctly ; the helium line was visible, but faint. By the bombardment with the cathode rays the lead was only just melted, so that the average temperature was much less than when it was heated in the quartz tube. This rather suggests that the X_3 might be due to a kind of dissociation of the metal by the cathode rays, and not to a liberation of a store of that substance. Another experiment shows, however, that for lead, at any rate, this view is not tenable. I took some lead which had just been deposited from a solution of lead acetate by putting a piece of zinc into the solution, and forming the well-known lead-tree. When I bombarded this freshly precipitated lead, I could get no trace of the X_3 line ; the helium line, too, was absent. I then tried another experiment. I took a piece of lead and divided it into two parts. The first of these I bombarded by the cathode rays : it gave the X_3 line quite distinctly. The other part I dissolved in boiling nitric acid, getting lead nitrate. The nitrate was heated and converted into oxide, and this was bombarded by the cathode rays : it did not give the X_3 line, showing that the X_3 is not produced by the bombardment, but is something stored up in the lead, which can be detached from it when the lead is dissolved. I have tried several samples of lead; the one which gave the X_3 line most distinctly was a piece of lead from the roof of Trinity College Chapel, several hundred years old. A sample of Kahlbaum's chemically pure lead, which must, I suppose, at no distant date have been subjected to severe ordeals by fire and water, showed the line quite distinctly, though not so well as the older lead. I have tried similar experiments with iron, and found that iron which gave the three line very distinctly ceased to do so after it had been dissolved in acid.

As the most obvious explanation of X_3 is that it is H_3, bearing the same relation to hydrogen that ozone does to oxygen, and produced in some way from the hydrogen dissolved in the metal, I tried if I could produce it by charging metals with large quantities of hydrogen, and then seeing if the hydrogen coming from the metal gave any traces of H_3. Thus, for example, I tested the hydrogen given off from hot palladium, but found no trace of X_3. I then charged nickel at a temperature of about 355° C. with hydrogen in the way recommended by Sabatier, but found no increase in the brightness of the X_3 over nickel that had not been deliberately exposed to hydrogen. I tried if the brightness of the line would be increased by adding hydrogen to the bulb A, in which the bombardment took place, but found no effect. I also tried adding oxygen to this bulb, thinking that if it was H_3 it would combine with the oxygen, and thus be eliminated, but no great diminution in the intensity was produced by this treatment. The gas seems quite stable, at least it can be kept for several days without suffering any diminution that can be detected; indeed, when once it has got into a bulb, there is considerable difficulty in getting the bulb free from it. It must be remembered, too, that by the method it is produced the gas is subjected all the time to electric discharges which would break it up unless it possesses very great stability. Thus if X_3 is a polymeric modification of hydrogen, it must possess the following properties :—

1. It must be very stable.
2. It must resist the action of oxygen.
3. It must not be decomposed by long-continued exposure to the electric discharge.

These are properties which a priori we should hardly have expected an allotropic modification of hydrogen to possess.

Mendeleef predicted the existence of an element with an atomic weight 3. According to him this element should be intensely electro-negative and possess the properties of fluorine to an exaggerated extent. The gas X_3 can, however, be kept in glass vessels, which we should not expect to be possible if it possessed more than fluorine's power of combining with glass. I prefer to defer expressing any opinion as to the actual nature of the gas until I have had the opportunity of making further experiments upon it. It is only about two months ago that I found how to get the gas with any certainty, and, as the method involves long bombardments, each experiment takes a considerable time. This has prevented me from making several experiments which suggest themselves, and which ought to be made before coming to a final decision. I thought, however, that the investigation, though incomplete, might not be unsuitable for a Friday evening discourse, as the gas, whatever its nature, is certainly one of considerable interest, and its detection illustrates the delicacy of this new method.

[J. J. T.]

WEEKLY EVENING MEETING,

Friday, January 24, 1913.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

PROFESSOR JOHN OLIVER ARNOLD, D.Met. F.R.S.

Recent Advances in Scientific Steel Metallurgy.

IN having to address this distinguished audience to-night, I find myself in a somewhat disconcerting position. Last Friday you listened to a discourse from one of the world's greatest exponents of pure science. As an exponent of applied science, I realize my limitations, and warned by the fate of Icarus, I shall not attempt to emulate Sir J. J. Thomson in his classical, or shall I say Ionic, flights into ethereal regions. Nevertheless, I have to get somewhere, and will do my best. Perhaps with some accuracy I may obliquely describe my position by a reference to the student of physics who was asked to define scientifically the terms transparent, translucent, and opaque. He replied that if he attempted a very scientific answer he was afraid he might not satisfy the examiners. He could, however, convince them that he understood the meaning of the terms by a practical example. "For instance," he said, "the windows of this examination room were once transparent, they are at present translucent, but if not speedily cleaned, they will soon be opaque."

To render clear the exact nature of certain modern scientific advances in steel metallurgy, it is necessary briefly to consider what is known of the past history of steel, more particularly with reference to cutting implements, whether for the purposes of peace or war. That steel (or to be more accurate probably steely-wrought iron) was known to the ancients, say 3000 years ago, seems to be proved by a passage translated by Pope from the ninth book of Homer's "Odyssey":—

> "And as when armourers temper in the ford
> The keen-edged pole-axe, or the shining sword,
> The red-hot metal hisses in the lake,
> So in his eyeballs hissed the plunging stake."

As has been truly remarked by Roscoe and Schorlemmer, the above description can be applied only to steel, that is to say, to iron containing a very considerable percentage of carbon.

So far as definite records are concerned, the story of early British steel metallurgy is wrapped in profound obscurity, and its history can be only indirectly surmised from collateral historical evidence. About 60 A.D. a great British army under the command of Princess Boadicea stormed the Roman camp at Colchester and annihilated the Ninth Legion. She then marched on St. Albans and London, and in both places put the garrisons and the Roman colonists to the sword, the stake, or the cross. Tacitus, the Roman historian, records that the losses of the Romans and their allies in these battles reached the startling total of 70,000 people. In the subsequent campaign, which ended in the defeat and death of the heroic British Queen, the same historian states that the British lost 80,000 persons. It is evident, therefore, that Boadicea must have commanded at least 100,000 British troops, or she could never have undertaken such extensive and formidable military operations. It is also clear that these troops were armed with swords and spears, to say nothing of the scythes attached to the axles of their war chariots. There is no reason to suppose that these weapons were not of native manufacture. They would be made partly of bronze and partly of steely-iron, since the country had been for a century more or less occupied by Roman soldiers and artizans. It is therefore almost certain that in the first century the manufacture of steely-iron weapons and implements would be on a fairly large scale, and would doubtless mainly be concentrated in iron-ore and charcoal-producing districts—such as Sussex and the Forest of Dean.

In connexion with Sheffield — now the greatest British steel centre—the earliest written record refers to the twelfth century, and states that in 1160 the monks of Kirkstead Abbey had somewhat extensive works at Kimberworth, near Sheffield, manufacturing wrought and, no doubt, steely irons. In 1386 Chaucer, in "The Reve's Tale," in describing a miller of the time of Edward III., wrote, " A Shefeld thywtel bare he in his hose." Since 1386 Sheffield steel in the form of table-knives has been in almost everybody's mouth. In 1590 Peter Bales, "The Writing Schoolmaster," recommends Sheffield razors and penknives for the cutting of quill-pens. It is obvious that for this purpose fine steel carrying a perfect cutting edge is necessary, and was being made at Sheffield prior to 1590. Hunter states that in 1615 Sheffield workmen could make armour only fit for the common man-at-arms. The armour for knights was imported from Spain and Italy. Scott, in "Ivanhoe," embodies this fact in his description of the siege of " Torquilstone "—

> " Thrice did Locksley bend his shaft against De Bracy, and thrice did his arrow bound back from the Knight's armour of proof. 'Curse on thy Spanish steel coat,' said Locksley. 'Had English smith forged it, these arrows had gone through an as if it had been silk or sendal.' "

The opening scene in "Ivanhoe" was near Woodhouse (5 miles east of Sheffield), where until quite recently wrought-iron was manufactured at the Rotherwood Iron Works.

In 1760, Horace Walpole, writing to George Montague, remarks, "I passed through Sheffield, which is one of the foulest towns in England in the most charming situation. There are two-and-twenty thousand inhabitants making knives and scissors. They remit eleven thousand pounds a week to London. One man there has discovered the art of plating copper with silver. I bought a pair of candlesticks for two guineas, that are quite pretty."

Antiquarians express the opinion that the remarkable concentration of the cutting steel industry round Sheffield was due to the juxtaposition of coal and iron ore in the district. This reason, however, is quite unconvincing to metallurgists, first, because charcoal and not coal was used, and, second, because the local ore produces an iron high in phosphorus from which it is practically impossible to make cutting implements of fine steel. There is little doubt that the main factor which originally determined the location of the chief British steel industry at Sheffield was the unique situation of the town in a hollow near the confluence of four rivulets into the Don. Along these streams running down the valleys of the Sheaf, the Porter, the Rivelin and the Locksley, the old Sheffield steel-workers could, by the construction of numerous dams, get water power for their forging hammers and grinding wheels at a small cost, and water wheels worked by some of these dams are still in operation along these valleys, and that of the Don itself actuating tilt-hammers and grindstones.* The latter are made from the carboniferous sandstones of the district. There is proof positive that the basis metal, consisting of nearly pure iron, from which the best Sheffield cutting steels are still made, was being imported into the town in the sixteenth century from abroad.

The slide on the screen reproduces entries abstracted from the accounts of The Sheffield Church Burgesses for the year 1557.

> "Paid to Robert More for one stone and quarter of Danske Yron XXIId.
> Paid to ye same Robt. for X lib of Spanysche Yron XV."

In modern money the cost of this raw material works out to at least £60 per ton, or £3 per cwt.† The Danish (Danske) iron was probably Swedish, just as at present much of the Danish butter imported comes from Swedish dairies.

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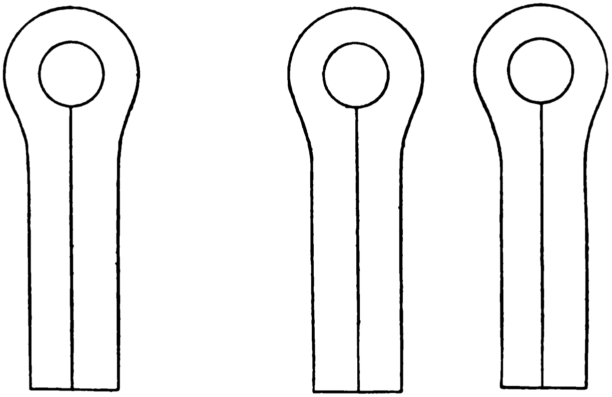
* There is evidence in old documents that the name Sheffield may be a corruption of "Escafeld," meaning "the field of waters."

† Professor Thorold Rogers, in his Oxford Lectures, 1888–9, stated that about 1685, using a multiplier of 2, the value in modern money of English wrought iron was about 73*l.* per ton. The Sheffield record, however, proves beyond doubt that in 1557, *or more than a century-and-a-quarter earlier*, the imported and superior Spanish and Swedish irons were commanding in Sheffield, retail, not more than 14*l.* per ton, which, using a multiplier of 4·5, is equivalent in present money to 63*l.* per ton.

In connexion with the early importation of pure Swedish or Spanish iron for a basis metal, it is significant that in 1442 Sheffield obtained a Royal warrant to construct tow-paths to make the River Don navigable. This river runs into the Humber at Goole, and there is little doubt that as early as the fifteenth century Sheffield steel-makers were endeavouring to replace the costly pack-horse transit of foreign raw materials by cheaper water carriage from the Humber. It is next of interest to consider how, during the fourteenth, fifteenth, sixteenth, seventeenth, and half the eighteenth centuries, Sheffield made all its fine steel. It seems almost certain that the nearly pure imported Swedish or Spanish irons were carburized "in the dry way" by cementation in charcoal at a yellow heat.



Brand of
Single Shear Steel.

Brand of
Double Shear Steel.

FIG. 1.

The slide on the screen shows the highly ductile bar iron and the blistered and brittle steel resulting from its cementation-carburization. The blister bar was then made into what, for perhaps two hundred and fifty years, has been known as "Shear Steel."

The next slide shows the method of producing from blister bar both single and double shear steel. The origin of the name "shear steel" was due to the fact that British cloth-workers insisted on having this fine quality of steel for their cloth cutting shears, and this material is still branded with rude representations of clothier's shears, as shown by the slide on the screen (Fig. 1). One pair of shears signifies single shear, and two pairs double shear steel. The chemical composition of this steel, which is the purest made, is exhibited on the screen. With its high reputation built up during

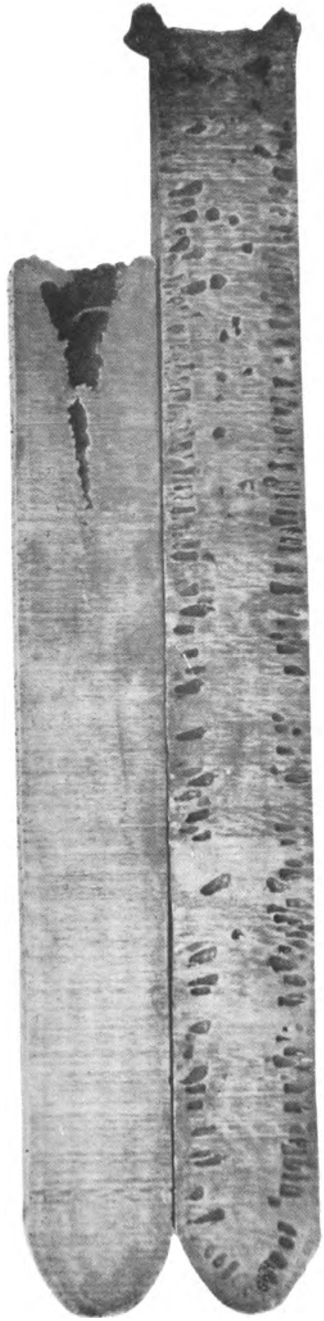


Fig. 2.

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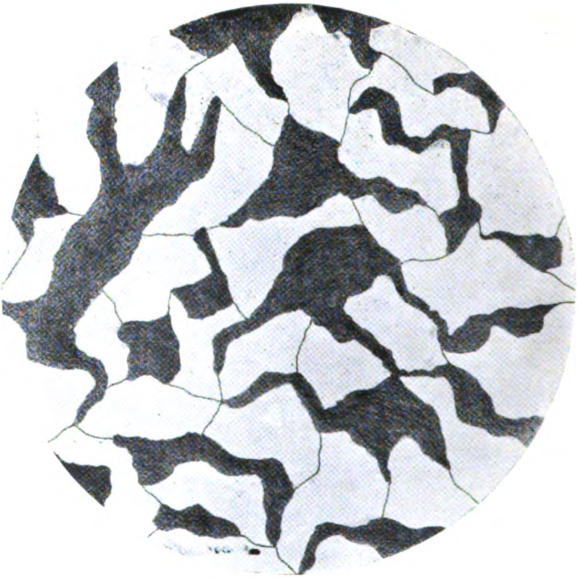
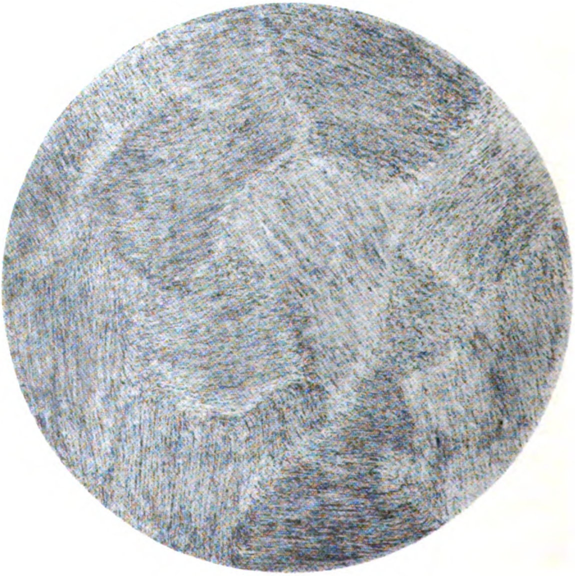


Carbon 0·38 per cent.

FIG. 3.
(Magnified 600 diameters.)



Carbon 0·89 per cent.

FIG. 4.
(Magnified 600 diameters.)

centuries, this material has naturally had its name branded on inferior kinds of steel. Indeed, bars of steel up to 6 inches in diameter have been sold as "shear steel," at 18*s*. per cwt., the price of the raw material from which shear steel is manufactured. Probably a bar 1½ inch in diameter marks the advisable limit of size for genuine shear steel, and its average market price is about 45*s*. per cwt.

The year 1740 marked for Sheffield, and indeed for the world, the beginning of an epoch of great metallurgical importance. Benjamin Huntsman, a well-known clock-maker of Doncaster, found that shear steel, on account of its sometimes varying temper and of its weld-lines, often presented uneven hardness and exasperating flaws when made into clock springs. He consequently determined to make a steel even in texture and free from weld flaws. He experimented successfully and worked out a method for the production of sound steel ingots by the fluid or crucible process, and so founded in Sheffield an industry destined to become world-wide, and which soon extended the fame of Sheffield steel throughout the civilized world. The next slide shows a composition typical of crucible cast steel. It is less pure than shear steel, but sounder, being free from weld lines. It is said that the famous American General Sherman when asked to "spare the good Indians," replied that the only good Indians he had ever met were dead Indians. Be this as it may, it is certain that no steel can be good unless it is properly "killed," or in other words "dead melted." The slide on the screen (Fig. 2) shows sections of two crucible steel ingots of identical composition and weight, when poured in a "lively" and in a "killed" condition. Ignoring the "pipe" or central contraction cavity, the killed steel is quite solid, whilst the unkilled metal is riddled from end to end with gas cavities or "blowholes" containing under pressure hydrogen, carbonic oxide and nitrogen gases, evolved in the plastic steel during solidification, and thus rendering the ingot commercially worthless. The sound, and hence apparently much smaller, ingot has been "killed" by the presence of a trace (say 0·01 per cent) of metallic aluminium. The scientific explanation of this, the most remarkable phenomenon in the whole range of steel metallurgy, may be found in text-books or in reports of metallurgical lectures, but the present lecturer must confess that he is no nearer a convincing solution of this problem than when he began his researches twenty-five years ago.

It is next necessary to correlate the chemical and micrographic analyses of the plain carbon steel upon which the world depended for its cutting implements from the time of Homer to 1870.

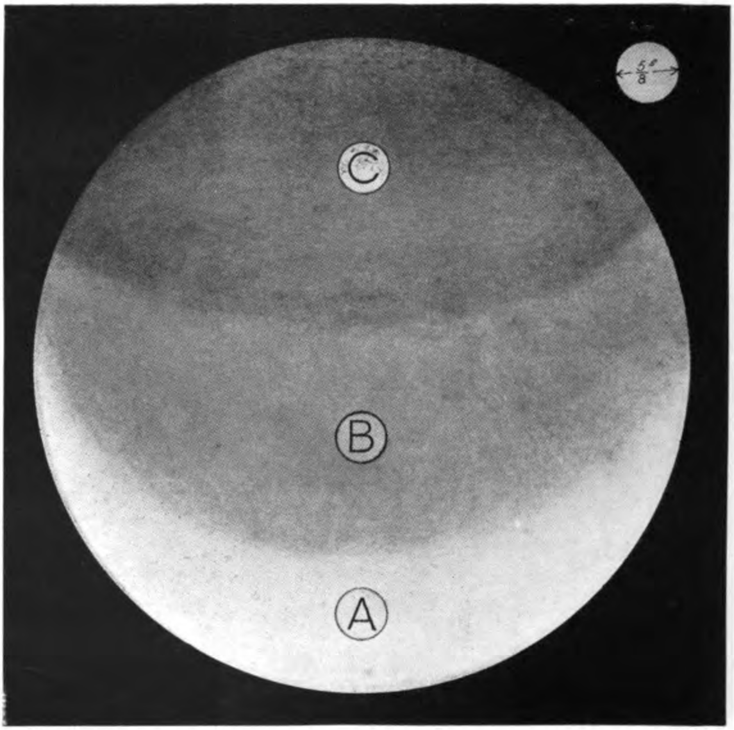
The slide on the screen shows the structure of pure Swedish iron, contaminated with a little slag. Ignoring this, the mass consists of white allotrimorphic crystals of iron with optically black boundaries. The next slide (Fig. 3) is a micrograph of nearly pure iron, containing about 0·4 per cent of carbon. Nearly half the mass consists of the dark-etching compound constituent pearlite. The next slide (Fig. 4) shows the structure of nearly pure iron, containing 0·89 per cent of carbon. The mass now consists entirely of pearlite, a mechanical mixture of 87 per cent of iron with 13 per cent of normal carbide of iron, Fe_3C. The mass abrasion hardness of normal pearlite is about 4·5, that is, between fluorspar and apatite on Moh's mineral scale.

We have next to consider the phenomena known as the hardening and tempering of steel.

The slide on the screen (Figs. 5 and 5A) shows very clearly the beginning (region C), the progression (region B), and end (region A) of the hardening of steel, that is to say, the transformation (during a thermal amplitude of perhaps 3° C.) of the compound constituent pearlite ($21\ Fe + Fe_3C$) to the micrographically amorphous constituent hardenite, which corresponds to the empirical figures $Fe_{24}C$, in which the carbide of iron, owing to the quenching, is trapped in some molecular association with the whole of the iron. The constituent hardenite has a hardness of seven on Moh's mineral scale—that is to say, it is as hard as quartz, flint, or rock crystal. It is a little difficult to realize how much the thermal capability of the mineral pearlite (with a hardness of 4·5) to transform itself into the igneous rock hardenite (with a hardness of 7) has contributed to the advance of civilization and to the material well-being of the human race. But unfortunately it was found that hardenite was thermally very unstable, and that its cutting powers were greatly limited by the fact that the heat of friction in turning operations caused the hardenite to revert largely to relatively soft pearlite at a blue heat, say 300° C. This property naturally limited the operations of engineers as to speed, as to traverse, and as to depth of cut, and consequently as to the cost and rate of output of all the engines and appliances necessary to our modern civilization.

The slide on the screen shows in the black areas the evolution of the latent heat of hardening, and consequently the transformation of the quartz-like hardenite to soft pearlite. This change at about 250° C. acquires a marked increase in velocity, which reaches a maximum at about 300° C. Here the soft pearlite becomes the predominant partner, and the cutting power of the mass has practically vanished.

About the year 1870 marked the first beginnings of an epoch in cutting-steel metallurgy, which may be called the tungsten-chrome era. Robert Forrester Mushet, at the Clyde Works, Sheffield, began to manufacture on a considerable scale his "self-hardening steel." Mushet had practically discovered that when carbon steel was alloyed with a large percentage of tungsten, it, when cooled from a yellow heat in a draught of air, was not only sufficiently hardened, but that, owing to the fortifying action of the tungsten on the carbon, the hardenite was thermally considerably more stable than that of plain carbon steel. It is probable that in Mushet's early steels the "breaking-down" point of the hardenite was raised to a temperature of

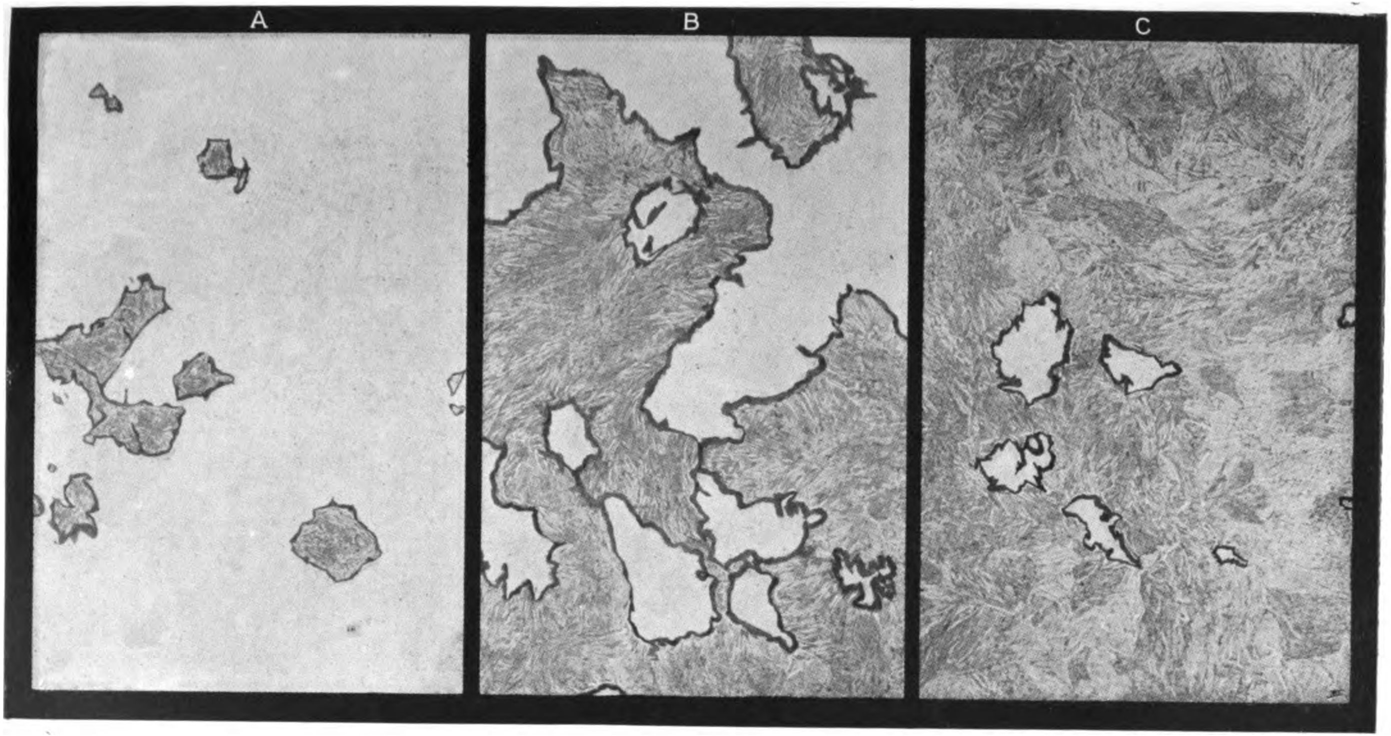


Carbon 0·89 per cent.

The Edge A was quartz-hard, stripping file teeth.
The Edge C was quite soft to the file.

FIG. 5.

Etched ⅝ in. diameter disc.
(Magnified about 12 diameters after differential heating and rapid quenching.)
(For high power magnification see Fig. 5A.)



Transformation nearly completed.
Temperature about 730° C.
Pale areas, Hardenite.

Transformation half completed.
Temperature about 729° C.
Laminated areas, normal Pearlitel.

Transformation beginning.
Temperature about 728° C.
Dark borders, Troostitic Pearlite.

Carbon 0·89 per cent.

FIG. 5A.

(Magnified about 450 diameters.

perhaps 400° C., thus enabling engineers to take bigger cuts and work at higher speeds. Later, about 1880, Mushet still further fortified his hardenite by the addition of relatively small percentages of chromium, and between 1880 and 1900 self- or air-hardening steels were produced by many steel manufacturers in considerable variety. The slide on the screen (Table I.) shows roughly the ranges of chemical composition of turning-tool steels from 1740 to 1912.

TABLE I.—Ranges of Chemical Compositions for Turning-Tool Steels from 1740 to 1912.

Type of Steel	Element							
	Carbon	Silicon	Manganese	Sulphur	Phosphorus	Tungsten	Chromium	Vanadium
Huntsman's plain carbon, 1740	1·2 to 1·4	0·10 to 0·15	0·10 to 0·30	0·02 to 0·03	0·01 to 0·02	absent	absent	absen
Mushet's tungsten, 1870	1·5 to 2·0	0·4 to 0·6	1·5 to 2·0	0·02 to 0·04	0·01 to 0·02	6·0 to 10·0	0·0 to 1·0	absent
Modern high speed	0·5 to 0·8	0·10 to 0·15	0·10 to 0·25	0·03 to 0·06	0·02 to 0·03	12 to 22	2·5 to 6·0	0·0 to 2·0

In connexion with cutting steels, a profound sensation was made throughout the steel world when, at the Paris Exhibition in 1900, the Bethlehem Steel Company, of America, showed turning tools made under the alleged patent of Messrs. Taylor and White, cutting very mild steel at a speed which rendered the nose of the tool red-hot. It was obvious that in these tools the thermal stability of the hardenite had been raised to perhaps 600° C. The chemical compositions in the patent embodied nothing which had not been included in the Mushet type of steel for a period of about 20 years prior to the date of the American patent. In fact what Taylor and White had really done was to show that this type of steel was capable of retaining its cutting edge at a much higher temperature than most engineers and metallurgists had realized. For this demonstration every credit is due to the Bethlehem Company. Sheffield steel-makers, realizing future possibilities, made from the year 1900 and onward a series of experimental researches, which eventually gave to engineers that astonishing material known as High Speed Steel, in which the thermal stability of the fortified hardenite was raised to about 700° C. The slide on the screen (Table I.) shows the striking difference in chemical composition between Mushet's and high speed steels; nevertheless, the latter are merely a progressive experimental development of the former. The claims of the Taylor-White patent were the subject of a protracted lawsuit, the costs of which were about £50,000. In the end, Mr. Justice Cross of the United States Circuit Court, in a lengthy and luminous judgment, pronounced the Taylor-White patent to be absolutely invalid. Nevertheless, it is still claimed that the patent in suit was utilized by British manufacturers in producing modern high speed steel. It is therefore only fair to consider what this patent really claimed.

The slide on the screen (Fig. 6) shows a physical curve of tungsten-chrome steels which the patentees claimed to have discovered. The co-ordinates are vertically the cutting efficiencies of tungsten-chrome steels with any carbon from 0·8 to 1·8 per cent (the amount being a matter of indifference), and horizontally the hardening temperatures in degrees Fahrenheit. The short horizontal line "A–B" between 1500° and 1550° F. was alleged to be the range in which, prior to the patent, all tungsten-chrome air-hardening steel had been hardened. The falling line "B–C" between 1550° and 1725° F. was stated to be the breaking-down range discovered by the patentees, along which the cutting power of the steel steadily deteriorated. Then along the rising line "C–D" from 1725° to 2000° F. (the maximum temperature specified in the patent) the quality of the steel improved as the temperature of hardening rose, until in the higher part of this range the turning tools had an efficiency never before

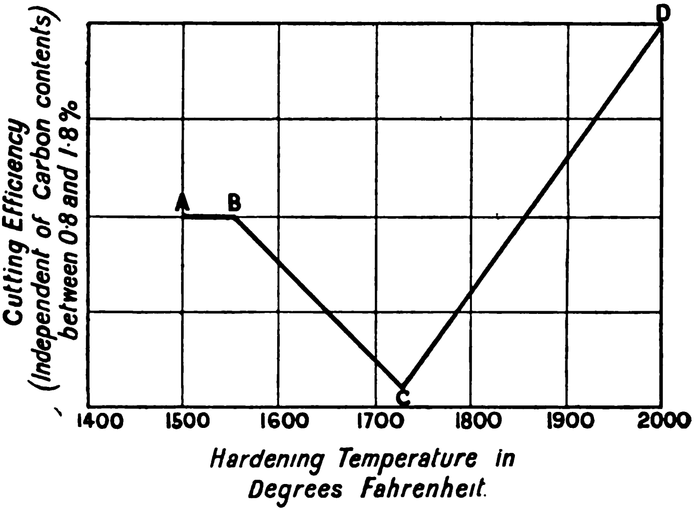


Fig. 6.—Physical Diagram claimed by Messrs. Taylor and White for Tungsten-Chrome Steels.

achieved in the art, and in effect (to use the words of Coleridge's "Ancient Mariner") the patentees claimed

"We were the first that ever burst
Into that silent sea."

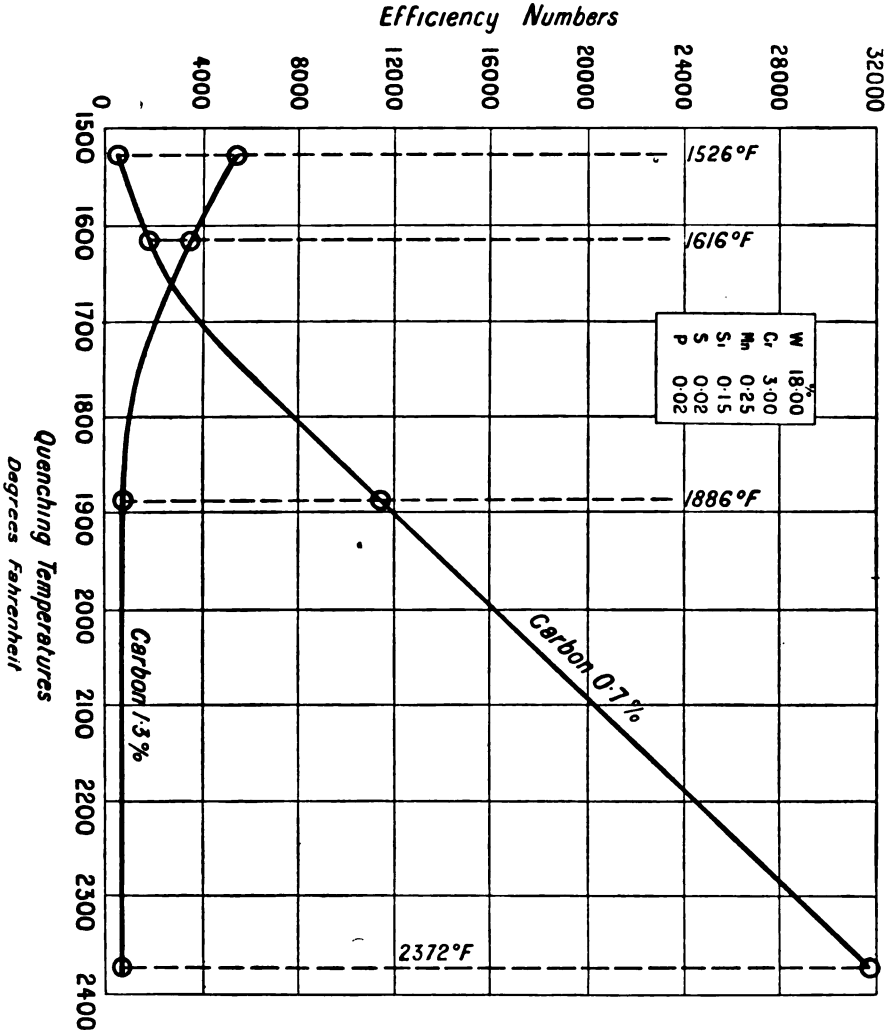


Fig. 7.—Physical Curves obtained by Arnold and McWilliam for Tungsten-Chrome Steels.

My late colleague, Dr. A. McWilliam, and I were commissioned to investigate at Sheffield University the accuracy or otherwise of the curve specified in the patent. The results are embodied in the curves on the screen (Fig. 7). The co-ordinates are, horizontally hardening temperatures in degrees Fahrenheit, and vertically cutting efficiency numbers obtained by the approximate and relative formula $e = t \times s^2$, where "e" is an efficiency number, "t" the time endurance in minutes, and "s" the cutting speed *cæteris paribus* in feet per minute. It will be seen that with a steel containing about 18 per cent of tungsten, 3 per cent of chromium and 1·3 per cent of carbon, the maximum efficiency number of about 6000 is obtained at the lowest temperature 830° C. or 1526° F., after which the higher the hardening temperature the less the efficiency number, which at 1300° C. or about 2400° F. has fallen to 500, or only twice the efficiency of plain carbon steel. In a similar steel containing, however, only 0·7 per cent of carbon, the efficiency number at 830° C. or 1526° F. is only about 500, but the efficiency steadily rises with the hardening temperature till at 1300° C. or 2372° F. it reaches the astounding number of about 32,000. In a word, there is no breaking-down range, and so far from the percentage of carbon being immaterial, the cutting efficiency is actually a function of the carbon and hardening temperatures.

It has already been pointed out that the year 1870 marked the commencement of the tungsten era, and 1880 that of the tungsten-chrome era. But the years 1899 to 1902 inaugurated what is destined to be the most remarkable epoch of the three, namely, the vanadium era. During these years a series of researches on the influence of the comparatively rare metal vanadium on plain carbon steel and on alloy steels were carried out in the experimental steel works of Sheffield University. At that time (1899) vanadium was 60*s*. per lb. In 1912, owing to the large demand, the cost had fallen to 10*s*. per lb.

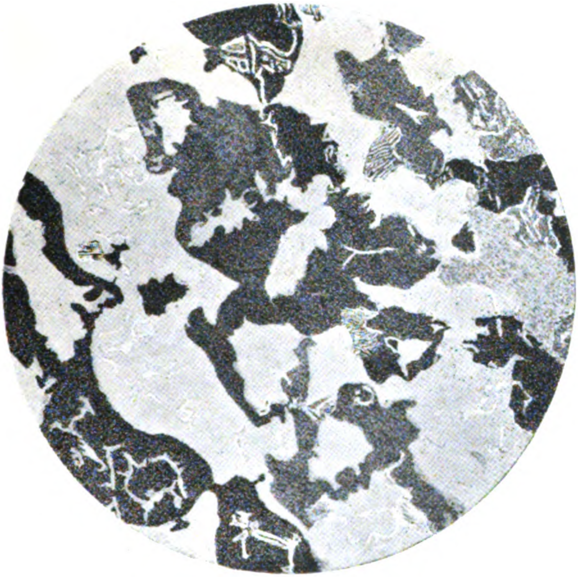
The first report, having reference mainly to cutting steels, was issued in June 1900, and the second and third reports respectively in January and June 1902. The results were briefly summarized in the two paragraphs shown on the screen.

June 28*th*, 1900.

> "The results of this preliminary investigation have profoundly impressed upon my mind the future before vanadium as a steel-making element, and even at this early stage of my knowledge of its effect, I venture to say that its action resembles that of tungsten, but that it is from ten to twenty times as powerful as the latter element."

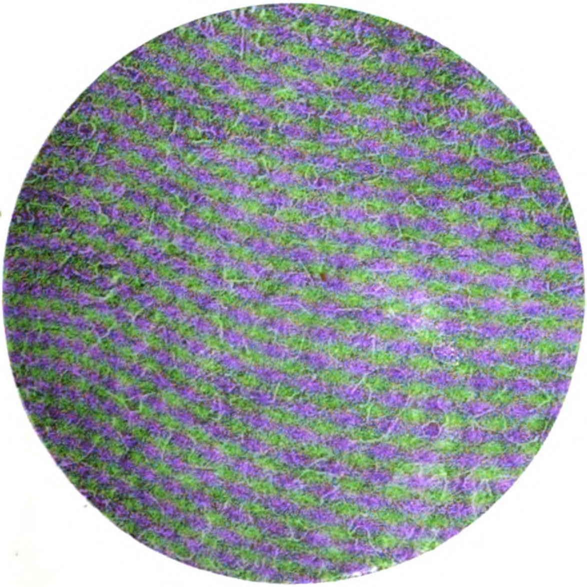
January 14*th*, 1902.

> "It is already evident that as a steel-making element vanadium will place in the hands of metallurgists and engineers a very powerful weapon, because it is now



Carbon, 0·60 per cent. Vanadium, 0·71 per cent.

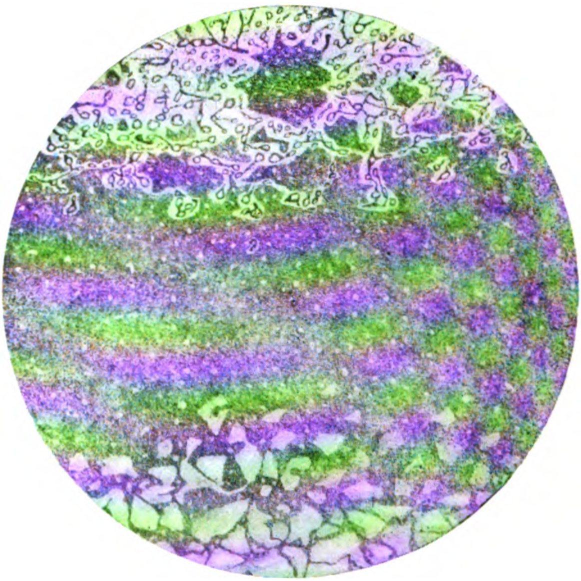
Pale ground mass of slightly Vanadiferous Ferrite. Dark areas, Troostitic Vanadium Pearlite. Less dark areas, Sorbitic Vanadium Pearlite. White cell walls and masses, "B" Iron Cementite, resulting from thermal decomposition of laminated Iron Pearlite, a few areas of which still remain undecomposed.



Carbon, 0·93 per cent. Vanadium, 5·84 per cent.

Ground mass of Sorbitic Vanadium Pearlite, overlaid with a broken and irregular mesh-work of Vanadiferous Ferrite.

FIGS. 8 and 8A. (Magnified 450 diameters.)



Carbon, 1·10 per cent. Vanadium, 13·45 per cent.

Transformation stages of Vanadium Cementite and Vanadiferous Ferrite into Vanadium Hardenite. Upper area, mainly Vanadiferous Ferrite with Vanadium Cementite nodules, together with a little Sorbitic Vanadium Pearlite. Middle area, ground mass of unsaturated Vanadium Pearlite, overlaid with undissolved nodules of Vanadium Cementite. Lower area, mainly structureless Vanadium Hardenite cells formed from a series of centres and surrounded by walls of the structure described for middle area. Hardening temperature, near 1400° C.

FIG. 9.

(Magnified 450 diameters.)

> demonstrated beyond doubt that the addition of a few tenths per cent of vanadium raises the elastic limit of mild structural steel at least 50 per cent, without seriously impairing its ductility or presenting any difficulty in the hot or cold working of the steel."

Some of the results upon which these paragraphs were founded are tabulated on the screen. Perhaps the most remarkable results in this series are :—

First.

A plain carbon steel, containing about 1 per cent of carbon, has a yield point of 35 tons per square inch, a maximum stress of 60 tons per square inch, an elongation of 10 per cent on 2 inches, and a reduction of area of 10 per cent. The addition to such steel of about 0·6 per cent of vanadium raised the yield point from 35 to 65 tons, the maximum stress from 60 to 86 tons per square inch, still leaving an elongation of 7 and a reduction of area of 8 per cent.

Second.

A steel containing 0·25 per cent of carbon and 3·3 per cent of nickel registered a yield point of 33 tons, a maximum stress of 42 tons per square inch, an elongation of 26 per cent on 2 inches, and a reduction of area of 53 per cent.

A practically identical steel, but containing in addition about 0·25 per cent of vanadium, recorded a yield point of 50 against 33 tons, a maximum stress of 68 against 42 tons per square inch. The elongation was 17 per cent on 2 inches, and the reduction of area 36 per cent.

Third.

A steel containing 0·25 per cent of carbon and about 1 per cent of chromium registered a yield point of 27 tons, and a maximum stress of 41 tons per square inch, together with an elongation of 36 per cent on 2 inches and a reduction of area of 55 per cent.

The addition of 0·25 per cent of vanadium raised the yield point from 27 to 40, and the maximum stress from 41 to 55 tons per square inch. The elongation was lowered from 36 to 26, and the reduction of area from 55 to 53 per cent.

Thus vanadium differs from tungsten in having an almost magically beneficial effect, not only on cutting, but also on structural steels. In connexion with vanadium steels it is an interesting fact that the series of copyrighted and published reports issued from Sheffield University during the years 1900 to 1902 were unconscious plagiarisms of a series of American patents issued during the years 1904 to 1908. This seems to constitute a remarkable problem in psychology.

A study of what may be called the pure science of vanadium steels, made by the lecturer and Professor A. A. Read, of the University of Wales, has yielded results of profound theoretical and probably practical importance. The slide on the screen (Table II.) shows that vanadium does not seem to form a double carbide with iron. It gradually wrests the carbon from the carbide of iron till when about 5 per cent of vanadium is present Fe_3C cannot exist, and practically speaking only a vanadium carbide V_4C_3 containing 15 per cent of carbon is present, and this constituent is constant at any rate in tool steels containing up to 14 per cent of vanadium. The micrographic analysis of these alloys, as shown in the three micrographs about to be thrown upon the screen, has resulted in the discovery of three new constituents, viz. vanadium pearlite (Figs. 8 and 8A), vanadium hardenite, and vanadium cementite (see top area of Fig. 9). Vanadium hardenite seems to have a hardness of 8 (topaz) as compared with the hardness 7 (quartz) of iron hardenite.

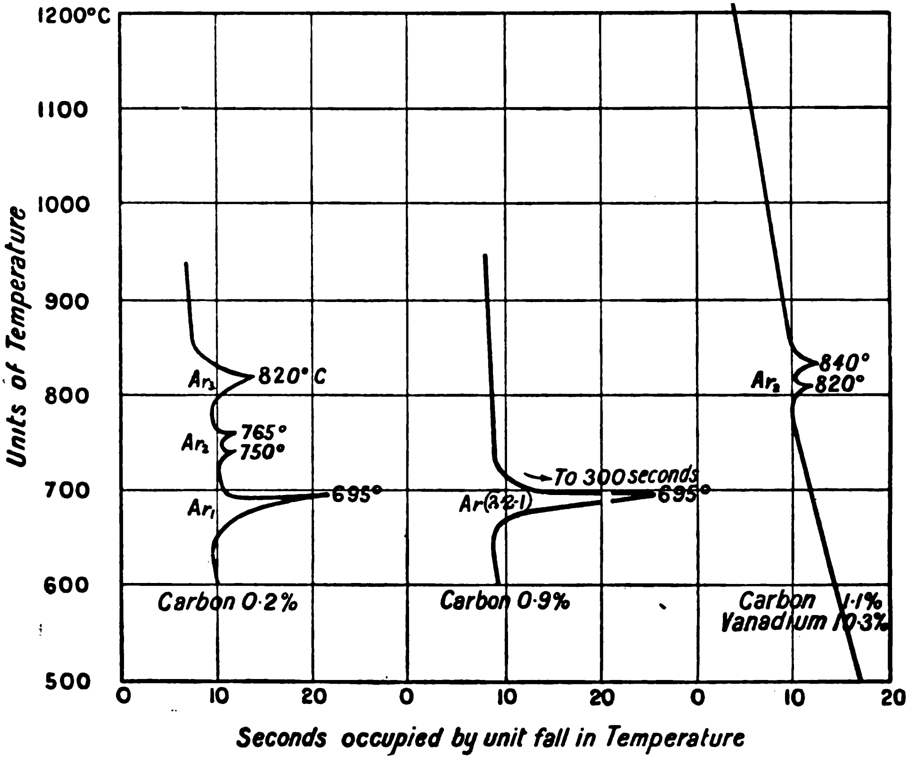


Fig. 10.

TABLE II.—ELECTROLYTIC ISOLATION OF THE CARBIDES OF IRON AND VANADIUM.

(Journal of the Iron and Steel Institute, No. 1, 1912.)

Arnold and Read.

No. of Steel	Carbon per Cent.	Vanadium per Cent.	Amperes	Volts at Terminals	Time in Acid	Grammes Dissolved	Weight of Dry Residue	Percentage of Total Carbon obtained with Carbide Residue	Analysis of Carbide			Corresponding to the Formula	Theory		
									Carbon per Cent.	Iron per Cent.	Vanadium per Cent.		Carbon per Cent.	Iron per Cent.	Vanadium per Cent.
1315	0·60	0·71	0·5	0·6 to 1·5	Hours 12	8·920	0·6753	95·77	7·61	83·71	8·68	$11Fe_3C+V_4C_3$	7·57	83·24	9·19
”	”	”	”	”	”	8·926	0·6847	96·98	7·66	83·69	8·65				
”	”	”	”	”	11	8·260	0·6154	92·71	7·49	83·27	9·24				
1316	0·63	2·32	”	0·7 to 1·2	”	8·054	0·4346	88·10	10·66	54·60	34·74	$2Fe_3C + V_4C_3$	10·00	56·00	34·00
”	”	”	”	”	”	8·053	0·4148	82·80	10·44	54·71	34·85				
”	”	”	0·55	”	10	7·630	0·4180	91·20	10·63	53·30	36·07				
1309	0·93	5·84	0·5	0·8 to 1·3	12	8·625	0·5066	99·15	15·94	0·54	83·52	V_4C_3	15·00	..	85·00
”	”	”	”	”	”	8·612	0·4973	98·19	15·94	0·47	83·59				
”	”	”	”	”	”	8·313	0·4890	97·63	16·05	0·83	83·12				
1310	1·07	10·30	”	0·8 to 1·4	”	8·168	0·5952	96·76	15·03	1·04	83·93				
”	”	”	”	”	”	8·160	0·5732	95·22	15·00	0·63	84·37				
1312	1·10	13·45	”	0·8 to 1·6	”	8·303	0·6757	99·78	14·12	0·82	85·06				
”	”	”	”	”	”	8·571	0·7267	99·06	13·76	1·27	84·97				

The recalescence results obtained are of great practical as well as theoretical interest. They strongly suggest the explanation of the curious thermo-mechanical behaviour of high-speed steels, and incidentally they appear to provisionally prove that the hardening is not due to allotropic change, but to the carbon change only. The slide on the screen (Fig. 10) shows (1) the inverse rate recalescence curve of a 0·2 per cent plain carbon steel which exhibits all Osmond's critical points, viz. Ar_3, Ar_2 (with a double peak), and Ar_1, the carbon change point; (2) the recalescence of a saturated steel containing 0·89 per cent of carbon, in which all three points are merged into one very large evolution of heat at 695° C.; (3) the recalescence curve of a steel containing 1·1 per cent of carbon and 10·3 per cent of vanadium. This curve was registered from 1210° to 500° C. It presents only the double-peaked point Ar_2. When the steel is quenched all along the above range it still remains quite soft to the file. To harden it, it is necessary before quenching in water to heat the alloy above the A_1 or carbon change point, which takes place at a white heat, near 1400° C. The steel is then very hard.

The micrograph on the screen (Fig. 9) shows the transformation on heating up to a white heat (*a*) of annealed vanadium cementite into sorbitic vanadium pearlite, (*b*) of sorbitic vanadium pearlite into amorphous and topaz-hard vanadium hardenite.

The final slide (Table III.) exhibits the advance in concrete-cutting efficiency of turning tools from 1740 to 1912. It will be noted that the best steel of this kind made in Sheffield in 1740 would be absolutely incapable of cutting at all under conditions under which the best modern high speed steel would remove 700 cubic inches of metal before breaking down.

TABLE III.—COMPARATIVE CUTTING POWERS OF TURNING-TOOL STEELS FROM 1740 TO 1912.

Type of Steel	Cubic Inches removed *Cæteris paribus*	Type of Steel	Cubic Inches removed *Cæteris paribus*
Huntsman's . .	16	Huntsman's . .	0
Low-grade high speed	216	High-grade high speed	700

The advantages of this enormous increase in cutting power are manifold, and an obvious example is the relative rapidity with which huge naval guns may now be turned out.

In January 1909 I had the honour of suggesting to a Royal Institution audience the coming of a new British steel, which would have a cutting power four times as great as the best steel then on the market. The skilful application of vanadium by Sheffield steel-makers has practically fulfilled that forecast, and the world-wide sensation and publicity created by the announcement has left Great Britain supreme in this very important branch of scientific steel metallurgy.

An aspect of iron and steel metallurgy already demanding attention is the diminishing quantity of the world's iron ore supply. To a great extent the latter could be strongly reinforced from the huge deposits of iron sands now lying useless, if a simple, economical, and direct process of reduction could be devised. That metallurgical science and art will do this eventually seems certain, and I hold an opinion, founded on practical data, that the solution of this hitherto baffling problem is nearer than most metallurgists suppose.

In conclusion, it may be pointed out that the skeleton history of early Sheffield steel metallurgy sketched in this discourse is in some important points in conflict with the somewhat disparaging historical outline written by Lord Macaulay; but in this particular connexion there seems to be a modicum of truth in the answer of the school-boy who, when asked to mention his favourite work of fiction, unhesitatingly replied, " Macaulay's History of England."

[J. O. A.]

WEEKLY EVENING MEETING,

Friday, January 31, 1913.

SIR WILLIAM CROOKES, O.M. LL.D. D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

GEORGE M. TREVELYAN, Esq.

The Poetry and Philosophy of George Meredith.

[ABSTRACT.]

THE quality that differentiates Meredith's novels from other novels is the element of poetry. In realism and psychology he is surpassed by living Englishmen; but he is so much more than an analyst of motives. He makes us in love with life. Even in his comedy, when it is successful [*Richmond Roy*], the poetical predominates. [Examples of poetical element in novels.] In his prose and verse his poetry uses metaphors largely; but they are compared ideas of things, rather than compared visual images of things as in Keats. [Examples.]

Meredith's philosophy—attitude to life—also inspires his novels. And he alone of the great Victorian poets accepted the scientific [Darwinian] cosmology as the very basis of his ethic, and even the inspiration of his poetry. [Examples: *Spirit of Earth in Autumn*, etc.] He subordinates the individual, and his desires to the race as a whole viewed in evolution. His attitude to Life and Death. [Examples: *Day of the Daughter of Hades.*]

Meredith of Welsh-Irish origin, on which he always laid great stress. His genius [flood of metaphors, etc.] is essentially Celtic in literary character. But, unlike Mr. Yeats and poets of the Celtic revival, he used it for English purposes, on English themes, about English life, and with the characteristic view of other great English writers, that "conduct is three-fourths of life." The solitudes of nature he loves passionately, but they always have a curious effect on him; they always set him thinking of London and the life of men, and create desire in him to return to the thick of the fight. [Examples.] Close connexion to his mind between the life of men and that of nature. Earth "our mother." In the novels his men and women get their superhuman grandeur when seen in contact with nature. [Examples.] His drawing-room scenes less good. *Love in the Valley*: Meredith said about it to the lecturer, "It has something of *down there* in it." "Down there" = of Earth. *Juggling Jerry*, another example of connexion of soul of man with nature, the inspiration of nearly all that is good in his poems and novels.

Meredith's conception of God, the ethical element in Nature towards which man is struggling, but through nature, not against her. Moral evolution in the individual, his chief interest in his novels—the "ordeal" of his young men and women. [Examples.] And so in his poems, e.g. *The Test of Manhood.*

His last poems show that as death became a near question for himself he ceased to be much interested in it. He thought only of the human race [in particular of the English], and of their problems after he should be gone. The only poem that he wrote about himself as he was waiting for death is as follows :—

Once I was part of the music I heard
    On the boughs or sweet between earth and sky.
    For joy of the beating of wings on high
My heart shot into the breast of the bird.

I hear it now, and I see it fly,
    And a life in wrinkles again is stirred.
    My heart shoots into the breast of the bird,
As it will for sheer love till the last long sigh.

[G. M. T.]

GENERAL MONTHLY MEETING.

Monday, February 3, 1913.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

Edward G. Acheson, Esq. D.Sc.
Mrs. Ethel Norman Thompson,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. G. H. Griffin for his Donation of £21 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Honorary Secretary announced the decease on January 5, 1913, of Louis Paul Cailletet, and on December 7, 1912, of Sir George Darwin; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted:—

Resolved, That the Managers of the Royal Institution desire to record (at this their first Meeting since his decease) their sense of the loss sustained by the Institution and the Scientific World in the decease of Louis Paul Cailletet, Membre de l'Institut, Officier de la Legion d'Honneur, Officier de l'Instruction Publique, and an Honorary Member of the Royal Institution.

He was celebrated for his important investigations on the laws and properties of gases when highly compressed, and he succeeded in forming a liquid mist of the hitherto permanent gases.

The early work of Professor Cailletet was devoted to Metallurgy, and the scientific investigation of the principles of steel manufacture, and the phenomena of cementation and puddling. His work on the theory of smelting led to the analyses of the gases occluded in iron.

The Managers of the Royal Institution desire to convey to the family the expression of their most sincere sympathy in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record (at this their first Meeting since his decease) their sense of the loss sustained by the Institution and the Scientific World in the decease of Sir George Howard Darwin, K.C.B. LL.D. D.Sc. F.R.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.

Sir George Darwin has been on the list of Members for forty-three years, and has been a Manager of the Institution.

He delivered two Friday Evening Discourses—one on "Meteorites and the History of the Stellar Systems," in 1889, and another on "Sir William Herschel," in 1912, and also the Tyndall Lectures in 1903, on "The Astronomical Influence of the Tides." Sixty six of his papers up to date have been published in four volumes by the Cambridge University Press, forming a valuable record of his scientific labours. In 1897 he delivered a course of lectures at the Lowell Institute, Boston, which were published in the following year in a popular work entitled, "The Tides and Kindred Phenomena in the Solar System," and a third edition was published in 1910.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Lady Darwin and the family in their bereavement.

The Chairman read the following Letters which had been received from the Honorary Members elected at the General Meeting on December 2, 1912 :—

[*Translation.*]

INSTITUT DE FRANCE

ACADEMIE DES SCIENCES, PARIS,
December 16, 1912.

SIR,

I have received your letter in which you kindly inform me that the Members of the Royal Institution have been good enough to elect me an Honorary Member of the Royal Institution.

I do not wish to delay in asking you to address to them my very sincere and very respectful thanks. I am particularly sensible of this new mark of sympathy and esteem coming to me from your country.

Please accept, Sir, the assurance of my sentiments and highest esteem.

(Signed) G. DARBOUX,
Perpetual Secretary of the Academy of Sciences.

P.S.—As soon as I receive the diploma which you inform me you are sending, I will not omit to apprize you of its arrival. I shall be particularly obliged if you will transmit my thanks to His Grace the Duke of Northumberland.

MERAN,
January 6, 1913.

DEAR SIR,

Please accept my best thanks for the news you were kind enough to give me, that I have been elected an Honorary Member of the Royal Institution.

The Diploma itself now likewise having reached me, I beg you kindly to present to your famous Institution the expression of my respectful thanks for the great honour shown me by this election.

Yours most respectfully,
G. MITTAG-LEFFLER.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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E. Flatau, La Migraine. 8vo. 1912.

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LOWELL, PROFESSOR PERCIVAL.
Transparency of the Spectra of Uranus, showing its Rotation.

WEEKLY EVENING MEETING,

Friday, February 7, 1913.

Sir Francis Laking, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

Sir John Murray, K.C.B. LL.D. D.Sc. F.R.S.

Life in the Great Oceans.

The term *Biosphere* is now used by naturalists to designate that mantle of living matter which clothes the globe wherever the atmosphere, the hydrosphere, and the lithosphere are in contact. Life does not extend very high above, nor very deep below, the surface of the land, but in the ocean it is quite different. Life is present everywhere throughout the mass of ocean waters, from the equator to the poles and from the surface down to the bottom at a depth, it may be, of six English miles. The conditions which determine the life-histories and distribution of different marine organisms comprise: temperature, light, currents, salinity, viscosity, depth, pressure, and chemical composition of the water. [The methods of capturing the organisms in different levels of the ocean were then explained.]

Throughout the photic zone, extending down to nearly 3000 feet (500 fathoms), vegetable life is present, both attached to the shore in shallow water, and floating in the upper layers of the open ocean in the form of vast meadows of unicellular algæ, the dead remains of which fall through the underlying waters to the bottom of the deepest seas, supplying food for the animals inhabiting the intermediate and bottom waters. Considering the vast extent of the ocean waters, it may be said that the total quantity of living matter in the ocean greatly exceeds that covering the land-surfaces of the globe.

The broad facts of an oceanographical biological survey may be thus summed up: In the photic zone there are (1) a littoral and shallow-water fauna and flora; (2) a surface oceanic fauna and flora; (3) a mud-line fauna, in depths just beyond 100 fathoms; (4) an intermediate, or twilight, fauna in the open ocean. In the region below the photic zone there are (5) an archibenthal fauna of the continental slope down to 1700 fathoms; (6) a deep-water intermediate fauna; and (7) an abyssal benthonic fauna, beyond 1700 fathoms.

Dealing first with plant-life in the ocean, it may be stated that the phanerogams are represented only by the eel-grass (*Zostera marina*), which is very common in certain localities, while the vast majority of sea-plants belong to the algæ. Unlike terrestrial plants, the marine algæ find their nourishment dissolved in the sea-water and uniformly distributed around them, so that they can take in food throughout the entire surface of the organism: they all possess green chlorophyll—that magician which, conjuring with the sunbeams, is able to build up organic compounds from inorganic constituents.

The marine algæ have been arranged into four groups according to their predominant colouring: (1) Chlorophyceæ, or green algæ; (2) Cyanophyceæ, or blue-green algæ; (3) Phæophyceæ, or brown algæ; and (4) Rhodophyceæ, or red algæ.

The attached brown algæ are the best known of marine plants, and include the *Fucus* and *Laminaria* of the shore and shallow water, the gigantic *Macrocystis*, sometimes with stems 700 or 800 feet in length, and the famous Gulf weed (*Sargassum*), which, though found in great floating banks in the Sargasso Sea, is derived from the coasts of the West Indian region. The attached red algæ include delicately branching forms, like the *Polysiphonia* of the English coasts, and massive forms encrusted with calcareous matter (nullipores or corallines), which play an important part in the building-up of some coral reefs and in protecting rocks from erosion.

The floating algæ (or Phytoplankton) are all of small size, mostly indeed of microscopic dimensions, and till recent years were very little studied. They occur throughout the photic zone in great profusion, especially in the sub-surface layers, building up the organic substances upon which all marine animals depend for food. They include a few representatives of green and blue-green algæ, but consist mostly of diatoms, peridineans, brown and calcareous flagellates.

Diatoms are found in fresh water and in damp places as well as in the sea, where they occur both free in the water and attached to other algæ and animals: the cell-walls are siliceous and often beautifully sculptured. They have been arranged in four groups, according to their type of structure: (1) the bladder type, like *Coscinodiscus*; (2) the ribbon type, like *Fragillaria*; (3) the hair type, like *Rhizosolenia*; and (4) the branching type, like *Chætoceras*. They vary in form, and develop special suspension-organs, in response to variations in the viscosity of the water, and in coastal regions they form resting-spores which sink to the bottom or into deep water and lie dormant during unfavourable seasons of the year. The oceanic diatoms occur most abundantly where the water is relatively fresh and loaded with suspended matter, this distribution being correlated apparently with the amount of colloid silica or clay in the water. The dead frustules accumulate on the ocean-floor in the Southern Ocean and in other regions in such numbers that the deposit is called diatom ooze.

Peridineans are found in abundance and in a variety of forms, many of them brilliantly phosphorescent; the cell-walls are composed of cellulose or allied substance readily soluble in sea-water, consequently the remains of these organisms are never detected in marine deposits. Among the flagellates of the open ocean *Pyrocystis* is well known from the intensity of its phosphorescent light.

The calcareous flagellates (Coccolithophoridæ) have the cell-wall strengthened by calcareous shields, sometimes with a central spine. These shields (coccoliths and rhabdoliths) were known from the geological strata and from recent marine deposits long before the living organisms (coccospheres and rhabdospheres) were discovered in the surface waters of the ocean.

The brown algæ include the Xanthellæ or "yellow cells" found living in symbiosis with radiolaria and other invertebrates, evidently to the mutual advantage of the plant and the animal. Among the Phæodaria, a group of radiolarians inhabiting the deep sea, the place of the yellow cells in the other radiolaria is apparently taken by the phæodellæ, dark-coloured cells, which may possibly be a lower form of algal life than the yellow cells, and capable of evolving oxygen under the influence of the phosphorescence of deep-sea animals.

Of the blue-green algæ, the best known is *Trichodesmium*, which sometimes occurs in such abundance at the surface as to form a scum of great extent, called by sailors "whales' spawn." The green colour so conspicuous in the vegetation on land is found in the sea apparently only in one form—the green alga *Halosphæra*.

Bacteria are the most numerous and most widely distributed of living plants, being found everywhere in earth, air and water, and as parasites in plants and animals. With a few possible exceptions they cannot live without carbonaceous and nitrogenous organic matters. They are present everywhere in the ocean. It may be said that it is the fate of all living substances to become sooner or later, directly or indirectly, food for bacteria. Life, as a whole, could not continue without bacteria. They do not originate life, but supply life with the necessary material. One group is said to be practically independent of organic compounds, being able, without chlorophyll and sunlight, to appropriate the carbon dioxide from the atmosphere, and to store up free nitrogen, thus giving some indication of how the first forms of life to appear on the earth obtained their nourishment. Phosphorescent bacteria are limited to ocean water, for they have never been observed in fresh water; indeed, no phosphorescent organisms of any kind are known from fresh water, though the power of emitting phosphorescent light is widely distributed in nearly all groups of marine organisms.

Turning now to the animal life in the ocean, it may be stated that most marine animals are cold-blooded, the temperature of their circulating fluid depending upon that of the sea-water in which they live, the marine mammals (whales and seals) being the only representatives of the warm-blooded air-breathing animals, whose blood-temperature remains more or less constant whatever the external temperature may be.

Temperature conditions are thus of paramount importance in the life-history of the cold-blooded marine fauna, animal metabolism being retarded in cold water and accelerated in warm water. This explains the relatively large number of individuals found living in cold polar waters as compared with the warm tropical waters, for in the colder waters eggs and young and adults (some of them ten or twelve years old) are found at all seasons, while in the warmer waters the various stages are passed through rapidly, so that the age of the oldest individuals may be reckoned in days, weeks or months. While pelagic larval stages are characteristic of most animals living in warm tropical waters, polar marine animals have usually a direct development. The secretion of calcium carbonate by animals to form shell and skeleton is much greater in warm than in cold water: thus the numerous pteropods and foraminifera of the tropics have only one or two small representatives in polar regions, while coral reefs and calcareous bottom-deposits are limited to the warmer regions of the ocean. The viscosity of sea-water is greater at low than at high temperatures, and this has an effect upon the floating powers of organisms and upon the development of suspension-organs.

The pelagic animals (Zooplankton) include nearly all the marine groups, the principal ones being: foraminifera, radiolaria, crustacea, pteropods, cephalopods, tunicates, and fishes; there are no sponges, only one pelagic echinoderm (though many pelagic larvæ), and only one group of insects (*Halobates*). The colouring and amount of pigmentation in plankton animals, as well as the development of eyes and light-organs, appear to be correlated with the intensity of sunlight and the depth of water; thus at the surface the animals are usually colourless, silvery, or bluish, except in the case of those living in the vicinity of floating objects, which exhibit a remarkable protective colouring; in depths of 100 to 200 fathoms the fishes are grey, or have silvery sides and brown backs, while in depths beyond 300 fathoms red or dark colours predominate. Many plankton animals emit phosphorescent light by the aid of a luminous secretion or special light-organs more or less complicated in structure; both light-organs and eyes appear to be larger in depths down to 300 fathoms than in deeper water.

There is a bathypelagic (or twilight) fauna living near the lower limit of the photic zone, where a great change in the viscosity, temperature, and other physical conditions takes place, probably retarding the fall of dead organisms and thus forming a sort of artificial bottom, or seaward extension of the continental mud-line. The animals inhabiting the intermediate zone of water between the lower limit of the photic zone and the bottom seem to be less numerous than in the layers nearer the surface and nearer the bottom, and are mostly deep black or bright red in colour.

The bottom-living animals (Benthos) are most abundant in shallow water, and decrease in numbers towards the deep water far from land, except perhaps in the deep water of the great Southern Ocean. Characteristic assemblages of benthonic animals are found in different regions and at different depths, and various schemes have been devised to express their distribution. In European waters we may generally recognize: (1) the littoral zone, extending from high-water mark to a depth of about 20 fathoms, which is sub-divided into areas and belts according to latitude and to the materials covering the bottom; (2) the sublittoral or shallow-water zone extending from about 20 fathoms to about 100 fathoms; (3) the archibenthal zone, occupying the continental slopes beyond 100 fathoms down to 1700 fathoms (the mean sphere level)*; and (4) the deep-sea or abyssal zone in depths beyond 1700 fathoms.

Many characteristic deep-sea forms have long stalks or long legs, lifting the bodies of the animals out of the ooze; others have well-developed tactile organs; others are delicate in structure when compared with their shallow-water allies, especially those that secrete calcium carbonate. Phosphorescent light plays an important rôle in deep-sea life, and is emitted principally by cœlenterates, which have frequently been brought up from great depths in a light-giving condition. The subdued colouring of deep-sea animals, mostly red and brown monotones, is doubtless correlated with the faint gleams of phosphorescent light, but it is difficult to explain the variation in the size of the eyes observable among some deep-sea forms, the same haul of the trawl sometimes bringing up creatures with large eyes along with others having small eyes or totally blind. Since the low temperature prevailing in the abyssal zone retards the action of the digestive enzymes, the food requirements of the deep-sea animals must be less than those of animals living in the warmer waters of the ocean.

The colonizing of the deep sea seems to have been effected by successive migrations from the shallower reaches of the ocean, especially from the vicinity of the mud-line, but apparently more frequently from cold than from warm regions. We may suppose that, if the waters of the ocean had at one time a nearly uniform temperature throughout, the deep sea would be unsuitable for animal life from the lack of circulation and hence of atmospheric oxygen, though benthonic animals might occupy the whole of the shallow-water zone. When cooling commenced in the polar regions the forms with pelagic larval stages would be killed out or forced to retire towards the tropics, some of them continuing to occupy the temperate regions by restricting the period of reproduction to the summer season. Thus, in the tropical and coral-reef regions we find to-day the remnants of a former universal shallow-water fauna, which has been replaced in polar regions by animals from the mud-line having mostly a direct development. This would explain the similarity between the two polar faunas, the more numerous individuals but relatively fewer species in the polar areas as compared with the tropical area, and the resemblance between polar shallow-water animals and deep-sea forms. On the other hand, if we assume an ocean with a universal low temperature and universal cold-water fauna and flora, then with heating at the equator the present tropical faunas and floras would be evolved.

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* Recent researches show that the fish-fauna in this zone of the north-east Atlantic is very uniform from the Faroes to the Canaries, several species being common to these two localities.

The "Challenger" dredgings and trawlings at all depths indicate a gradual decrease in the number of species with increasing depth, the abundance of individuals in the shallower waters contrasting strongly with the few individuals per species in depths beyond 1000 fathoms. The ratio of species to genera falls regularly on proceeding into deeper and deeper water, which might be explained by supposing the deep sea to have been peopled by continuous migrations from the mud-line at many periods and from many regions. Trawlings in the deep water of the Southern Ocean yielded a greater variety of animals than similar hauls in shallow water: one haul in 1600 fathoms gave 200 individuals belonging to 89 species (excluding Protozoa), while eight hauls between 1260 and 2600 fathoms yielded 272 species—an average of 34 species per haul.

The majority of deep-sea species have apparently a restricted distribution, and the idea that a universal and peculiar fauna of great antiquity overspread the deep ocean-floor has not been supported by deep-sea investigations. Some deep-water species are much larger than their shallow-water allies, and others present archaic characters, but the relict fauna from remote geological periods which some naturalists hoped would be found to survive in the deep sea has yet to be discovered.

[J. M.]

WEEKLY EVENING MEETING,

Friday, February 14, 1913.

SIR JAMES CRICHTON BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

PROFESSOR ANDREW GRAY, M.A. LL.D. F.R.S.

Gyrostats and Gyrostatic Action.

WE are accustomed in daily life to handle non-rotating bodies, and their dynamical properties excite little attention, though it cannot be said that they are commonly understood. It is different, however, with rotating bodies. These, when handled, seem to be endowed with paradoxical, almost magical properties. I have here an egg-shaped piece of wood. I place it on the table and it rests, as we expect it to do, with its long axis horizontal. Our experience tells us that this is the natural and correct position of the body. But I set it spinning rapidly on the table, as you see, with the long axis horizontal, and you observe that after an apparently wobbling motion it erects itself so that its long axis is vertical. It was started spinning about a shortest axis, but the body has of itself changed the spin, and it is now turning about the long axis. In taking this position it has actually raised itself against gravity, through a height equal to half the difference between the lengths of the long and short axes. This seems paradoxical, but the man who is in the habit of spinning tops knows that this is the proper position of the body, that it must stand up in this way when spinning rapidly on a rough horizontal plane.

This experiment may be performed at the breakfast table with an egg as the spinning body. But the egg must be solid within—that is, it must be hard-boiled; a raw or soft-boiled egg will not spin. Perhaps this is why Columbus did not adopt this method for his celebrated experiment; there may, of course, have been other reasons.

It is thus made clear that by causing a body to rotate rapidly we endow it with new and strange properties. Between a top when spinning and the same top when not spinning there is a difference which reminds us of that between living and dead matter; and this will strike us still more forcibly when we consider some more complicated cases of rotational motion. The top, the ordinary spinning-top of the schoolboy, stands on its peg and "sleeps" in the upright position, in contempt of all the laws which govern statical equilibrium.

The experimental study of spinning-tops is carried on by very small boys and a few more or less aged people. Somehow, but I think quite wrongly, a top is regarded as a toy suitable only for a child, and that kind of amusement is scarcely encouraged by the benevolent despots who so completely direct the games of boys at school. Among older boys there used to be a regular game in Scotland of "peeries," and some of you may have read Clerk Maxwell's poetical description of the Homeric contests which distinguished the sport.

The top as a plaything is despised; nevertheless it is a most important contrivance. The earth on which we live is a top, and a considerable range of astronomical phenomena are most easily explained by reference to the behaviour of ordinary spinning tops. It is a top that directs the dirigible torpedo, that controls the monorail car, which may soon rise from the position of a small model to that of an important affair of practical railway engineering, and that in the gyrostatic compass gives a direction-pointer unaffected by the iron of the ship, or the rolling and pitching of the vessel. Its properties (summed up in what we call gyrostatic action) have to be reckoned with in all swift-running machinery, such as fast-speed turbines, and rotary engines of all kinds, especially if these drive flywheels or propellers. They affect very seriously the stability of aeroplanes, and even of submarines, and I am very doubtful if aviators have yet become in sufficient degree instinctively alive to the dangers of sudden turnings, such as those which are encouraged by the promoters of aviation displays in alighting competitions.

The man who has spun and studied tops and gyrostats appreciates as no one else can the extreme importance of properly balancing rotating machinery, and of avoiding gyrostatic action where such action is likely to interfere with the running of the machine as a whole.

The properties of a top are best studied in the gyroscope, or gyrostat, as it is better called. Here is a simple gyrostat, of the ordinary form sold in the toyshops, but with some important modifications to enable it to run for a long time at a high speed. It consists, as you see, of a heavy-rimmed metal disk, or flywheel, capable of rotation with but little friction on pivots held in sockets attached to a metal frame. Thus the flywheel may, by the quick withdrawal of a string wound round its axle, or in some other way, be set into rapid rotation in the frame, which in turn is mounted in various ways to show gyrostatic effects. But this ordinary form, as well as some others of a more pretentious character, suffers from the great disadvantage of having no means of maintaining the spin, and the continual renewal of the spin is a great nuisance.

I have here a gyrostat (Fig. 1) in which this drawback has been

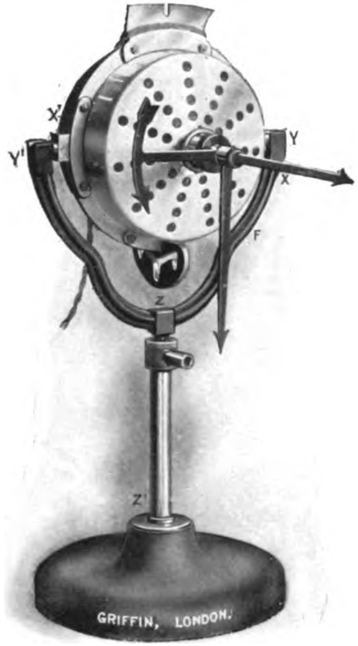

Fig. 1.—Motor-Gyrostat in "Fork and Pedestal" Mounting.

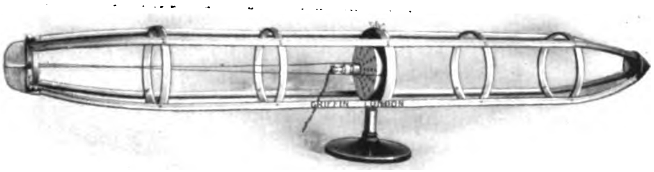

Fig. 2.—Motor-Gyrostat mounted to Demonstrate the Principle of the Dirigible Torpedo.

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overcome by the simple and effective device of making the flywheel itself the rotor of a high-speed continuous-current electric motor. The ordinary gramme-ring armature is well adapted for this. It gives a wheel of great moment of inertia, or as I call it "spin inertia" (that is, the matter of the wheel is distributed so as to be on the whole as distant from the axis as possible), which can be run at high speed for a long time without trouble of any kind from bearings or contacts.

For my first experiments the motor gyrostat is set up with the axis of the flywheel horizontal, in this mounting, which consists, as you see, of a fork perched on a pillar. Notice the possible motions, the freedoms, I may call them, of the arrangement. The flywheel can turn about its axle, the case can turn about the line of the pivots which carry it in the fork, and the fork about a vertical axis provided in the pillar. These three axles, which we shall number (1), (2), (3), are mutually at right angles and meet at the centre of gravity of the movable system or gyrostat proper. When thus set up the gyrostat is said to be freely mounted.

With the flywheel at rest I push down on one side of the case, and immediately turning takes place, as we should expect, about the axis (2). Pushing down the other side of the case causes the instrument to turn about the axis (2) in the opposite direction. I grasp the fork in my hands and turn it about the axle (3) in either direction. Nothing unexpected happens; the gyrostat turns with the fork, its axis remaining horizontal throughout. Again, I grasp the pillar in my hands and turn it on the table, and you see that the friction of the axle (3) is sufficient to cause the fork and gyrostat to move round with the pillar. As before, the axis of the flywheel remains horizontal.

My assistant now causes a current of electricity to flow in the coils which form part of the flywheel and in the coils which surround the soft iron core of the magnet which is stationary within the ring. So far you can only tell that the flywheel is turning by the faint hum which its motion sets up. But when I repeat the operations which I have just performed on the non-rotating gyrostat, the behaviour of the instrument is quite startlingly different. I push down on one side of the case as before; a resisting force is experienced, and the gyrostat turns, not visibly about the axle (2), but about (3), the vertical axis. So long as I maintain the tilting force so long does the resistance and this turning about the vertical persist. I withdraw the tilting force, and the turning motion ceases.

Now I would direct attention to these rods with arrow heads, which are screwed to the gyrostat case. This curved one shows the direction in which the flywheel is spinning. The straight rods are intended to represent the spin-momentum and the tilting action respectively. Both are completely known when their amounts and their planes are known. The spin-momentum is got by multiplying two numbers together, one representing the spin-inertia of the wheel (which is greater the more the mass is placed in the rim), the other the speed of turning. The turning action or "couple" is also got by multiplying the force with which I push by the arm or leverage of the force about the axis. So then we represent these two by lines drawn at right angles to the two planes, making the lines of lengths to represent the two products. Standing on one side of the plane of the flywheel, you see it turning against the hands of a clock; standing on one side of the plane of the turning action which I apply, you observe that action tending to turn the body also against the hands of a clock. The two lines representing the two products drawn towards you from the two planes represent also the directions of the turning actions of the couples. For example, the direction of rotation of the flywheel being that shown by the curved rod, the line representing the spin-momentum points outwards from the side of the gyrostat to which the rods are attached. I call this the *spin-axis*. The other line, representing the turning action which I applied, I call the *couple-axis*.

Now observe that I set the couple-axis so as to point toward your left. I push down the side of the gyrostat nearest me, and you see that the spin-axis turns towards the left. Again, I turn the couple-axis so as to point to your right. When so placed it represents a turning action tending to depress the end of the axle of the flywheel that is nearer you. I apply such an action, and the spin-axis turns towards your right. In both cases the spin-axis turned towards the instantaneous position of the couple-axis.

Now I set the couple-axis vertical, pointing up. It represents a turning action tending to produce horizontal turning in the counter-clock direction as seen from above. I apply such an action to the fork, when you see that the gyrostat turns the spin-axis towards the upward direction. Finally, I set the couple-axis vertical but pointing down, as in Fig. 1. It now represents a turning action tending to produce clock-wise rotation as viewed from above, counter-clock rotation as seen from below. I apply the action represented and the gyrostat turns the spin-axis towards the downward direction.

These experiments may be summed up as follows:—The flywheel is spinning about axis (1). Any attempt to tilt the gyrostat about axis (2) produces turning about (3); an attempt to tilt it about (3) produces turning about (2). This response of the body seems paradoxical, but in point of fact, and this is the secret of the whole affair, *this turning of the body as a whole amounts to the production of spin-momentum about the couple-axis at exactly the proper rate.* It is quite easy to prove this by the consideration, in the most elementary way, of the accelerations of the different particles composing the wheel.

The turning of the spin-axis towards the couple-axis is called a precessional motion, from a similar motion of the earth which pro-

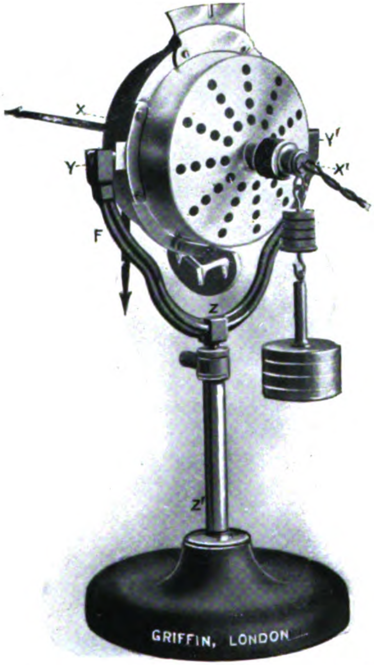

FIG. 3.—MOTOR-GYROSTAT IN PEDESTAL, WITH WEIGHT ATTACHED.

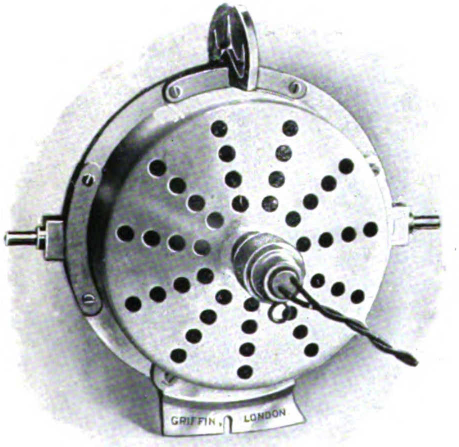

FIG. 4.—MOTOR-GYROSTAT BALANCING ON A SKATE.

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duces the astronomical phenomenon called the precession of the equinoxes. The turning action, or couple, as I shall now call it, may be said to cause the flywheel to "precess" towards the couple-axis. This relation of directions is very important, and should be kept always in mind.

If this turning response of the body, about an axis which we shall call (3), is prevented when turning about an axis (2), at right angles to (3), is changing the direction of the axis of a rotor—an axis (1), say, at right angles to (2) and (3)—a preventing couple, usually called *gyrostatic*, about the axis (3), must be applied by the bearings to the axle of the rotor, and therefore an equal and opposite couple by the axle to the bearings. This couple, it is easy to prove, is equal to the product of the spin-momentum and the angular speed at which the direction of the axis of the rotor is being changed. Thus the greater the moment of inertia of the rotor, or its angular speed, or the angular speed of the change of direction of the axis, the greater is the gyrostatic couple.

For example, the rotor of a dynamo, mounted on one of the decks with its rotor-axis athwart ship, applies, when the ship rolls, a couple to the bearings, the plane of which is parallel to the deck, and which consists of a forward force on one bearing and a sternward force on the other. These forces are reversed with reversal of the direction of rolling, so that an alternating force is applied to each bearing tending to shear it off the deck. Thus if the bearings are at all loose, the axle will knock alternately on the front and back of each bearing.

Similarly the axle of the rotor of a fore-and-aft turbine, when the ship pitches, applies a force to port to the bearing at one end, and a force to starboard at the other end, which forces are reversed when the direction of the pitching motion is reversed. When the course is being changed the forces of the gyrostatic couple are applied to the top of one bearing and the bottom of the other.

Now, returning to the pillar-gyrostat, and putting the flywheel in rapid rotation, I turn the pillar round on the table. I have turned, as you see, the base round through one revolution, and throughout the turning motion the axle of the flywheel has remained pointing in the same direction. The friction at the axle about which I have turned the pillar, which you will remember, was sufficient to carry the gyrostat round when there was no spin, is now quite insufficient to cause any serious change of position of the gyrostat. Only a very small couple producing precession acted.

This experiment illustrates the principle of permanence of direction of the axis of rotation, in the absence of a couple producing precession, the principle on which depend the gyrostatic compass and the self-directing torpedo. Carried within the body of the torpedo is a fast-spinning gyrostat, and at the instant at which the torpedo leaves the impulse-tube this gyrostat is mounted freely with its axis coincident with that of the torpedo—that is pointed, so to speak, exactly along the "cigar." Any turning of the torpedo body sideways brings about a relative shift between the gyrostat and torpedo axes, and this shift brings into operation a vertical rudder at the stern of the torpedo. If the nose of the torpedo turns to port, the rudder steers the craft to starboard, and *vice versâ*.

Here (Fig. 2) is a skeleton frame representing a torpedo. It is mounted on a vertical axle, and carried on pivots within the structure is one of our motor-gyrostats. At the stern of the frame is a small rudder, and this is connected by means of cords to the gyrostat. I set the flywheel in rotation. When, as I do now, I turn the nose of the torpedo to port, the rudder steers to starboard; when I turn the nose to starboard the rudder steers the craft to port.

The case of the pedestal gyrostat is provided with a hook at one extremity of the axis (see Fig. 3). The effect of hanging a weight on this hook is to apply a couple tending to cause turning about the axis (2)—that is, which would produce such turning if the fly-wheel were not spinning. But the wheel is spinning, and the visible actual turning is about the axis (3). Observe also that the wheel is rotating comparatively slowly, and that the precessional motion is great. I increase the speed of the fly-wheel and the gyrostat precesses more slowly. I replace the weight by a larger one, and for the same spin the precessional motion is greatly increased. Thus for a given applied couple the faster the spin the slower the precessional motion, and for a given spin the greater the couple the faster the precessional motion.

Now while the weight is in position and the gyrostat precessing about the axle (3), I attempt to hurry the precessional motion, and immediately the gyrostat turns about the axis (2) so as to *rise* against gravity. I try to delay the precession, and again the gyrostat turns about the axis (2), but now so as to descend under gravity.

Without being aware of it, people are constantly meeting with examples of gyrostatic action in daily life. A child expert in trundling a hoop causes it to turn its path to the right or left, by striking it a blow at the top with the hoop-stick, the effect of which the ordinary person would suppose, if he thought about it, should be to make the hoop to fall over to the right or the left. A bicyclist riding without holding the handles leans over to the right if he wants to steer the bicycle to the right, and to the left if he wants to steer to the left. And if he feels himself falling over to right or left he turns the handles instinctively so as to turn the bicycle to that side, when the machine resumes the upright position. In the bicycle, however, the spin of the wheels is not the most important action to be taken account of.

The gyrostatic action in the bicycle is much more marked in a motor machine, for in that a massive fly-wheel rotates in the same

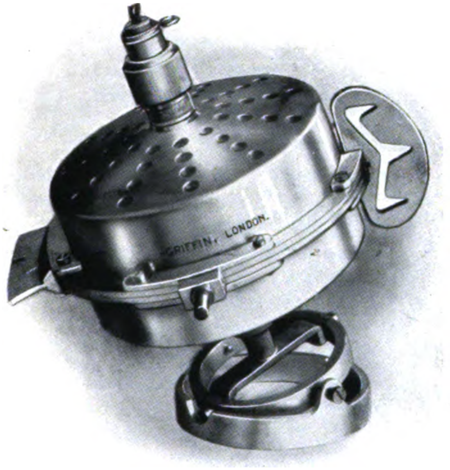

FIG. 5.—MOTOR-GYROSTAT ON GIMBALS.

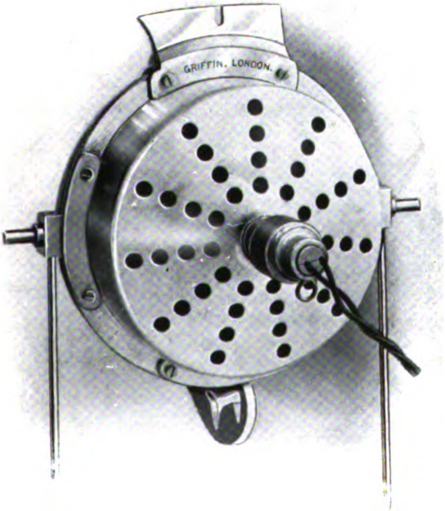

FIG. 6.—MOTOR-GYROSTAT BALANCING ON STILTS.

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direction as the wheels. As the bicycle turns a corner it is constrained to precess, and a couple is needed to produce this precession of the rotating parts quite apart from that required to turn the rest of the machine. This the rider applies by leaning over to the *inside* of the turn, and leans over more than he would have to if the fly-wheel were not there or were not rotating.

Good examples of gyrostatic action are given by paddle and turbine steamers. A paddle steamer is steadier in a cross-sea than a screw steamer of the same size. This is due in part to the gyrostatic action of the paddle-wheels, which, but for their comparatively slow speed of rotation, would form a compound gyrostat of considerable power. For this gyrostat the spin-momentum may be conveniently represented by a line drawn from the steamer towards the port-side. A couple tending to tilt the steamer over to starboard is represented by a line drawn towards the bow, and a couple tending to tilt the steamer to port by a line drawn towards the stern. Hence, if the steamer heels over to starboard, her bow, in consequence of gyrostatic action, precesses to starboard, but the starboard wheel, becoming somewhat more deeply immersed, uses more power and exerts a turning influence to port. Thus the steersman has less difficulty in keeping the vessel on a straight course. But if the vessel be turned by the rudder, say to port, the vessel will by gyrostatic action be slightly heeled over to starboard, and the starboard wheel, being more deeply immersed, will assist the turning-action of the rudder. When, however, the steamer falls off her course, to port or starboard, the gyrostatic action causes the correcting action applied by the rudder to be resisted. Though the gyrostatic action of the wheels is not very great, calculation shows that it is enough to produce an appreciable variation in the immersion of the wheels.

The gyrostatic action of the fly-wheel in a motor-car is of some practical interest. The fly-wheel is placed with its plane athwart the car—that is, with the axis, so to speak, fore and aft. It rotates in the clockwise direction as viewed by an observer behind the car. The effect of turning a corner to the left gives a gyrostatic couple throwing the weight of the car more on the back wheels; turning to the right throws the weight more on the front wheels. The forces applied by the ground to the front wheels are diminished in the former case and increased in the latter. There is danger, therefore, of the steering power of the car being interfered with, if the corner is taken at too great a speed.

As a final example, we take an aeroplane. Here the rotor of the engine and the propeller together form a compound gyrostat of considerable power. As the bearings are fore and aft, the action is similar to that of the fly-wheel of the motor-car. Turning horizontally in one direction gives rise to the gyrostatic couple tending to make the aeroplane dive turning the opposite way sets up a couple which makes the aeroplane rear up in front. If the aeroplane is kept horizontal such couples have to be balanced by stresses in the framework. These considerations show that sudden turning of aeroplanes should if possible, be avoided. Manœuvres calling for such turning are accompanied by very considerable danger. No doubt aviators are aware of the existence of gyrostatic action, but there is considerable haziness in people's minds as to its direction in the various possible cases. The peculiar properties of rotating bodies need not, of course, be understood theoretically by aviators, though it is well to know something about them. But the aviator, like a person walking or swimming, must know instinctively what to do in an emergency, and what motions must be avoided. The gyrostatic action he has to contend with lies hid, as it were, until he tries some new and violent manœuvre ; and then it brings him to grief.

I now pass on to some special experiments which can be carried out with these motor-gyrostats. First take one or two old experiments (see Thomson and Tait's "Natural Philosophy," § 345^{x} *et seq.*), which are more effectively performed with these fast-running instruments. Here is a skate attachment (Fig. 4) on which I place the gyrostat after its speed has been adjusted to the moderate value of about 6000 revolutions per minute. The plane of the flywheel is inclined to the vertical, and you see that the top does not fall down, but precesses round on the table. I increase the inclination and the precession becomes more rapid. Now I attempt to hurry the precession, and the gyrostat stands up erect ; I try to resist the precession and the gyrostat falls over.

I mount the gyrostat with its wheel horizontal over a flexible support, in the present case a universal joint (Fig. 5). Without rotation the instrument would fall over at once, but you see that it stands stably erect when the flywheel is spinning, and has a precessional motion when disturbed from the upright position.

Again, here is a two-stilt support (Fig. 6). One of the stilts is held by a long socket, at one side of the case, and may be regarded as rigidly attached. The other stilt is simply a bit of wire pointed at both ends ; one end rests on a table, the other, the upper end, rests loosely in a hollow in the upper-side of this projecting piece attached to the case. The gyrostat is thus supported between two stilts, one fixed the other quite loose, and its axis is at right angles to the plane of these when the arrangement stands upright. It would be hard to devise a more unstable support. You see that there is no possibility of making the arrangement stand up without spin. But you see, on the other hand, that there is a fair amount of stability with the flywheel spinning if the arrangement is allowed to oscillate, or, as one might say, wriggle, backwards and forwards, horizontally.

In the next experiment (due originally, I have been told, to the late Prof. Blackburn) the gyrostat is rigidly clamped to this metal bar,

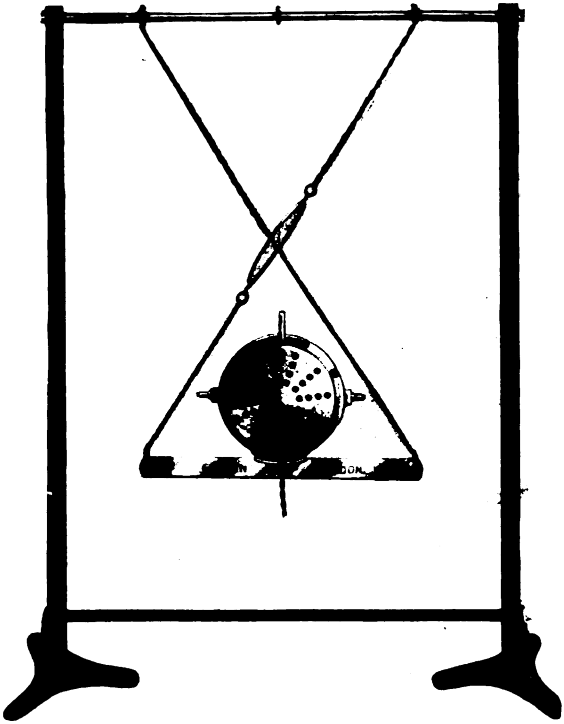

Fig. 7.—Motor-Gyrostat on Crossed Bifilar Support.

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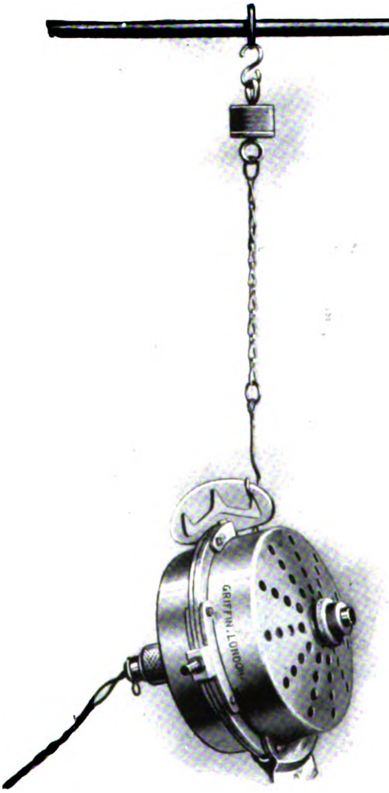

Fig. 8.—Motor-Gyrostat Precessing on Chain Support.

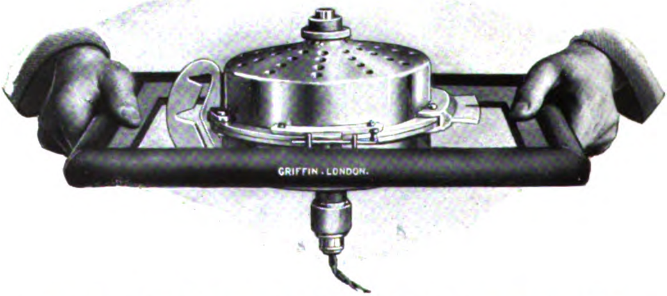

Fig. 9.—Motor-Gyrostat Mounted to Demonstrate the Principle of the Gyrostatic Compass.

which as you see, is hung by two chains attached to its ends (see Fig. 7). The chains have been crossed by passing one through a large ring in the middle of the other. I turn the gyrostat so that the chains and the rim of the case are in the vertical plane. You observe that the arrangement is one of instability. The gyrostat has perfect freedom to fall over towards you, or towards me. Further, in consequence of the crossing of the chains the gyrostat is unstable as regards motion about a vertical axis. The arrangement is thus doubly unstable without rotation.

I now set the flywheel into rapid rotation, arrange the instrument as before, and leave it to itself, when, as you observe it balances with great ease.

I now repeat the experiment with the chains uncrossed. Here there is only one instability without rotation, and the gyrostat falls over. An important point to be observed is that the rotation will stabilise two non-rotational instabilities, but not one. In point of fact, a system possessing non-rotational freedoms, all of which are unstable, can be stabilised if the number of freedoms is even, but not if the number is odd.

A general explanation of the experiment just performed may be given as follows. Starting with the bar, gyrostat rim, and chains (crossed) in one vertical plane, we may suppose the gyrostat to fall over slightly. In consequence of the tilting couple introduced the gyrostat precesses so that its axis turns in a plane which is nearly horizontal. The chains now get slightly out of the vertical, and at once a couple hurrying the precessional motion is brought to bear on the gyrostat, which, in consequence, erects itself into the vertical position. The couple does not retard but hurries the precession because the bars are crossed. This holds for both directions in which it is possible for the gyrostat to fall over. Again, suppose, starting with the rim, bar, and chain in the same vertical plane, the chains get out of the vertical. There is now a couple brought to bear on the gyrostat tending to turn its axis in a horizontal plane. In consequence the gyrostat tilts over on the bar—in other words, it has a precessional motion about a horizontal axis in the plane of the flywheel. This brings into action a couple due to gravity, which is such as to hurry the last-mentioned precessional motion; the horizontal motion is opposed and reversed, and with the reversal the gyrostat regains the upright position. This holds for both directions in which the bar tends to turn in consequence of the crossed chains. The result is complete stability.

Similar explanations are applicable to the other cases of motion you have seen.

I now suspend the gyrostat from the horizontal beam by means of this chain terminating in a hook (Fig. 8), which engages in a central recess of the rim attachment. The chain carries a ball-bearing race. I place the gyrostat with its axis horizontal and leave it to itself. The centre of gravity of the gyrostat lies vertically below the hook, and under those conditions there is no couple tending to tilt the instrument. I transfer the hook to one of the side recesses, set the gyrostat so that its axis is horizontal, and leave it to itself, when, instead of falling down it turns its axis in a plane which is nearly horizontal. If I delay the precessional motion the gyrostat descends, if I accelerate the precession the gyrostat ascends. I transfer the hook to the opposite side recess, place the gyrostat so that its axis is horizontal, and again let go. The gyrostat precesses as before, but in the opposite direction. Again I hurry the precession, and again the gyrostat rises; again I delay the motion, and the gyrostat descends.

In these experiments, when the hook engages in either of the side recesses there is a couple due to gravity tending to produce angular momentum in a vertical plane. The axis of spin-momentum turns towards an instantaneous position of the couple axis at right angles to it, at angular speed ω say. If μ be the spin-momentum, and the top has been properly started, angular momentum *about the couple-axis* is being produced at rate $\mu\omega$ by this turning, and this is equal to the moment of the couple. The precessional moment remains at the value required to give just the rate of production of angular momentum corresponding to the couple. This is the point generally missed in popular explanations of the gyrostatic action.

It is important to notice, however, that as these experiments are usually carried out, the precession, though apparently steady to the eye, is not, strictly speaking, perfectly steady. There is a very slight alternate rise and fall of the axis. To get quite steady motion, the top must not be simply spun and then left to itself; it must be started with the right amount of precession.

I now place the gyrostat within this wooden tray (Fig. 9). The pivots carried by the rim of the gyrostat engage on bearings provided in the tray, and these are on a level with the centre of gravity of the whole. I hold the tray so that its plane is horizontal, and carry it round in a horizontal circle. Nothing happens. Still holding the tray so that its plane is horizontal, I carry it round in a horizontal circle in the reverse direction. The gyrostat immediately turns a somersault, and is thereafter stable. If I reverse the direction of rotation of the tray, again the gyrostat turns a somersault, and remains again quiescent.

The gyrostat is stable, with its axis vertical, so long as the direction of spin coincides with that in which the tray is being turned. If this latter direction is reversed, the gyrostat turns a somersault so as to render the two directions coincident. It appears as if the arrangement had a will of its own, and refused to be carried round against its direction of spin.

The theory of this experiment is very instructive. Both cases are represented by one differential equation, but in one case there is a

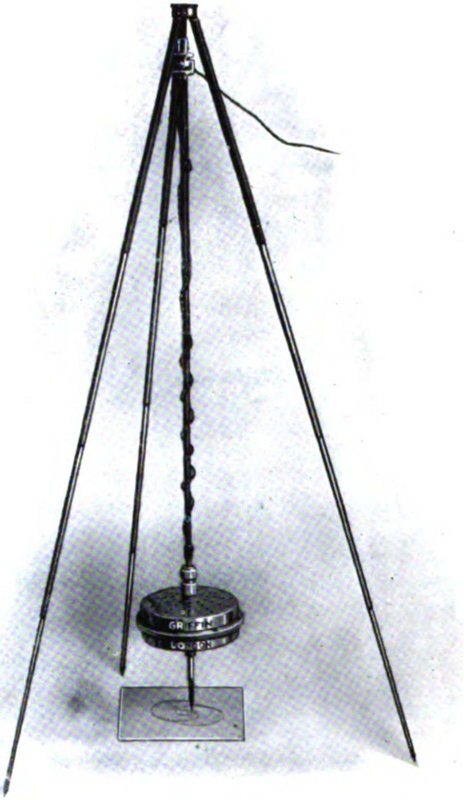

FIG. 10.—MOTOR-GYROSTAT FITTED UP AS A GYROSTATIC PENDULUM.

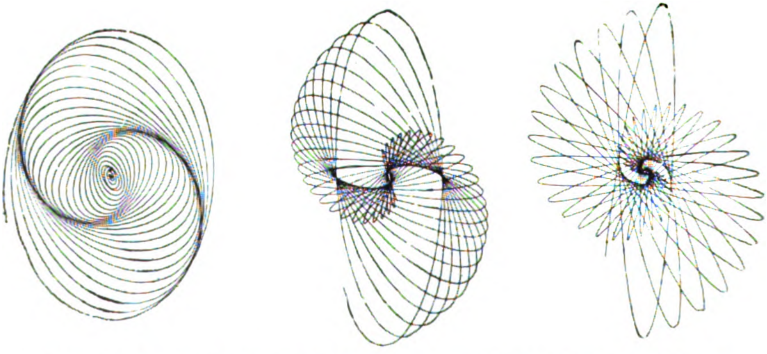

FIG. 11.—SOME CURVES OBTAINED WITH THE GYROSTATIC PENDULUM.

real period of vibration about the vertical; in the other the period is mathematically unreal, and the gyrostat axis moves farther away from the vertical. No better illustration of the two cases of the equation can be found.

The behaviour of the tray gyrostat is exemplified also in the gyrostatic compass. A heavy and rapidly rotating flywheel is mounted so that its axis is maintained horizontally by means of an elastic support. Under these conditions the equilibrium position of the flywheel under the horizontal component of the turning velocity of the earth (which corresponds to the turning of the tray) is arranged to be that in which the axis of rotation points due north and south. If time permitted, I should be glad to make an experiment with a carefully balanced motor-gyrostat, which would not only show the turning of the earth under the gyrostat, but enable the rate of turning to be measured.

I would now direct your attention to this motor-gyrostat, which forms the bob of an ordinary compound pendulum (Fig. 10). The tube carrying the gyrostat is attached, by means of a universal joint, to the apex of a triangular stand, made of telescope tubing. The gyrostat is attached to the lower end of its supporting tube, by means of a special cap provided with spring contact pieces, to allow the current to be led into the motor, and the flywheel is free to rotate about an axis coincident with the rod. Screwed to the lower side of the gyrostat is a pen, which presses lightly on a card placed below.

We have now the pendulum rod in the vertical position. I draw the pendulum to one side and let go, when you see that it vibrates to and fro, and the pen traces out a straight line on the paper. The flywheel has as yet no spin. I start the flywheel revolving, draw the pendulum to one side, and let go, either from rest, or with a certain amount of sidelong motion, when you observe that the pen describes a flower-shaped path (Fig. 11). The path is shown for different amounts of sidelong motion. The peculiar appearance of these curves is due to the rapid falling off of amplitude produced by friction.

When the flywheel is revolving there are, in general, two couples acting on the pendulum, one due to gravity, the other due to gyrostatic action. At an instant at which the axis of the gyrostat is vertical the former couple is zero, and the latter one is a maximum, for at that instant the angular velocity with which the axis of the gyrostat is changing direction is greatest. When the pendulum is at one extremity of its swing the former couple is a maximum, and the latter one is zero. At that instant the deflection of the bob from the vertical is a maximum, and it is at rest, or is moving sideways, according to the mode of starting, except in so far as the initial conditions have been interfered with by friction. By this relation of the couples the form of the path can be explained.

Another mode of motion is possible which has a very intimate connexion with the theory of vibrations of light-emitting molecules in a magnetic field, as indeed I pointed out here several years ago in a Friday Evening Discourse (see "Nature," April 13, 1899, and August 24, 1899). The bob can be made to move in a circle about the vertical through the point of support either with or against the direction of rotation of the flywheel. The two periods are different, and the motions correspond to the circularly polarized light of two distinct periods, which molecules, situated in a magnetic field, are found to emit. Thus the gyrostatic pendulum gives a dynamical analogue of the cause of the Zeeman effect.

In 1907 Herr Otto Schlick introduced a method of employing a gyrostat to counteract the rolling of a vessel at sea. The gyrostat is carried on bearings placed athwart the ship. These bearings are in line with the flywheel, and a weight is attached to the frame of the gyrostat in a position in line with the axis. It will be seen that when the ship is on even keel the gyrostat rests with its axis vertical, and with the weight vertically below the centre of gravity of the flywheel. Heeling of the ship in one direction causes the gyrostat to precess in one direction on the bearings on which it is mounted; heeling in the other direction causes precession in the opposite direction, and couples resisting the rolling motion are brought to bear on the ship. The device may be employed in two ways. In the first place, if the bearings on which the frame of the gyrostat is carried within the ship are smooth, the effect of the gyrostat is to resist the rolling force of the waves, and to bring about a lengthening of the free period of the ship, according to a mathematical theory which, when put in the proper way, is really very simple. Excessive rolling of a ship is due to the cumulative action of the waves, and such cumulative action is only possible where the period of the ship and that of the waves are of about the same order. A large ship has a very long period, and synchronism of the ship and the waves is impossible. The effect of introducing a gyrostatic control, operated in the manner just described, is to endow the small ship with the period of a very large one.

In the second mode of operating the gyrostat, friction is introduced at the bearings on which the frame of the gyrostat is mounted. With this addition the ship is forcibly prevented from excessive rolling. In the trials of the device it was found that, with the control in operation, the angle of roll of the ship did not exceed 1° in a cross-sea which produced a total swing of 35° when the control was out of action. It is interesting to notice that, contrary to the opinions which were expressed when the device was first suggested, the preventing of the rolling of a ship does not result in the waves breaking over her; a ship controlled by a gyrostat is, I believe, a dry one.

I have here a motor-gyrostat fitted within a skeleton frame representing a ship (Fig. 12). The frame is mounted on two bear-

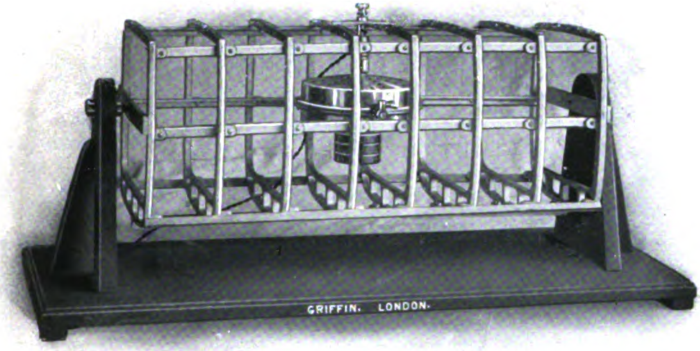

FIG. 12.—MOTOR-GYROSTAT FITTED UP TO DEMONSTRATE SCHLICK'S METHOD OF STEADYING A SHIP IN A CROSS SEA.

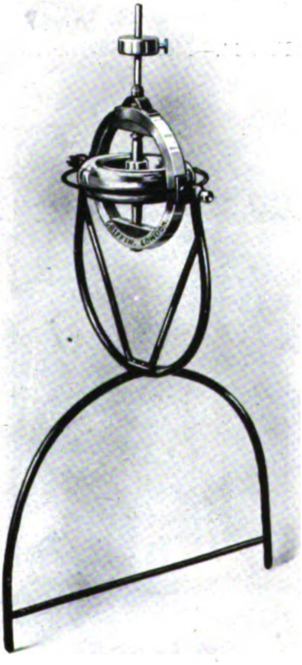

FIG. 13.—NEW MONORAIL-TOP.

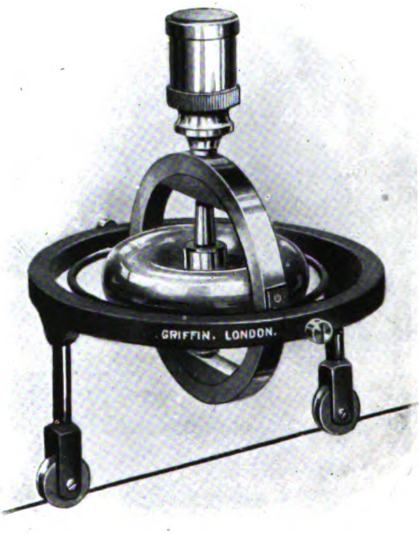

Fig. 14.—Monorail-Top on Wire.

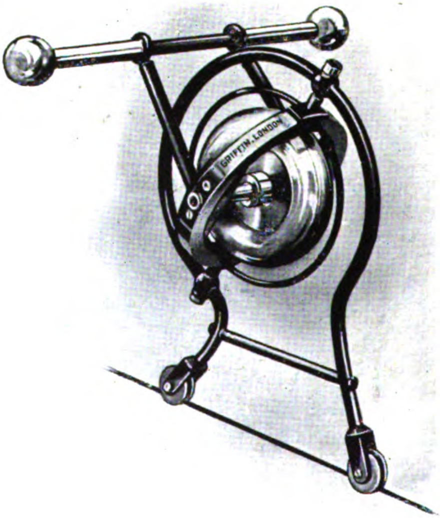

Fig. 15.—Pole-Balancing Top.

ings arranged on wooden uprights, and may be made to oscillate on these bearings, so as to imitate the rolling of a ship in a cross-sea. The frame of the gyrostat is mounted on two bearings placed athwart the frame, and a weight is attached to the outside of the case in a position in line with the axis of the flywheel. The centre of gravity of the gyrostat is in line with the bearings. A clip-device is provided which allows the gyrostat to be clamped to the skeleton frame, and provision is made whereby a graded amount of friction may be applied at one of the bearings.

I now set the skeleton frame vibrating with the flywheel at rest. You observe the period. I start the motor-gyrostat, and repeat the vibrations, with the gyrostat clipped to the frame. The ship rolls precisely as before. I free the gyrostat from the frame, and again set the ship rolling, when you see that not only is the period vastly increased, but the rolling motion is quickly wiped out.

When the gyrostat is clipped to the frame it produces no effect upon the rolling motion. The couples opposing the rolling motion arise from the precessional motion, and hence the gyrostat must be given freedom to precess. In this connexion it is interesting to observe that in 1870 it was proposed by Sir Henry Bessemer to obtain a steady cabin for a cross-channel steamer by placing it on a gyrostat with its axis vertical and supported on fore-and-aft trunnions. This plan was bound to fail. The dependence of the effect on freedom of the axis to precess, in a direction which is not that of rolling, was not understood. We now see that the object would have been attained by supporting the cabin on fore and aft trunnions and mounting the gyrostat, within the cabin, on trunnions placed athwart the ship.

Here is a monorail-top of new design (Figs. 13 and 14). The frame on stilts represents the car, and mounted on pivots placed across the frame is a gyrostat. Carried by a rod fixed to the frame of the gyrostat, and in line with the axis of the flywheel, is a weight. When the frame is placed on the table so that the legs and axis of the gyrostat are vertical, with the weight above the flywheel, the arrangement is doubly unstable without rotation; the system of gyrostat and weight is usually mounted on the pivots, and the entire structure is unstable about the line of contact of the feet with the table. When the flywheel is rotating, however, the top balances on the table. The two non-rotational instabilities have been stabilised.

I now place the top on the table with the legs and axis of the flywheel vertical, but with the weight below the gyrostat. The arrangement is unstable. Here there is only *one* instability without rotation, and the result is instability with or without rotation.

Here is a stilt-top similar to the one just shown, but provided with wheels adapted to engage on a stretched wire. You observe the remarkable balancing power of the arrangement.

In this top (Fig. 15) a gyrostat is pivoted within a structure which represents a tight-rope balancer. The structure terminates in wheels adapted to engage on the wire. Attached to the gyrostat are two arms, and carried by these is a light rod weighted at both ends. My assistant spins the flywheel and places the structure upon the wire with the legs vertical and the pole horizontal. The top, as you observe, balances on the wire. If the top tilts over on the wire towards me, the gyrostat precesses in the direction which carries the pole over towards you, and *vice versâ*. That is, if the balancer begins to fall over to one side it immediately puts over the pole to the other side. The action is exactly that of a tight-rope acrobat.

The rider of a bicycle keeps the machine upright by operating the handle-bar. If the machine tilts over to the left the rider turns the handle-bar to the left, and the forward momentum of the bicycle and rider, aided by the gyrostatic action of the wheels (a relatively small factor in this case) results in the erection of the machine. Similarly, if the machine tilts to the right the front handle-bar of the machine is turned to the right.

Here I have a small bicycle of the old-fashioned "high" type, provided with a gyrostatic rider. When the gyrostat is spinning rapidly you observe that the top is completely stable. The gyrostat operates the front wheel, just as does the rider on the ordinary bicycle.

Again, here is a small safety bicycle provided with a gyrostatic rider (Fig. 16). In this case the gyrostat is mounted above the back wheel, and is connected by arms to the handle-bar of the front wheel. The action is the same as in the other model.

The tops I have shown you are very interesting from the fact that in each case the gyrostat not only detects but sets about correcting any tendency of the top to fall over. They behave as if they possessed both a nervous and a muscular system.

I have also here a gyrostat which can be made to progress in space by a reciprocating motion—in fact, a walking gyrostat (Fig. 17). The gyrostat is suspended by two chains from two horizontally stretched wires. The wires are carried by a wooden frame, which is mounted, as you see, on two trunnions carried by wooden uprights. The chains attached to the arms of the gyrostat terminate in two rings, and these are threaded on the stretched wires.

The gyrostat is spun and replaced on the wires. When the frame is tilted to and fro on the trunnions, the gyrostat walks "hand-over-hand" along the wires. By the tilting of the frame the weight of the gyrostat is thrown alternately on each of the chains, and in consequence of the precessional motion the gyrostat moves along, carrying the chains with it.

At present the spin is great, and therefore the precessional motion is small. The gyrostat proceeds with a slow and stately motion. As time goes on the spin falls off, and the rate of walking increases, until finally the gyrostat literally runs along the wires,

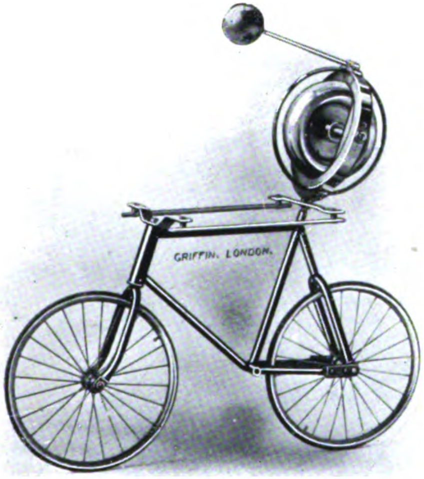

FIG. 16.—GYROSTATIC BICYCLE RIDER.

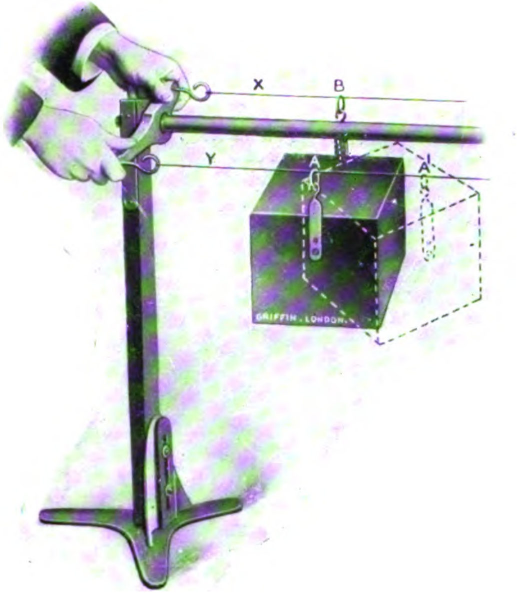

FIG. 17.—"WALKING" GYROSTAT.

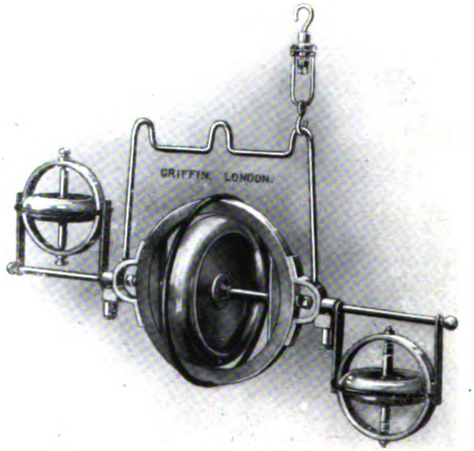

Fig. 18.—Acrobatic Top.

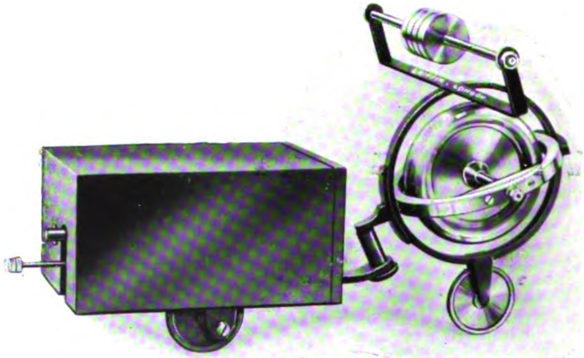

Fig. 19.—Motor-Car.

with considerable loss of dignity. When the gyrostat is enclosed in a box, or within an acrobatic figure, the behaviour seems very mysterious.

Here is still another form of acrobatic top, consisting of a large gyrostat, the axis of which is horizontal, and two small ones, with axes vertical, mounted, one on each side of the large one, on sleeves threaded on a horizontal bar, as shown in Fig. 18. My assistant spins the flywheel of the large gyrostat, which is then suspended by means of a string and hook from the upper bar of the frame. At present the centre of gravity of the gyrostat is vertically below the hook, and under these conditions there is no precessional motion. He now spins the two small gyrostats and attaches them to the large one. Each small gyrostat is carried by two sleeves which are threaded on a horizontal bar. The hook is now transferred to one of the side recesses provided in the upper bar of the large gyrostat, and the system is left to itself, when it turns round in azimuth. One of the small gyrostats throws itself up and balances on the bar. The experiment is repeated with the hook engaging in the other side recess, when you observe that the small gyrostat which previously occupied the lower position now rises into the upright one, and the gyrostat which occupied the upright position now occupies the lower one.

This top admits of a large variety of designs. It is easy to imagine a gyrostatic circus rider performing balancing feats on the back of a gyrostatic horse!

I conclude with a gyrostatic model (Fig. 19) which depends for its action upon an entirely novel and practical method of operating a gyrostat or gyrostats. The method has a very large variety of applications, into which I shall not enter at present. It is here shown applied to a motor-car. The car runs on two wheels in tandem; it can be set to run either in a straight path or a path curved in either direction. The arrangement includes two parts connected by a vertical or nearly vertical hinge. Each is supported on a single wheel. The front part carries a gyrostat with axis horizontal (in this case), the afterpart contains the propelling mechanism. A quasi-gravitational field of force is produced by the propeller behind acting through the hinge, and the construction is such that there is true stability, not the *quasi-stability*, accompanied by continually increasing gyrostatic oscillation, which obtains in many other cases.

[A. G.]

WEEKLY EVENING MEETING.

Friday, February 21, 1913.

Donald W. C. Hood, Esq., C.V.O. M.D. F.R.C.P.,
Vice-President, in the Chair.

Spencer U. Pickering, Esq., M.A. F.R.S.

Horticultural Investigations at the Woburn Experimental Fruit Farm.

In giving a brief account of some of the results obtained at the Woburn Experimental Fruit Farm, I trust that I may be forgiven if I start by mentioning a few facts which are probably familiar to the majority of my audience.

In a flower, such as that of an apple-tree [Slide shown], there is a tubular structure in the centre, forming the female portion of the flower, and that is surrounded and overtopped by a number of rods, bearing at their extremities sacks of pollen—this constituting the male element. When a grain of pollen, either of the same or another flower, enters the central tube, or pistil, fertilization occurs, and a seed, or pip, begins to form at the bottom of the pistil. As it develops, the woody substance surrounding it, which is really a portion of the branch of the tree, gradually swells to a remarkable extent, and eventually forms the fleshy or edible portion of the fruit. [Slides shown.] We commonly call it the fruit, but it is only a metamorphosed portion of the mother-tree: the real fruit of the tree, the progeny of male and female elements, is the pip. When this is sown in the ground, it germinates, and eventually forms a new tree, which, though probably showing some resemblance to its two parents, will be a new variety, and will not bear apples of the same sort as the mother-tree. This is as true with plants as it is with animals: children are not mere reproductions of their parents; but there is this difference in the two cases, that, while we hope and expect our children to be improvements on their parents, the general tendency of the progeny of cultivated fruits is to deteriorate, and to hark back to their primitive and uncultivated ancestors. It is true that all the excellent new varieties of fruits which are raised are produced in this way, but it is only one pip out of many hundreds which will yield a satisfactory tree, the rest being worthless. It will be understood from this how fallacious it is to sow the seeds of some particularly good apple in the expectation that we shall get a tree producing similar fruit—we shall probably get something not much better than a crab-apple. And one reason which makes it all the more improbable that a pip will give rise to a tree bearing fruit like that of the mother-tree, is that in many cases the female portion of the flower cannot be fertilized except by pollen from a tree of a different variety. Why this is so, we do not yet know, but the result is that generally we must have two distinct varieties blended in the seeds of our cultivated fruits. A further result of practical importance is that to get good fruit crops we must, as a rule, have several different varieties planted in the same plantation. Failure of crops is often due to the neglect of this factor.

As it is impossible to reproduce a fruit-tree of any given variety from seed, other methods of multiplication must be adopted, namely, budding or grafting. A young tree of a similar character is taken (the stock), and in the one case a bud, or in the other case a twig (scion) from the tree to be propagated is united with the stem of the stock. [Slides shown.] All the growth arising from this bud, or buds, is similar to that of the tree from which it was taken ; the stock acts as little else than a channel for conveying nourishment to the ingrafted buds. Every existing tree of any particular variety of fruit is thus part of the one individual of the variety which was first raised.

Though a Ribston tree, for instance, will always bear Ribston pippins, and exhibit the peculiarities of growth of the original Ribston tree, independent of the stock on to which it has been grafted, yet this stock does exert a certain influence on the character of the growth of scion. For apples we use two classes of stocks ; the one, the crab stock, is obtained by sowing the seeds of crab apples, and is characterized by forming a scanty number of roots, but these are stout, and have a tendency to obtain deep hold of the ground ; the other, the paradise stock, is derived from a French variety of apple, and forms a much larger number of roots, but smaller, and tending to spread out near the surface of the ground. [Slides shown.] The grafted tree partakes of the character of the roots of the stock ; on the paradise stock it becomes more spreading in its habit, and grows less vigorously than on the crab stock [Slides shown], and, whilst the former is more suitable for growing trees in the bush form, the crab stock is more suited for standard trees. [Slides shown.] It may also be mentioned that trees on the paradise stock come into bearing sooner than those on the crab stock, but do not live so long.

In the case of pears, the corresponding stocks are : the pear stock for standard trees, and the quince stock for bush trees. [Slides shown.]

It must be remembered, however, that the effect of the stock on the growth of a tree is a subsidiary matter ; the characteristics of the growth are mainly dependent on the nature of the scion. [Slides shown.]

Though different stocks are suitable for standard and bush trees, the trees cannot be made to take these shapes without assistance by pruning; and this brings us to a subject on which many experiments have been made at Woburn. We will not, however, start quite so far back as the pruning which is necessary to make a tree take the form of a standard, but begin at the point where the tree has been removed from the nursery to the plantation, generally at an age of two to four years. During such removal many of the roots are destroyed, and nearly all of them are injured. The seat of growth of a root is situated at the extreme tip of the root, the power of multiplication being confined to a few meristematic cells which are centred there, these being protected only by some layers of outer cells, known as the root-cap, which are continually being rubbed off, and reproduced from the meristematic cells, as the root forces its way through the earth. [Slide shown.] The whole root-tip is very minute, and when it is destroyed, growth becomes impossible; but there are certain cells situated at intervals along the roots which are capable of becoming modified and giving rise to new root-tips, just as there are cells in the branches capable of developing into buds if all the visible buds of a tree are destroyed.

When a tree is removed from the soil, most of the root-tips will inevitably be broken off, and the rest will become dried up by exposure to the air, so that the damage to the roots must be serious. But the well-being of a tree depends on the balance between roots and branches, both of which supply certain, but different, elements necessary for growth, and this serious damage to the roots can only be counterbalanced by damaging the branches to a corresponding extent. This is done by severely pruning the branches, cutting them back, as it is termed, to about one-third of their length. The effect of omitting this operation is often disastrous; the tree may become permanently stunted, and even, in the case of plums, which tend to bear heavily after moving, it may be fatal. The general effect is to arrest all growth for the first year, while the branches bear leaves little more than half the size they should be, and incapable of functioning properly. [Slide shown.]

Though good horticulturists agree as to the necessity of cutting back after transplanting, they differ as to the time when this should be done, some advocating its being done at once, others deferring it till the spring, and others, again, postponing it till one year after planting. The results of our experiments on a large number of trees show that the time of cutting back makes little difference to the ultimate size of the tree so long as it is not performed while the tree is in active growth. If it is done in the summer, however, the tree receives a serious check from which it does not recover for at least the next seven years. [Slide shown.] Deferring the cutting back till the following winter does not give the tree any such check as regards its growth, but it affects its fruiting. Such deferred cutting back is generally followed in the second year by vigorous growth, the tree making up for the absence of growth during the first year, and it perseveres in this habit of growing in subsequent years, when it ought to be growing and fruiting as well. [Slide shown.]

Passing on to the question of the annual pruning of a tree, it may be mentioned in the first place, though I will not trouble you with the details, that it has been found, just as in the case of cutting back, that it does not matter at what period of the year this is done, so long as it is not done while the tree is in active growth. As to the extent of this pruning, however, more may be said. It is a common belief that the more you prune a tree the more it will grow. It seems fairly obvious that, even if true at all, this must be true only within certain limits; and, as applied to young freely growing trees, it appears to be quite untrue. Various plantations of similar trees at Woburn have been systematically pruned to different extents during the seventeen years since they were first planted, and the photographs of average specimens from these plantations are sufficient to show that, as regards the general size, the trees which have never been pruned are larger than those which are pruned moderately, and these again are larger than those which have been pruned hard. [Slides shown.] What may be noticed as to the latter is that it is a sturdier tree than that pruned moderately, the trunk and main branches having gone on swelling, while the extension of the branches was prevented by the severe pruning. On the other hand, the unpruned tree, as might naturally be expected, is somewhat straggly and not well shaped.

Another experiment will illustrate the extent to which pruning is opposed to growth. Four strictly similar twigs, 36 inches long, were selected on the same tree: one was not cut back, the others were shortened to 24, 12 and 6 inches, respectively. [Slide shown.] At the end of the following season the weight of these twigs (taking the average of many series) was in the proportion of 562 : 310 : 178 : 100, and from every point of view the growth of the twigs had been greater in proportion as they had been less pruned. [Slide shown.]

In one respect the advantage of reducing the pruning is not apparent from a mere inspection of these photographs: this advantage consists in there being more fruit-buds, and, therefore, a greater promise of fruit, the less the twigs were pruned: the relative proportions of fruit-buds in these cases were 314 : 238 : 165 : 100. That this promise is actually fulfilled in practice, is proved by the records of the crops borne by plantations of similar trees which have for many years been pruned to different extents. In one case these plantations contained three different varieties of apples, it was found that during the first five years, and also during the second five years, the unpruned trees bore twice as much, and the hard-pruned ones little more than half as much, as the trees which had been pruned moderately. [Slide shown.] These trees were on the paradise stock; but the same was found to be the case with apples on the crab stock, for we have another plantation where 117 different varieties of apples are grown, four trees on the one stock, and four on the other; in each case one half of them have been pruned lightly, and one half heavily, and in both cases the crops from the latter have been less than one half of those from the former. [Slide shown.]

What has surprised us is that the heavier crops in these cases have not been accompanied by any serious diminution in the size of the individual fruits; there has been some diminution, which would mean a reduction in the market value of the produce, but not nearly enough to counterbalance the great increase in the total weight of the crop.

It is thus established as a fundamental principle, that the less pruning there is, the more will a tree grow, and the more fruit will it bear. But this does not mean that we should dispense with pruning altogether. The chief object in training a young tree is to make it sturdy and well shaped, so that it will be capable of bearing a heavy crop when it comes to full maturity; to effect this, the extension of the branches must be checked so as to give the stem and main branches time to fill out, and occasionally a branch will have to be removed altogether, either to admit light and air into the tree, or to prevent it rubbing against other branches. To what extent this pruning should be carried, and for how long it should be continued, must depend on the habit of the tree. The instance which has already been shown of an unpruned tree, where the absence of pruning had very little injured the shape and sturdiness of the tree, is that of a variety which is a strong grower and does not bear heavy crops in its youth; but a similar treatment of a weak grower and precocious bearer produces very different results [Slide shown]; the branches are all bent out of shape, and, when loaded with fruit, much of this will be on the ground or whipped off by the wind. Instances such as this may be seen in nearly any farm orchard throughout the country; but examples of over-pruning are almost as general, and are to be found in most private gardens, where the stunted trees throw out every year thickets of twigs, serving no other purpose than that of feeding the bonfire. [Slide shown.] The proper amount of pruning in each individual case, can only be determined by common sense and experience, the guiding principle being to reduce this pruning so far as is consistent with the formation of a sturdy well-shaped tree.

Besides the annual branch-pruning there are other operations included under the term pruning, but the only one to which I can now allude is root-pruning. In this the roots are unearthed and cut back, with the view to increasing the fruitfulness of the tree. The check which such an operation gives to the growth is very severe, and if carried to excess, will kill the tree entirely. [Slides shown.] It is evidently one which should be undertaken only under very exceptional circumstances, such as where the tree is showing rampant growth, and will neither flower nor fruit. We hear little of root-pruning except in private gardens, and we should hardly ever hear anything of it there, if a more rational system of branch-pruning were adopted. When the branches are cut away to an excessive extent, the balance between branch and root can only be restored by cutting the roots away too. But to injure the tree in one way, and to attempt to correct matters by injuring it in another way, is not a very intelligent procedure. Nor does it pay; for though a tree may often bear heavily after root-pruning (generally in the second year after) it was found, even in the case of the trees root-pruned only every fourth year, that the total crops were much smaller than those from the trees which had not been root-pruned, for the trees themselves were smaller, and could not carry so much fruit.

Of course, in cases where we are dealing with wall-fruit, and where there must be excessive pruning to keep the tree confined to the wall, root-pruning may be necessary; but that is a special case.

I will now pass on to the problems connected with the transplanting of a tree from the nursery to the fruit ground. I have already shown you a diagram of the root-tip, and explained that most of these root-tips are destroyed when a tree is transplanted; and, as these tips are necessary for the growth of the tree, its continued existence depends on the formation of new root-tips from other cells specially adapted for the purpose. Many of the old root-tips are torn off in lifting the tree, but others are killed by becoming dried up on exposure to the air. Some exposure is always inevitable, and in most cases several days elapse between the lifting and the planting of a tree. It is of great importance, however, that this exposure should be reduced to the narrowest limits. A number of trees were lifted at Woburn, and some of them were replanted at once, whilst others were left in a shed for four days before doing so, and it was found that the latter made only four-fifths as much growth as the former during the following season. [Slide shown.] It is on this account that planting trees in the spring should be discountenanced, as drying winds are then more prevalent; but if this drying effect is avoided, it is immaterial when the planting is carried out—similar trees planted at different times between November 28 and March 3 were found to do equally well.

Much stress is always laid by horticulturists on the importance of selecting trees with a good supply of fibrous roots, and of taking the utmost care of these roots, spreading them out, and shaking the earth lightly between them. But such precepts are based on ignorance as to the principles of root-growth. Nine-tenths of these roots have lost their tips, they are useless, and as good as dead, for they certainly will die in a very short time. Anyone can satisfy himself on this point: it is only necessary to mark a few of these roots by tying strands of silk round them, and on lifting the tree again at the end of the season it will be found that these rootlets have all, or nearly all, died, and that in their place a new system of rootlets has arisen from the thicker portions of the older roots. [Slide shown.] It is evident, therefore, that it is quite a fallacy to set so much store on a tree being well furnished with fibrous roots, and, still more so, to treat these roots with such care; the only thing that can be said in favour of wanting a tree with such roots is that a tree which has shown a good root-development in the nursery will probably do so again in the plantation, but nothing can be said in favour of tending the old rootlets so carefully; in fact, we have found that trees do better if the smaller of the fibrous roots are removed before planting, and also if all the roots are shortened to a certain extent. The reason of this is not only that it is well to remove parts of the tree which are bound to die, but that the new rootlets which form will be more vigorous if they originate from the thicker portions of the old roots, where the store of material for their nourishment is greater. The practice of leaving the roots as long as possible, and carefully trimming their ends, is quite a mistaken one, for the ends of these roots, having lost the root-tip, cannot start into growth again, and it has been found that of the new rootlets which originate, only some 15 per cent arise from the neighbourhood of the ends of the old roots, the rest originating from higher up towards the stem, or even from the stem itself. [Slide shown.]

Two other conclusions may also be drawn from what has been mentioned, namely, that it can make little or no difference to the future welfare of the tree whether the ends of the old roots are trimmed, or left jagged and torn, as they are when removed from the nursery, nor whether these roots are carefully spread out in the ground, instead of being huddled into the hole prepared for them; for it is the new rootlets which are to be formed, and not the old ones, on which the future life of the tree depends. Both these conclusions have been verified by actual experiment. Even when the roots were twisted and tied together in a bundle, the tree did just as well as when they were spread out in the orthodox fashion. [Slide shown.]

It is thus seen that all these practices which are supposed to be essential to the proper planting of a tree, are really immaterial, and, in fact, that the violation of them within certain limits is beneficial. But the benefit was not sufficient to explain certain results which we obtained, and which puzzled us for many years. We had made a plantation in which, by way of demonstration, the trees had been planted in violation of all the accepted canons, and we expected that these trees would afford an awful lesson to the careless planter. But instead of that, they flourished rather better than their carefully-planted neighbours. The results were naturally set aside as accidental, and a repetition, and subsequently, many repetitions were made: but the roughly planted trees refused to behave badly, and flourished so much more than their neighbours that they often showed two or three times more growth than these did. The principal cause of this was eventually traced to the fact that the soil round these trees had been heavily rammed at the planting, instead of being shaken over the roots, and merely pressed down. When we consider that the welfare of the transplanted tree depends on its sending out new rootlets from the old roots, it is evident that anything which brings the soil into intimate contact with these roots will be beneficial, and ramming the soil down, especially if it is in a wet condition at the time, will do this more effectually than could ever be done by the gentler method of planting. These somewhat surprising results, therefore, receive a simple explanation, and it is easy to satisfy ourselves, by lifting the trees at the end of a year that the rammed ones have actually formed more new roots than the carefully planted ones. [Slide shown.] Such novel methods of planting naturally excited the wrath of horticulturists, who, as a body, are inclined to carry the veneration for traditional procedure to excess, and we were careful to obtain overwhelming evidence as to the facts before publishing our results. Some seventy sets of experiments were made, in which about 2000 trees were used; the soils in which the trees were planted being of every variety, and situated in eight different counties. Naturally, the results varied, but the average of them showed that ramming might be expected to increase the growth of the tree by nearly 50 per cent, during the first or first two years, at any rate in heavy or fairly heavy soils. [Slide shown.] In a light sandy soil, it naturally had no effect, for the obvious reason that, by the time the tree started into growth, any consolidation of the soil caused by the ramming would have disappeared.

Naturally, ramming a heavy soil till it becomes puddled is not good for the soil, as it prevents proper aeration, and if the whole ground were rammed the results would, doubtless, be disastrous. But it is only the ball of earth round the roots which is consolidated in this way, and the roots soon make their way out into the looser soil beyond. In one case only were the results of ramming very bad, and that was in the London clay, where the absence of aeration caused sulphuretted hydrogen to be developed. In other clay soils, no such results ensued (the Woburn farm itself is on the Oxford clay); but the adverse influence of the consolidation of the soil was generally manifested by the fact that the rammed trees lagged behind the unrammed ones during the first half, or sometimes the whole of the first season after planting, not asserting their superiority till the roots had extended beyond the ball of consolidated earth round them. [Slide shown.]

Besides producing increased growth and vigour, ramming presents the further advantage that, when adopted, trees can be planted whatever the condition of the soil may be. When a heavy soil is in a sticky condition through wet, it is impossible to plant trees in it in the ordinary way, for the lumps of earth cannot be shaken between the roots so as to secure even an imperfect contact with them; but with ramming this contact is effected even more satisfactorily when the soil is wet than when it is dry. We have planted hundreds of trees in our stiff soil at Woburn in this way with the best results, at a time when planting in the ordinary way would have been quite out of the question.

That trees will not flourish unless the soil in which they are growing is sufficiently aerated, is well known. And this is the reason why a tree, when planted, should not be buried deeply in the soil: but a latitude of a good many inches in the depth at which it is planted has been found to make no difference, because the new roots on which the life of the tree depends, form most readily at whatever depth is most suited to their development, or if they form at other depths, they soon make their way to the most favourable level.

The fact that roots flourish best at some particular level not far below the surface (the depth varying in different soils) is the explanation of some results which appeared to show the exact contrary. A number of paradise stocks were planted with their roots at various depths from 6 to 24 inches below the surface, and it was found that those which had been buried deepest, although they were the most backward at first, rapidly outstripped the others, and in two years had made twice as much growth as these had done. On lifting the trees the reason of this was apparent; in the case of those which had been planted at the ordinary depth, the root-system of the trees was the same as that which they had possessed when planted, though more developed [Slide shown], whereas with the buried trees the original roots, finding themselves at an unsuitable depth, had not developed, but in their place numerous fresh roots had developed from the stem of the tree itself, and these, finding ample food-material stored in the stem, had developed strongly, and formed a vigorous root-system, with the natural accompaniment of vigorous branch growth. Such results, depending as they do on the ability of the tree to send out new roots from its stem, would not be obtained if a grafted tree were buried in the same way, for roots do not easily arise from the stem of such a tree: indeed, when the experiment was made with crab stocks instead of paradise stocks, the results were unfavourable, for the crab stock does not root so easily from the stem as does the paradise stock. Thus, these experiments are the reverse of showing that an ordinary fruit tree should be planted at a great depth.

It will be seen that all the anomalous results which have been obtained on the subject of planting are easily explained by, and are the natural consequences of, the fact that a tree when transplanted has to form a new root-system before it can begin to grow again, and if this is but kept in view, the whole subject becomes simplified, and the reason becomes evident why many of the practices supposed to be essential to the proper planting of a tree do not bear the test of actual experiment.

[S. U. P.]

WEEKLY EVENING MEETING,

Friday, February 28, 1913.

THE RIGHT HON. LORD RAYLEIGH, O.M. M.A. D.C.L. LL.D.
D.Sc. F.R.S., in the Chair.

PROFESSOR THE HON. R. J. STRUTT, M.A. F.R.S.

Active Nitrogen.

EVERYONE has heard of ozone, the active modification of oxygen, which is produced when this gas is subjected to electric discharge. I hope to show you to-night that nitrogen can also be made to assume an active state under suitable experimental conditions. We will begin with an experiment (Fig. 1) which will serve to introduce the subject.

A rapid stream of rarefied nitrogen gas passes through the tube *a* at a pressure of a few mm. of mercury, and on its way the gas is sparked through by a series of high tension electric discharges from a Leyden jar. It then issues as a jet into the large vessel *b*, where it is seen to be brilliantly luminous, the stream of gas bein visible as a whirling cloud of brilliant yellow light. Notice that this light is of a different colour from that of the electric discharge in the former vessel.

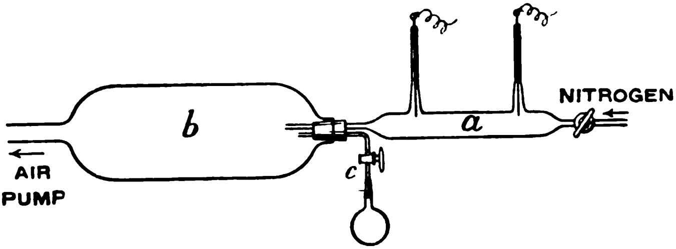

FIG. 1.

Why does the gas remain luminous in this way for an appreciable time after the electric discharge has passed through it? The view which I shall develop this evening is that the discharge has split the nitrogen molecules into single atoms. Nitrogen atoms in this condition are uneasy, and are anxious to find partners again. But to do this takes time. The reunion of the nitrogen atoms is attended with the emission of the yellow light which you see, and this continues as long as the process of pairing off is incomplete.

Preliminary to even considering this theory, we must be certain that nothing but nitrogen is necessary to the success of the experiment, and that no other substance intervenes. Some experimenters in Germany have recently expressed the opinion that traces of oxygen are concerned. I am satisfied, however, that they are entirely mistaken. The nitrogen used in the experiment you have just seen has been standing in contact with phosphorus until the phosphorus no longer glows in the dark. If I added a $\frac{1}{100000}$th part of oxygen to the nitrogen, the phosphorus would begin glowing again quite perceptibly. So we may be sure that there is not that amount of oxygen present; and I do not think it is reasonable to attribute these brilliant effects to a smaller amount. Again, we may enquire what is the effect of adding oxygen intentionally? I find that the addition of 2 per cent of oxygen is enough to obliterate the phenomena altogether. Much more might be said on the subject, but we must pass on.

It is convenient for some purposes to experiment in a different way. We have here two similar glass globes containing rarefied nitrogen. I can induce an electric discharge in them without electrodes by putting them in this coil of wire, through which a Leyden jar is constantly discharging. When I withdraw them you see that they are brilliantly luminous, and that they remain so for several minutes after stimulation. By holding them alternately in the exciting coil we can get them about equally bright, and you see that the luminosity of each decays at about the same rate. Now I stimulate them equally again, and cool one down by immersing it in liquid air. It shines brightly for a moment, but soon becomes quenched. I withdraw it, and you can compare it with the other, which is still brightly luminous.

This experiment shows that cooling the gas shortens the period of luminosity. Let me show you next that the brilliance is increased by cooling. I have exhausted this bulb to a suitable degree, and cool the neck by immersion in liquid air, contained in a transparent vessel (Fig. 2). You see how much brighter the cooled portion is after excitation than the rest of the bulb. There is no doubt a certain ambiguity in this form of experiment, because cooling a portion of the vessel causes a local concentration of the gas in that portion. I must ask you to take it from me that special experiments have proved that this cause is not enough to explain the greatly increased brightness you have seen. The reunion of nitrogen atoms occurs then more quickly the lower the temperature. This is a unique instance of a chemical action being quickened by cooling. In all other cases heating accelerates the action. Plausible objections may be made to this statement, but I must content myself now with saying that they admit of answer.

When oxygen and hydrogen unite, the union may occur in two distinct ways. It may occur with luminosity throughout the volume of the mixture, as when the gases are exploded, or, again, it may occur at the surface of a solid such as clean platinum. In the latter case there is no luminosity.

Similarly, active nitrogen atoms may reunite in the volume of the gas with luminosity—this we have seen already—or the combination may occur without luminosity at a suitable surface. Oxidized copper affords such a surface. This bulb (Fig. 3) can be made to glow like those you have seen before, by inserting it into a coil; and if the copper wire is situated in the side tube the glow lasts a long time, for the gas has as yet no access to it. But if I excite the gas again, and turn the bulb round so as to drop the oxidized wire into it, you see that the luminosity is extinguished in a fraction of a second. Combination of the nitrogen atoms occurs much more quickly at the surface, so that the whole quantity of active nitrogen

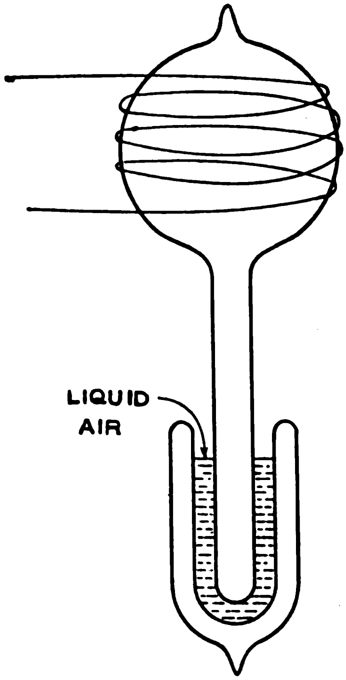

Fig. 2.

present is almost instantly used up. Incidentally, the experiment illustrates the extremely rapid diffusion of the gaseous residuum in an exhausted vessel, for every particle of the active nitrogen must evidently find its way to the surface of the wire in the fraction of a second.

We pass now to consider the effect of nitrogen in this condition on other substances. The yellow glow we have studied so far is due to the recombination of nitrogen atoms, and accordingly it shows a nitrogen spectrum, though with very curious modifications.

If we offer to the monatomic nitrogen other substances, it will often unite chemically with them, which of course cold ordinary nitrogen will not do. I go back to the apparatus used in the first experiment, and admit some acetylene by a stopcock (*c*, Fig. 1). The jet of active nitrogen now enters an atmosphere of acetylene, and you see that the character of the light is at once changed; it has become lilac. I turn off the acetylene and substitute chloroform vapour. We now get an orange light. This may appear very different, but the difference is unessential. The spectrum is in each case that characteristic of cyanogen and its compounds, only the violet portion of this spectrum is more intense with acetylene, the red portion with chloroform.

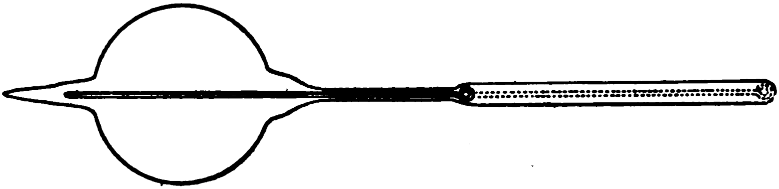

FIG. 3.

Since we get the cyanogen spectrum without having any cyanogen compound originally present, we may suspect that some such compound has been formed. Let us pass from suspicion to proof. Using chloroform vapour from a bulb containing the liquid (*see* Fig. 1), we pass the gases through a vessel in which a test-tube is inserted. This test-tube contains liquid air, and any condensable constituent is frozen out on to its external surface (Fig. 4). After a few minutes' run, we take out the test-tube and dip it in a solution of potash. I now add a mixture of ferrous and ferric salts and excess of hydrochloric acid. I pour out the liquid on to this white porcelain dish, and you see that abundance of prussian blue has been formed. This proves the presence of some cyanogen compound.

We can get the same result with pentane, ether, benzene, or almost any other organic vapour. With these the amount of cyanogen formed is much the same, but the cyanogen spectrum, curiously enough, is far less conspicuous. Benzene, for instance, almost quenches the nitrogen glow, and little can be seen of the cyanogen spectrum either. In most cases it appears that hydrocyanic acid is formed, but the orange cyanogen glow, only obtained in compounds containing much chlorine, is probably due to the formation of chloride of cyanogen in addition. This, when absorbed in potash, forms a cyanate, which has been detected chemically.

In the case just considered, the spectrum observed, when active nitrogen is mixed with another substance, is that of the product of the action. In some cases, however, the spectrum developed is that of the substance originally introduced. I admit some of the vapour of perchloride of tin: you see the brilliant blue glow. I introduce a drop of the liquid chloride on a wire loop into the flame of a Bunsen burner, and you see the same blue colour, though less advantageously. The brilliance of the luminous effect does not seem to give any trustworthy indication as to whether much chemical action is going on. If, for instance, we admit bisulphide of carbon vapour to the

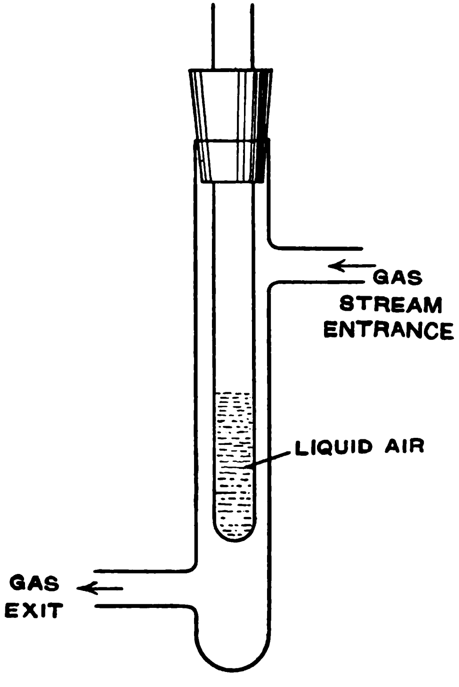

Fig. 4.

active nitrogen stream, we do not get very brilliant effects of luminosity—nothing striking enough to be worth showing you—but none the less interesting chemical actions are going on. The tube in which the action occurs gets covered with the dark blue transparent deposit, which I show by projection on the screen. This substance is a known compound of nitrogen and sulphur, originally investigated by Mr. Burt in 1910. If the gases are condensed farther on in the tube by liquid air, we get a second deposit of brown colour, which can be identified as the brown polymeric carbon monosulphide studied by Sir James Dewar and the late Dr. H. O. Jones. You see, then, that the chemical action is completely traced. Active nitrogen takes part of the sulphur from carbon disulphide, leaving carbon monosulphide.

The behaviour of active nitrogen with metallic vapours is of interest, though it has not yet been very completely studied. I select the case of mercury to show you. We pass the stream of glowing gas through this tube, which contains a small pool of mercury. While the mercury is cold, the yellow glow passes on unaffected. I apply heat, and green mercury light, of the colour familiar in the mercury vapour lamps used in electric lighting, is apparent, when active nitrogen mingles with mercury vapour. Soon the tube gets obscured, except when I am actually heating it, by a dirty looking solid deposit containing much metallic mercury.

I wish to convince you that an explosive compound of nitrogen and mercury has been formed. For this purpose, to save the trouble of dismounting the tube already used, we will take a similar one prepared beforehand. I heat the mercurial deposit moderately over a Bunsen burner, and, if you will kindly be quite still for a moment, we shall hear a distinct crackling sound, as the explosive compound decomposes. At close quarters it is easy to see flashes of light accompanying the minute explosions, but these can scarcely be shown to an audience, as the opaque deposit over the greater part of the tube obscures them.

It has only been possible this evening to bring forward a selection of the results of two years' work on this subject at the Imperial College, with generous help from colleagues, and facilities provided by the Governors.

Let me conclude by reading to you a prophetic passage from one of Faraday's letters to Schönbein :—"What of nitrogen ? Is not its apparent quiet simplicity of action all a sham ? Not a sham, indeed, but still not the only state in which it can exist. If the compounds which a body can form, show something of the state and powers it may have when isolated, then what should nitrogen be in its separate state ? You see I do not work ; I cannot. But I fancy, and stuff my letters with such fancies (not a fit return) to you."

[R. J. S.]

GENERAL MONTHLY MEETING,

Monday, March 3, 1913.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Thomas William Edward Davenall, Esq.
Philipp Moritz Deneke, Esq.
Howard Trevelyan George, Esq., M.A. M.R.C.S. L.R.C.P.
Hugh George Gillespie, Esq.
William Vaux Graham, Esq., M.Inst.C.E.
Lady Heath,
The Hon. Marguerite de Fontaine Drever Joicey,
John Arthur Law, Esq.
Rev. James Marchant,
Wyndham Anstruther Milligan, Esq., M.D. M.A. F.R.C.S.
David Williamson Moncur, Esq.
The Hon. Mrs. Reginald Parker,
Mrs. Carson Roberts,
Miss Harriet Helen Tatlock,

were elected Members of the Royal Institution.

The Honorary Secretary announced the decease on January 31 of the Right Hon. The Earl of Crawford and Balcarres, on February 14, of George Matthey, Esq., and on February 27, of Sir William Henry White; and the following Resolutions, passed by the Managers at their Meeting held this day, were read, and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution in the decease of the Right Hon. The Earl of Crawford and Balcarres, Knight of the Thistle, Fellow of the Royal Society, Fellow of the Royal Astronomical Society, Fellow of the Society of Antiquaries, Commander of the Legion of Honour.

Lord Crawford was a Member of the Royal Institution for forty-four years, and a Manager in 1897.

Lord Crawford organized expeditions to Cadiz to observe the total eclipse of the sun in 1870, and to Mauritius for the transit of Venus, which achieved important results. He established the Dun Echt Observatory in 1871–4, the magnificent equipment of which he generously handed over to the Edinburgh Royal Observatory in 1888.

He made many voyages to southern seas in his yacht, during which he obtained important collections of animals and plants which he presented to the Natural History Department of the British Museum, and to the Zoological Society.

He published in the Bibliotheca Lindesiana important lists of Tracts, Proclamations and Broadsides, and in 1910 he published a complete catalogue of the Crawford Library in four volumes.

The Managers desire to offer, on behalf of the Members of the Royal Institution, their most sincere sympathy with Lady Crawford and the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution in the decease of George Matthey, Esq., Fellow of the Royal Society, Fellow of the Chemical Society, Associate Institution of Civil Engineers, Legion of Honour, Austrian Order of Francis Joseph, Prussian Great Gold Medal for Arts and Sciences, and some time Prime Warden of the Worshipful Company of Goldsmiths.

Mr. George Matthey was one of the oldest Members of the Royal Institution, having been elected fifty-six years ago. He repeatedly occupied the position of Vice-President and Manager. His metallurgical researches are well known, and have been of great importance in the use and development of platinum. He discovered a new method of obtaining the refined metal. In conjunction with Professor St. Clair Deville, Membre de l'Institut, and a Member of the International Metric Commission at Paris, he introduced into this country the preparation of iridio-platinum by means of the oxyhydrogen furnace, and this metal was subsequently used by the French Government for their standard metre.

Mr. George Matthey always took the deepest interest in the welfare of the Royal Institution, showing his generous and practical interest in the progress of science by his frequent contributions to the research fund, and being largely instrumental in inducing the Goldsmiths' Company to grant a donation when the fund required it in the early days of Low Temperature Research. He was a frequent attendant at the Lectures and Friday Evening Meetings.

The Managers desire to express, on behalf of the Members of the Royal Institution, their deepest sympathy with Mrs. Matthey and the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Royal Institution in the decease of Sir William Henry White, K.C.B. LL.D. Sc.D., Fellow of the Royal Societies of London and Edinburgh, late President of the Institution of Civil Engineers, and of the Institution of Mechanical Engineers. He held the position of Director of Naval Construction for seventeen years.

Sir William Henry White was celebrated for many notable achievements in naval architecture, and was author of a "Manual of Naval Architecture" and a "Treatise on Shipbuilding," besides numerous Memoirs on these subjects. He was awarded the John Fritz Medal in 1911 by a special Board of the leading American Engineering Societies. He was President Elect for the British Association in 1913.

Sir William Henry White was a Member of the Royal Institution for twenty-two years, and some time a Manager. He delivered two Friday Evening Discourses at the Royal Institution, on March 9, 1894, on "The Making of the Modern Fleet," and on June 9, 1905, on "Submarine Navigation," and a course of Afternoon Lectures in 1907 on "The Contest between Guns and Armour."

The Managers desire, on behalf of the Members of the Royal Institution, to convey to Lady White and the family the expression of their deepest sympathy in their bereavement.

The following After-Easter Lecture Arrangements were announced:—

ARTHUR SMITH WOODWARD, Esq., LL.D. F.R.S., Keeper of the Geological Department, British Museum. Two Lectures on RECENT DISCOVERIES OF EARLY MAN. On *Tuesdays*, April 1, 8.

PROFESSOR WILLIAM BATESON, M.A. D.Sc. F.R.S., Fullerian Professor of Physiology. Two Lectures on THE HEREDITY OF SEX AND SOME COGNATE PROBLEMS (*continued*). On *Tuesdays*, April 15, 22.

PROFESSOR W. STIRLING, M.D. LL.D. D.Sc., University of Manchester. Three Lectures on RECENT PHYSIOLOGICAL INQUIRIES: 1. PROTECTIVE AND OTHER REFLEX ACTS; 2. EQUILIBRIUM AND THE SIXTH SENSE; 3. DUCTLESS GLANDS AND THEIR DOMINATING INFLUENCES. On *Tuesdays*, April 29, May 6, 13.

PROFESSOR T. B. WOOD, M.A., Monro Fellow of Gonville and Caius College, Cambridge. Three Lectures on RECENT ADVANCES IN THE PRODUCTION AND UTILISATION OF WHEAT IN ENGLAND. On *Tuesdays*, May 20, 27, June 3.

E. FRANKLAND ARMSTRONG, Esq., D.Sc. Ph.D. F.C.G.I. Two Lectures on 1. THE BRIDGE INTO LIFE; 2. COLOUR IN FLOWERS. On *Thursdays*, April 3, 10.

PROFESSOR JOHN GARSTANG, B.Litt. M.A. F.S.A., University of Liverpool. Three Lectures on THE PROGRESS OF HITTITE STUDIES: 1. RECENT EXPLORATIONS; 2. RELIGIOUS MONUMENTS OF ASIA MINOR; 3. CULTS OF NORTHERN SYRIA. On *Thursdays*, April 17, 24, May 1.

EDWARD ARMSTRONG, Esq., M.A. F.B.A., Queens College, Oxford. Two Lectures on FLORENTINE TRAGEDIES: 1. THE EXILE OF DANTE; 2. THE BURNING OF SAVONAROLA. On *Thursdays*, May 8, 15.

PROFESSOR WILLIAM J. POPE, M.A. LL.D. F.R.S., University of Cambridge. Three Lectures on RECENT CHEMICAL ADVANCES: 1. MOLECULAR ARCHITECTURE; 2. CHEMISTRY IN SPACE; 3. THE STRUCTURE OF CRYSTALS. (Experimentally Illustrated.) On *Thursdays*, May 22, 29, June 5.

ARTHUR M. HIND, Esq., British Museum, Author of "A Short History of Engraving," etc. Two Lectures on 1. VAN DYCK AND THE GREAT ETCHERS AND ENGRAVERS OF PORTRAIT; 2. REMBRANDT'S ETCHINGS. On *Saturdays*, April 5, 12.

PROFESSOR SIR WALTER RALEIGH, M.A., University of Oxford. Three Lectures on 1. BOCCACCIO; 2. MEDIÆVAL FRENCH NOVELISTS; 3. CHAUCER. On *Saturdays*, April 19, 26, May 3.

H. A. HUMPHREY, Esq., M.Inst.C.E. F.C.G.I. Two Lectures on HUMPHREY INTERNAL COMBUSTION PUMPS. On *Saturdays*, May 10, 17.

PROFESSOR E. RUTHERFORD, D.Sc. LL.D. F.R.S., Nobel Laureate, University of Manchester. Three Lectures on RADIOACTIVITY: 1. THE ALPHA RAYS AND THEIR CONNECTION WITH THE TRANSFORMATIONS; 2. THE ORIGIN OF THE BETA AND GAMMA RAYS AND THE CONNECTION BETWEEN THEM; 3. THE RADIO-ACTIVE STATE OF THE EARTH AND ATMOSPHERE. (The Tyndall Lectures.) On *Saturdays*, May 24, 31, June 7.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Memoirs of Department of Agriculture: Bacteriological Series, Vol. I. No. 1. 8vo. 1912.

British Museum—Index to Charters, Vol. II. 8vo. 1912.
Catalogue of Additional MSS., 1906-10. 8vo. 1912.
Catalogue of Engraved British Portraits, Vol. III. 8vo. 1912.
Catalogue of Mediæval Finger Rings. 8vo. 1912.
Catalogue of Vases, Vol. I. Part 3. 8vo. 1912.

Abbadia, Observatoire (the Director)—Procés Verbaux des Séances de l'Académie des Sciences depuis la fondation, Tome II. 4to. 1912.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXII. 1º Semestre, Fasc. 2. 8vo. 1913.

Alchemical Society—Journal, Vol. I. Part 1. 8vo. 1913.

American Geographical Society—Bulletin, Vol. XLV. No. 1. 8vo. 1913.
Antiquaries, Society of—Archæologia, Vol. LXIII. 1911-12. 4to. 1912.
Proceedings, Vol. XXIV. 8vo. 1912.
Astronomical Society, Royal—Monthly Notices, Vol. LXXIII. No. 3. 8vo. 1913.
Bankers, Institute of—Journal, Vol. XXXIV. Part 2. 8vo. 1913.
List of Members, 1913. 8vo.
Birmingham and Midland Institute—Meteorological Observations, 1912. 8vo.
Boston Public Library—Bulletin, Vol. V. No. 4. 8vo. 1912.
British Architects, Royal Institute of—Journal, Third Series, Vol. XX. Nos. 7-8. 4to. 1913.
British Astronomical Association—Journal, Vol. XXIII. No. 4. 8vo. 1913.
Cambridge Philosophical Society—Proceedings, Vol. XVII. Part 1. 8vo. 1913.
Canada, Department of Mines—Report on Building Stones. By W. A. Parks. Vol. I. 8vo. 1912.
Canada, Office of the Archivist—Catalogue of Maps, Plans and Charts in the Map Room of the Dominion Archives. 8vo. 1912.
Chemical Industry, Society of—Journal, Vol. XXXII. Nos. 3-4. 8vo. 1913.
Chemical Society—Proceedings, Vol. XXIX. Nos. 410-411. 8vo. 1913.
Journal for February, 1913. 8vo.
Cracovie Academie des Sciences—Bulletin, 1912: Classe des Sciences, A, Nos. 9-10; B, Nos. 8-10. 8vo.
Darken, E. M., Esq. (the Author)—On the Evolution of Life from Fire. 8vo. 1913.
Durning-Lawrence, Sir Edwin, Bart., LL.B. F.R.A.S. M.R.I. (the Author)—The Shakespeare Myth. 8vo. 1912.
East India Association—Journal, Vol. IV. No. 1, Jan. 1913. 8vo.
Editors—Aeronautical Journal for Jan. 1913. 8vo.
Agricultural Economist for Feb. 1913. 8vo.
American Journal of Science for Feb. 1913. 8vo.
Athenæum for Feb. 1913. 4to.
Author for Feb. 1913. 8vo.
Canada for Feb. 1913. 8vo.
Chemical News for Feb. 1913. 4to.
Chemist and Druggist for Feb. 1913. 8vo.
Church Gazette for Feb.-March, 1913. 8vo.
Concrete for Feb. 1913. 8vo.
Dyer and Calico Printer for Feb. 1913. 4to.
Electrical Engineering for Feb. 1913. 4to.
Electrical Industries for Feb. 1913. 4to.
Electrical Review for Feb. 1913. 4to.
Electrical Times for Feb. 1913. 4to.
Electricity for Feb. 1913. 8vo.
Engineer for Feb. 1913. fol.
Engineering for Feb. 1913. fol.
Gardener's Chronicle for Feb. 1913. 8vo.
Horological Journal for Feb. 1913. 8vo.
Illuminating Engineer for Feb. 1913. 8vo.
Journal of Physical Chemistry for Feb. 1913. 8vo.
Journal of the British Dental Association for Feb. 1913. 8vo.
Junior Mechanics for Feb.-March, 1913. 8vo.
Law Journal for Feb. 1913. 8vo.
London University Gazette for Feb. 1913. 4to.
Marconigraph for March 1913. 4to.
Model Engineer for Feb. 1913. 8vo.
Musical Times for Feb. 1913. 8vo.
Nature for Feb. 1913. 4to.
New Church Magazine for March 1913. 8vo.

Editors—continued.

Page's Weekly for Feb. 1913. 8vo.

Physical Review for Jan. 1913. 8vo.

Power for Feb. 1913. 8vo.

Power User for Feb. 1913. 8vo.

Science Abstracts for Jan. 1913. 8vo.

Terrestrial Magnetism for Dec. 1912. 8vo.

Zoophilist for Feb. 1913. 8vo.

Essex Archæological Society—Transactions, Vol. XIII. Part 1. 8vo. 1913.

Florence Biblioteca Nazionale—Bulletin for Feb. 1913. 8vo.

Franklin Institute—Journal, Vol. CLXXV. No. 2 8vo. 1913.

Geographical Society, Royal—Journal, Vol. XLI. No. 2. 8vo. 1913.

Geological Society—Abstracts of Proceedings, No. 935. 8vo. 1913.

Göttingen, Royal Society of Sciences—Nachrichten, 1912: Math. Phys. Klasse, Heft 7 and Beiheft; Geschäftliche Mitteilungen, Heft 2. 8vo. 1913.

Institute of Chemistry—Proceedings, 1913, Part 1. 8vo.

Junior Institution of Engineers—Journal, Vol. XXII. 8vo. 1912.

Kempe, H. R., Esq., M.Inst.C.E. M.R.I. (the Editor)—The Engineers' Year Book, 1913. 8vo.

Life-Boat Institution, Royal National—Journal for Feb. 1913. 8vo.

London County Council—Gazette for Feb. 1913. 4to.

Madrid, Reale Academia de Ciencias—Revista, Tomo XI. Nos. 1–4. 8vo. 1912.

Anuario, 1913. 16mo.

Meteorological Society, Royal—Quarterly Journal, Vol. XXXIX. No. 165. 8vo. 1913.

Monaco, Institut Océanographique—Bulletin, Nos. 253–257. 8vo. 1913.

Montpellier Académie des Sciences—Bulletin, Jan.–Feb. 1913. 8vo.

National Church League—Church Gazette for Feb.–March 1913. 8vo.

Navy League—The Navy for Feb.-March 1913. 8vo.

New York, Society for Experimental Biology—Proceedings, Vol. X. No. 2. 8vo. 1912.

North of England Institute of Mining Engineers—Transactions, Vol. LX. Part 10; Vol. LXI. Part 9; Vol. LXII. Part 8; Vol. LXIII. Parts 1–2. 8vo. 1912–13.

Report on Carboniferous Limestone. 8vo. 1912.

Numismatic Society, Royal—Numismatic Chronicle, 1912, Part 4. 8vo.

Paris, Académie des Sciences—Comptes Rendus, Tome CLIII. 4to. 1911.

Paris, Société Française de Physique—Journal de Physique for Jan. 1913. 8vo.

Pharmaceutical Society of Great Britain—Journal for Feb. 1913. 8vo.

Photographic Society, Royal—Journal, Vol. LIII. No. 2. 8vo. 1913.

Physical Society of London—Proceedings, Vol. XXV. Part 2. 8vo. 1913.

Radcliffe Library, Oxford—Catalogue of Books, 1912. 8vo. 1913.

Rio de Janeiro—Annuario de Observatorio Nacional, 1913. 8vo.

Royal College of Physicians—List of Fellows, etc., 1913. 8vo.

Royal Colonial Institute—United Empire, Vol. IV. No. 2. 8vo. 1913.

Royal Engineers' Institute—Journal, Vol. XVII. No. 3. 8vo. 1913.

Royal Irish Academy—Proceedings, Vol. XXX. B, Part 3; C, Parts 14, 15, 17–19. 8vo. 1913.

Royal Society of Arts—Journal for Feb. 1913. 8vo.

Royal Society of London—Proceedings, A, Vol. LXXXVIII. Nos. 600–601; B, Vol. LXXXVI. No. 585. 8vo. 1913.

Philosophical Transactions, A, Vol. CCXII. No. 495; B, Vol. CCIII. Nos. 299–301. 4to. 1913.

Royal Society of New South Wales—Proceedings, Vol. XLV. Part 4. 8vo. 1912.

St. Clair, Lloyd, Esq. (the Author)—A Woman of Imagination. 8vo. 1913.

St. Petersburg, Imperial Academy of Sciences—Bulletin, 1913, Nos. 2–3. 8vo.

Bulletin, 5th Series, Vol. XVII. Nos. 2–5; Vol. XVIII. XIX. XX. XXI. 8vo. 1902–4.

Memoires, Vol. XXVI. No. 3; Vol. XXX. Nos. 9–11; Vol. XXXI. No. 1. 4to. 1912.

Selborne Society—Selborne Magazine for March 1913. 8vo.

Smith, the late B. Leigh, Esq., M.R.I.—The Scottish Geographical Magazine, Vol. XXIX. No. 2. 8vo. 1913.

South Africa, Union of—Agricultural Journal for Jan. 1913. 8vo.

Statistical Society, Royal—Journal, Vol. LXXVI. Part 3. 8vo. 1913.

Tariff Reform League—Monthly Notes on Tariff Reform for Feb. 1913. 8vo.

United Service Institution, Royal—Journal for Feb. 1913. 8vo.

United States Army, Surgeon-General's Office—Index Catalogue of the Library. 2nd Series, Vol. XVII. 4to. 1912.

United States, Department of the Interior—Geological Survey: Bulletin, Nos. 514, 515, 518, 519, 520, 523. 8vo. 1912.

Professional Paper 71 and Map. 4to. 1912.

Water Supply, Papers 284, 289, 290, 291, 294, 296, 298, 304. 8vo. 1912.

United States Patent Office — Gazette, Vol. CLXXXVI. No. 4; Vol. CXXXVII. Nos. 1–3. 8vo. 1913.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1913, Heft 2. 4to.

Western Australia, Agent-General—Geological Survey Bulletins, Nos. 45, 46, 50. 8vo. 1912.

WEEKLY EVENING MEETING,

Friday, March 7, 1913.

The Right Hon. Lord Rayleigh, O.M. D.C.L. LL.D. F.R.S.,
in the Chair.

C. T. R. Wilson, Esq., M.A. F.R.S.

Photography of the Paths of Particles Ejected from Atoms.

[Abstract.]

The phenomena of radioactivity are known to be due to the ejection from the atoms of the radioactive elements of two kinds of particles which travel with enormous velocities: (1) the alpha-particle, which is a positively charged helium atom having a mass four times that of the hydrogen atom; (2) the beta-particle, which carries a negative charge only half as large as the positive charge of the alpha-particle and has a mass less than the 1700th part of the hydrogen atom.

The velocity of the fastest beta-particles approaches very nearly to that of light, that of the alpha-particles being considerably less but still exceeding 10,000 miles a second.

By the action of Röntgen and other radiations, we can cause electrons or corpuscles which are identical with the beta-particles to be expelled from the atoms of any element with velocities comparable with those with which the alpha-particles are ejected from radium.

The methods which have been used hitherto in the study of the paths of these projectiles and of the effects produced by them in their flight have been somewhat indirect. The actual paths of individual particles have not been observed; it has been necessary to investigate the combined effects of a large number of particles.

It is true it has been found possible by two different methods to detect effects arising from the action of a single alpha-particle. Thus Rutherford introduced a method in which effects due to the ions set free along the path of a single alpha-particle could be detected by an electrometer; again in the Crookes spintharoscope each alpha-particle causes a starlike point of light to flash forth momentarily where it strikes the prepared screen. But it has not been found possible by such methods to detect effects arising from a single beta-particle.

It is plain that a great advance would be made if it were possible to induce each alpha- or beta-particle to leave a visible trail behind it along its whole course and to photograph this trail. This is what is accomplished by the method now described.

Each alpha- or beta-particle, in the course of its flight through a gas like air, traverses large numbers of the atoms of the gas. According to modern theories, such as those developed by Sir J. J. Thomson and Rutherford, each atom may be regarded as a sort of miniature solar system in which the planets are represented by negatively charged corpuscles or electrons; the forces with which we are concerned being of course electrical and not gravitational. When either an alpha- or a beta-particle passes near one of the members of the system, there are forces tending to deviate the flying particle from its otherwise straight course and to cause disturbances in the path of the planetary electron; these may be violent enough to cause the electron to escape from the system. An electron thus set free will become attached finally to some other atomic system, which thus acquires a negative charge, whilst the atom which has lost an electron has been left with an excess of positive electricity. We thus get positively and negatively charged atoms or ions.

Now a method of making visible the individual ions has long been available. Molecules of water or of other vapours attach themselves more readily to ions than to uncharged atoms or molecules. Thus, in the absence of other nuclei on which vapour can condense more readily, such as those called dust particles by Aitken, it is possible to arrange that every free ion shall act as a nucleus and cause the condensation of water vapour, whilst none condenses elsewhere. Each invisible ion may thus be converted into a visible water drop. The supersaturated condition necessary in order that water vapour may condense on the ions is most conveniently produced by the sudden expansion of moist air.

The advance which I have recently succeeded in making in the condensation method of studying ionization is this. The ions are now captured and converted into visible water drops in the positions which they occupied immediately after their liberation by the ionizing agent; the cloud of drops is then at once photographed. Thus the invisible trail of ions left behind along the course of any ionising particle is converted into a visible line of cloud of which a photograph is secured. In this way a record is obtained of the path of each projectile by making visible the atomic wreckage it has caused in its passage through the air or other gas. In many cases the individual ions produced along the tracks are visible in the photographs.

In order that undistorted pictures showing the result of the passage of the various rays may be obtained, it is essential that the expansion should be effected without stirring up the gas. This condition is secured by using a wide shallow cloud chamber of which the floor can be made to drop suddenly and so produce the desired increase of volume (Fig. 1).*

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* The apparatus is described in the Proceedings of the Royal Society, A., vol. 87 (1912), p. 277.

It is hardly necessary to say that the cloud chamber must be freed from dust particles and all nuclei on which water readily condenses. This is easily done by repeated expansions, each too small to cause condensation on the ions, any cloud formed being always allowed to settle before making another expansion.

The cloud chamber must be free from ions other than those produced by the ionizing agent under investigation. Since ions are always being produced even under normal conditions within a closed vessel, it is necessary to maintain an electric field between the top and bottom of the cloud chamber, so that they may be removed as fast as they are produced.

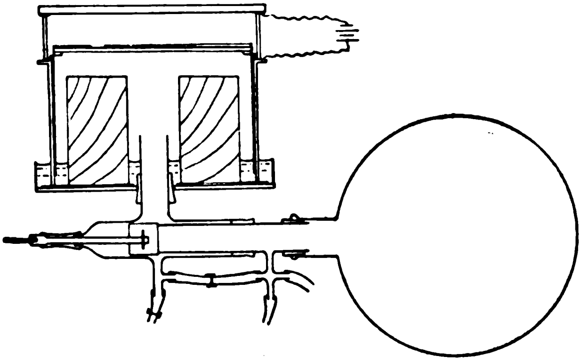

FIG. 1.

One very practical point in connexion with the cloud chamber remains to be mentioned. It is necessary that the interior should be maintained in a nearly saturated condition and yet that the roof and walls should be transparent and admit of a clear and undistorted view of the contents. A glass vessel containing moist air soon becomes coated internally with a dew-like deposit of minute drops. This difficulty is completely avoided by covering the inner surface of the glass with a film of gelatine.

The moist gelatine under the plate-glass roof of the cloud chamber forms a conducting film which is connected through a marginal ring of tinfoil with one terminal of a battery of cells, the other terminal being connected to the floor. In this way, a nearly uniform vertical electric field is maintained between the roof and floor of the chamber. The floor is virtually a pool of water made solid by the addition of gelatine and blackened by means of ink so that it forms a dark background for the clouds. It is supported by a glass plate which forms the top of a hollow cylindrical plunger working in water.

As regards the actual mechanism for causing the sudden drop of the floor of the cloud chamber, it is sufficient to state that the space below the plunger can be put in communication, through wide tubes, with an exhausted chamber by suddenly opening a valve.

In order that the ionizing particle should leave sharply defined cloud trails, it is necessary that they should traverse the moist gas immediately after this has been expanded while the water vapour is still supersaturated to an extent considerably exceeding the minimum which is required to cause condensation on the positive ions (which are more difficult to catch than the negative). Under these conditions, the ions lose their mobility and grow into visible drops before they have had time to diffuse appreciably away from the original track of the ionizing particle.

If the clouds formed by condensation on the ions are to be photographed, it is necessary to expose them to an instantaneous illumination of great intensity while the camera is in position. The instantaneous illumination is obtained by a Leyden jar discharge, the arrangement being essentially the same as that used by Lord Rayleigh in photographing jets of water and by Worthington in his study of the splash of a drop.

I have, however, allowed the spark to traverse mercury vapour at atmospheric pressure instead of air, the brightness being thereby greatly increased.

The spark, of course, has to be suitably timed, so that the cloud trails may be illuminated after the drops composing them have grown sufficiently to scatter plenty of light but before there has been any appreciable disturbance of the air by convection currents.

Figs. 2 to 13 are pictures obtained by this method. It is perhaps necessary to point out they are all photographs of clouds consisting of minute water drops condensed upon ions, for many of them have a very uncloudlike appearance.

Fig. 2 is a photograph of the tracks of some alpha-particles shot out from a minute quantity of radium placed within the cloud chamber, the camera looking down through the plate-glass roof. From the atoms of radium, alpha-particles are continually being projected with velocities of many thousands of miles per second, each producing more than 100,000 ions in the course of its flight. Under ordinary conditions the trail of ions left behind by each particle is invisible ; those formed by particles which have traversed the supersaturated air of the cloud chamber immediately after its expansion, however, are at once converted into visible cloud trails. These form the sharply defined spokes or rays of the picture. The more diffuse cloud rays are the tracks of particles which have traversed the air before its expansion, the ions having thus had time to wander out of the original track before losing their mobility through the condensation of water upon them. The electric field maintained in the cloud chamber fixes a limit to the age and hence to the diffuseness of the trails which are rendered visible; under the actual conditions any free ions would be driven by the electric force to the roof or floor within less than a fifth of a second after being set free. None of the ions made visible has had a free existence exceeding this limit.

It is clear that an ionizing particle, while traversing or even passing near to an older trail of ions on which a cloud has already formed, will not find the vapour supersaturated to the extent necessary to cause condensation on the ions; it will therefore fail to leave a visible trail in this region. This is doubtless the reason why the sharply defined trails only appear to begin at some distance from the source, the older trails being most closely packed in the region around the source

By means of a suitable shutter arrangement attached to the floor of the cloud chamber, it is possible to prevent alpha-particles from traversing the moist air till after the expansion. The diffuse cloud trails are then absent from the photographs (Fig. 3).

The most remarkable feature of the tracks of the alpha-particles is their general straightness. Sudden bends are to be observed, however, practically all the rays being bent within a millimetre or two of their ends. In this respect, as in others, the photographs confirm the conclusions arrived at by less direct methods.

In the next picture (Fig. 4) an enlargement of two of the tracks is shown, one of them having two sudden bends. The path is otherwise straight except very near its end. Now the alpha-particle has thousands of encounters with atoms of the gases of the air in each millimetre of its course by which ionization is brought about, as we know from measurements made by the electrical method; and in accordance with this, the cloud particles (which are simply ions magnified by condensation of water) are so closely packed that they are not separately visible in the photograph. It is remarkable that only two encounters out of the many thousands occurring in the course of its flight should succeed in deviating the particle visibly from its course and that in these cases the deviation should be quite large.

The alpha-particle, in passing near one of the electrons of an atom, may impart to it sufficient energy to cause it to escape from the atom, whilst on account of its own enormous momentum it is not perceptibly deviated from its course. We can thus understand the general straightness of the tracks. The sudden deviations must be due to encounters of a special kind; according to Rutherford's view, such large deviations would be caused by the alpha-particle passing near the centre of the atom, where he supposes the positive charge to be concentrated.

What is perhaps the most interesting feature of the particular track I have been describing remains to be mentioned. At the

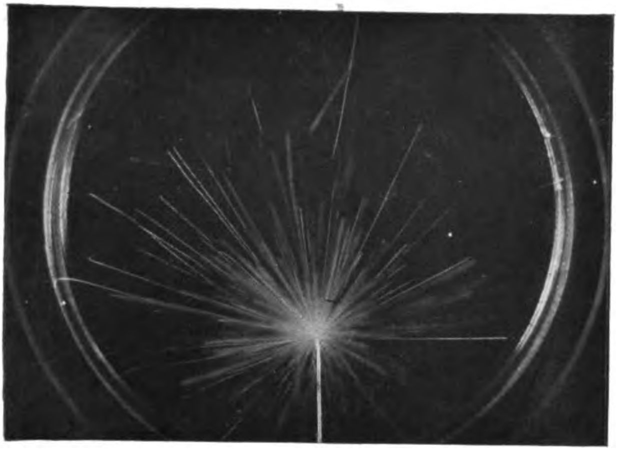

Fig. 2.

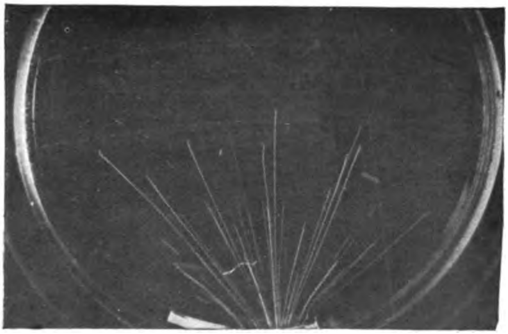

Fig. 3.

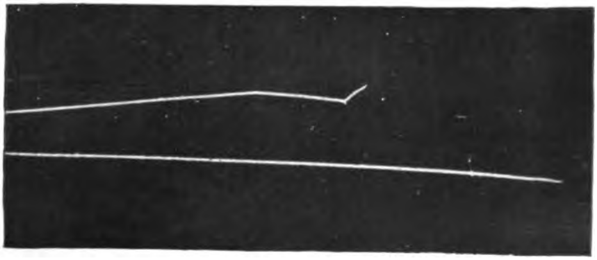

Fig. 4.

second of the two bends, there is a distinct spur which one can hardly interpret otherwise than as being due to the recoil of the system which has caused the deviation of the particle.

The next two photographs (Figs. 5, 6) show the effect produced in the cloud chamber by a trace of radium emanation—the radioactive gas which is the first product of the disintegration of radium. Each cloud ray is a visible record of the conversion, by expulsion of an alpha-particle, of a single atom of the emanation into an atom of the next member of the radioactive series. Since the rays start in the gas, it is now possible to get tracks which are complete from beginning to end. The ends are distinguishable by the characteristic bend or hook. At the beginning there is an enlarged head where, moreover, the cloud is of greater density; this represents ionization by the recoil of the atom from which the alpha-particle has escaped.

It may be noticed there is a sudden bend in one of the rays with which there is again associated a spur-like process.

Radioactive substances emit beta-particles as well as alpha-particles. These produce comparatively few ions along their tracks, which are thus much less conspicuous when converted into visible cloud rays than those of the alpha-particles. They are, in consequence, more difficult to photograph, and they have not appeared in any of the pictures shown thus far.

With suitable illumination, however, the droplets condensed on the individual ions may be photographed, provided they are not too closely packed. It is thus possible to study the path of any ionizing particle, however small the number of ions produced.

On account of the enormous velocities with which they are emitted—closely approaching that of light—the beta-particles are able to travel considerable distances in the air, distances many times greater than the diameter of the cloud chamber. It is therefore impossible to obtain a picture of the whole track of a single beta-particle.

Here, on one plate (Fig. 7) are shown the final portions of the tracks of an alpha- and of a beta-particle. The beta-ray shows much less intense ionization, as indicated by the comparative densities of the clouds; and its devious path forms a great contrast to the straightness of the alpha-ray.

The beta-particle, of course, is so much more readily diverted from its course on account of its much smaller mass.

If, however, we catch the beta-particle at a sufficiently early stage of its career, we find that its immense velocity compensates for its very small mass and its path may be sensibly straight for distances of several centimetres, in spite of the very large number of atoms which it must traverse. This is illustrated by the next picture (Fig. 8) which shows, in addition to the end of a beta-ray, a portion of the trail left by a beta-particle while its velocity was still very high; it is noticeable that it is practically straight. Another result of the high velocity is that very few ions have been set free along this path; for the faster the particle traverses an atom the shorter is the time during which the forces can act. The individual ions are readily distinguishable in the photograph; the droplets appear mainly in pairs (each representing a positive and negative ion) but there are, in addition, here and there closely packed groups of twenty or thirty.

In addition to the alpha- and beta-particles, radioactive bodies emit an extremely penetrating type of ionising rays—the gamma-rays—having properties similar to those of Röntgen rays. If we expose the cloud chamber to this radiation (cutting out the alpha- and beta-rays by a lead screen), we see on expansion extremely fine threads of cloud crossing the vessel in all directions. These are the tracks of beta-particles emitted mainly from the walls of the vessel under the influence of the gamma-rays. The whole of the ionization produced by gamma-rays appears to be, as it were, secondary and due to the beta-rays.

The remaining pictures illustrate some of the properties of Röntgen rays.

In studying the nature of the process of the ionization of air by X-rays by means of the expansion apparatus, it is convenient to use an instantaneous flash of the rays produced by sending a single Leyden jar discharge through the Crookes tube. The discharge is so timed that the rays pass through the cloud chamber immediately after the expansion of the air, so that they traverse it while it is supersaturated with water vapour. The ions produced are thus at once fixed by the condensation of water vapour upon them before any appreciable diffusion has occurred; the illuminating spark is timed to pass a fraction of a second later and so give an instantaneous photograph of the clouds condensed on the ions.

Fig. 9 is a photograph showing the effect of such a flash of X-rays—the radiation being confined to a narrow cylindrical beam by lead screens provided with apertures. The photograph was obtained with the camera pointed horizontally through the cloud chamber in a direction at right angles to the beam of X-rays.

In the light of knowledge furnished by other methods, we may interpret the picture in the following way. Under the influence of the X-rays, an atom here and there in the path of the cylindrical beam of X-rays has emitted a corpuscle or beta-particle with sufficient velocity to enable it to traverse several millimetres or even centimetres of air, ions being set free along its path. It is the paths of these beta-particles or cathode-rays which are made visible in the photograph. The X-rays do not appear to produce any ionization other than that effected through the agency of the beta-rays excited by them, as indeed Prof. Bragg has long maintained.

The only room for difference—apart from their mode of origin—

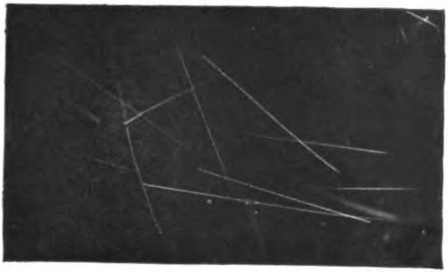

FIG. 5.

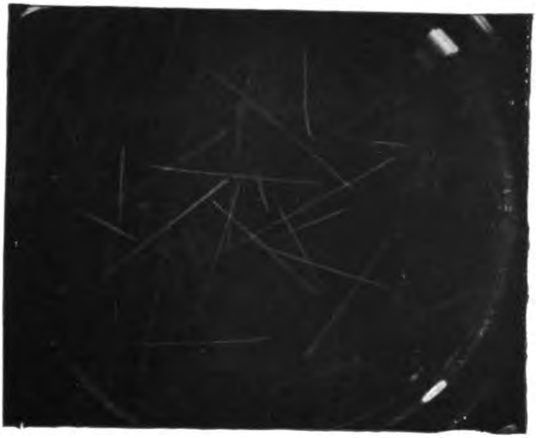

FIG. 6.

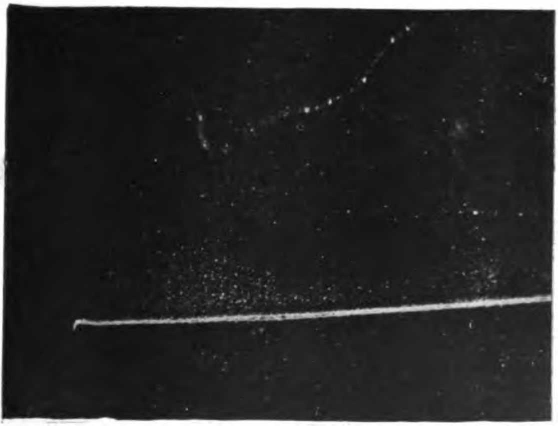

FIG. 7.

FIG. 8.

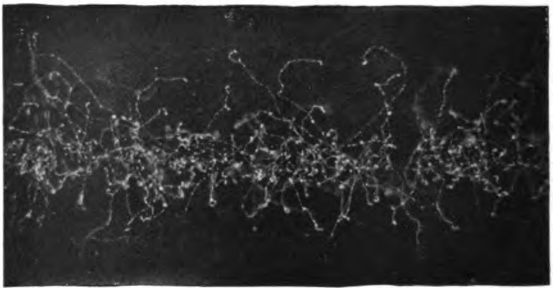

FIG. 9.

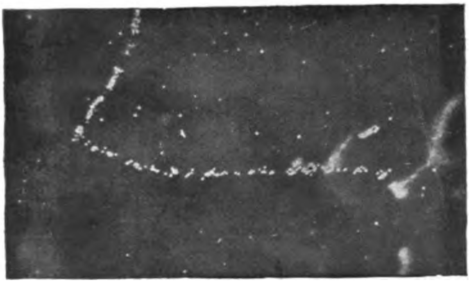

FIG. 10.

between the beta-rays produced by the action of X-rays and those emitted spontaneously by the radioactive substances lies in their initial velocity; for there is no lack of evidence that all negatively charged corpuscles are alike, except in so far as their properties are affected by their velocity. And in fact, the tracks of the beta-particles or cathode-rays excited in air by X-rays are indistinguishable from the end portions of beta-ray tracks, such as are shown in Figs. 7 and 8.

The tracks are far from straight, and as the particle approaches the end of its course the deviation becomes generally more and more marked, the particle being more easily deflected the smaller its velocity.

The departure from straightness is mainly of the nature of a general curvature due to an accumulation of inappreciable deflections at successive encounters; sudden deviations through large angles, the result of single encounters of a more effective kind, also appear occasionally.

The number of ions produced per centimetre is known to increase rapidly as the velocity of the cathode-ray particle diminishes. This is shown by the increased density of the clouds towards the ends of the tracks.

Fig. 10 is an enlargement of a portion of the track of a beta-particle emitted in air exposed to X-rays. The individual ions are clearly visible and may readily be counted; the number per centimetre amounts to about 188 pairs, when reduced to atmospheric pressure.

In taking the photograph shown in Fig. 11 the X-rays were made to traverse the air before instead of after the expansion. The ions liberated along the track of each cathode-ray were thus free to move under the action of the vertical electric force maintained in the cloud chamber, the positive travelling downwards, the negative upwards. Each trail was thus divided into two portions, one consisting of negative, the other of positive ions, before being converted into visible cloudlets by expansion of the moist air; the ions of each trail have also had time to be considerably scattered by diffusion.

The representations of X-ray clouds shown thus far have all been from photographs taken with the camera pointed horizontally and so placed that a magnified image was obtained. The remaining photographs were obtained with the camera pointed vertically downwards, the conditions being such that the whole visible contents of a horizontal stratum of the cloud chamber, about 2 cm. in thickness, were photographed just as in the case of the alpha-ray pictures. Very intense illumination is required to make the cathode-ray tracks visible in a picture taken in this way; and it is only recently that I have succeeded in photographing them.

A thin sheet of copper was fixed in the centre of the cloud chamber in the path of a narrow beam of X-rays, which was made to traverse the supersaturated air of the cloud chamber immediately after its expansion.

The absorption of X-rays by the copper is evident at a glance (Fig. 12) from the difference of the density of the clouds condensed on the incident and transmitted beams.

In passing through the copper the X-rays produce immense numbers of cathode-rays which form dense clouds immediately in front of and behind the copper plate. The clouds are not quite in contact with the copper, the clear space next the plate being due to the air becoming warmed by contact with the copper before the passage of the rays, so that the ions fail to find the supersaturation necessary for their growth into water drops.

From the researches of Barkla and others we know that when exposed to X-rays the copper plate will emit secondary rays—the homogeneous or characteristic or fluorescent rays of copper. These will in turn cause the air to emit secondary cathode or beta-rays The visible cloud trails left by these are seen in the photograph (Fig. 12). A photograph of this kind shows at once the distribution of the secondary radiation from a substance as well as the nature of the cathode-rays produced by this radiation in the surrounding gas. The cathode- or beta-rays produced in air by the copper-rays are all much alike in length (about 1 mm.) ; this is in striking contrast to the very varying length, ranging up to 2 or 3 cm., of those produced by the primary X-rays.

A photograph taken under similar circumstances with a silver plate in place of the copper one shows similar effects, but the cathode-rays produced in air by the silver-rays are many times as long.

Some photographs were also taken with X-rays incident upon the copper plate after their intensity had been reduced by interposing a considerable thickness of aluminium. This cuts out especially the less penetrating radiation. The individual cathode-rays which start from the copper are now readily seen (Fig. 13) ; they were before too closely interlaced to be separately visible. The surprising feature of this photograph is the great length of some of the cathode-rays emitted by both copper and air exposed to the X-rays. Some of the tracks are about 3 cm. in length when the air is at atmospheric pressure.

[C. T. R. W.]

FIG. 11.

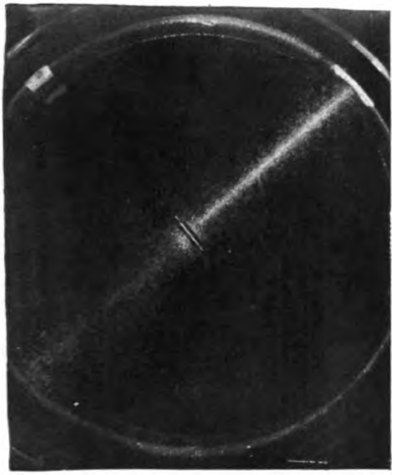

FIG. 12.

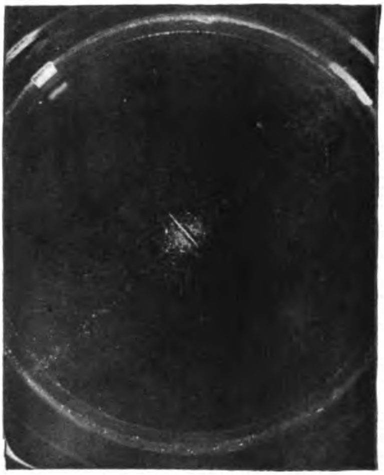

FIG. 13.

WEEKLY EVENING MEETING,

Friday, March 14, 1913.

DONALD W. C. HOOD, Esq., C.V.O. M.D. F.R.C.P., Vice-President, in the Chair.

A. E. H. TUTTON, Esq., D.Sc. M.A. F.R.S.

Great Advance in Crystallography.

CRYSTALLOGRAPHY has made such remarkable progress during the last few months, and the position at the present moment is so interesting, that it was considered opportune to review it in a Discourse from this historic lecture-table. For, firstly, the descriptions of the crystals of all the ten thousand substances which have ever been subjected to goniometrical measurement have been collected together and classified within the four volumes of a monumental work by Professor von Groth, of Munich. Secondly, this immense labour has been paralleled by the construction, by Professor von Fedorow, of St. Petersburg, of a tabular record of the main crystal elements of all these substances, arranged in a simplified form, and with the assurance, which has entailed untold labour to achieve, that they relate to a truly comparative orientation; so that this table is the index to and basis of a new method of "Crystallo-chemical analysis," which enables a trained investigator to identify any well-crystallized substance from the result of a brief goniometrical examination. And, thirdly, the whole of these invaluable results have been placed on a firm experimental basis; for the internal structure of crystals, as imagined in all its wonderful details by the greatest geometrical and mathematical minds amongst us, has been revealed on the photographic plate as the result of direct experiment with the excessively minute and all-penetrating wave-motion, or corpuscular energy, of the X-rays.

It is easy to prove that a crystal has an organized structure. The fact is at once revealed by the influence of rapidity or slowness of growth on its character. For example, if a little benzoic acid be melted on a glass plate over a spirit lamp and the plate allowed to cool rapidly in the air, on the object-stage of the projection polariscope, using crossed Nicols, the dark field is almost immediately illuminated by crystals beginning to grow at the margin of the liquid film, and from each bright spot a crystal-needle darts, with fiery tip, like a lightning flash, into the centre of the field, until the whole picture is an interlacing mass of acicular crystals brilliantly coloured in the polarized light, which renders them more visible. Again, if we crystallize a substance from solution, say potassium bichromate, it entirely depends on whether the degree of supersaturation is slight, the "metastable" condition of Miers and Ostwald, or excessive, the "labile" condition, as to what kind of crystal we obtain. From the metastable solution perfect little single crystals are started into slow growth, by the advent of germ-crystals of the same or an isomorphous substance from the air; while the labile solution spontaneously and rapidly crystallizes in the beautiful feathery forms illustrated on the screen. Ammonium chloride (Fig. 1) and metallic silver afford us even more beautiful screen pictures of arborescent crystallizations, and nothing can exceed the beauty of snow crystals, an example of rapid crystallization of water-vapour. For goniometry these labile forms are useless, but, nevertheless, they teach us much concerning the structure of crystals. For in them the skeleton, or inner framework and plan of architecture of the crystal is revealed.

It is hard to realize the clearly proved fact that our atmosphere teems with excessively minute crystals—for they possess the complete organization of a crystal—often not exceeding the one thousand millionth of a milligramme in weight, and capable, whenever they fall into a quiescent slightly super-saturated solution of a crystalline substance of like structure, of calling forth its power of crystallizing. In order to be able to exercise this remarkable power, however, the germ crystal must be isostructural in a very strict sense, if not identical with the substance which is set crystallizing. Not only must its symmetry be similar, but the dimensions of its structural units—the "bricks" of the crystal edifice, so to speak—must be all but identical. The conditions are, indeed, similar to those required for the facile formation of parallel growths of one crystallized substance on another, so admirably investigated by Barker.

Perhaps the most striking cases for the purpose of illustration are those of the rhombic alkaline sulphates, selenates, perchlorates, chromates, or other of the well crystallized salts of the alkali metals potassium, rubidium, and cæsium, and of the base ammonium which is so extraordinarily capable of replacing them. In any such group of salts the periodic law of Newlands and Mendeléeff is most beautifully illustrated by the regular progression of all the properties of the crystals of the three metallic salts, corresponding to the progression in the atomic weights of the metals, the salt of rubidium, the metal of intermediate atomic weight, having invariably intermediate properties, both morphological and optical. A slide showing the gradual slight change in the prism angle of the crystals of the sulphates will make the point clear, the slope of the prism face of rubidium sulphate being intermediate between the greater slope of that of the potassium salt and the lesser slope of that of the cæsium salt. The variation of the position of the optical ellipsoid in the three monoclinic double sulphates of the $6H_2O$ series containing these three salts is also illustrated by a slide in which the ellipsoid can be rotated through the three

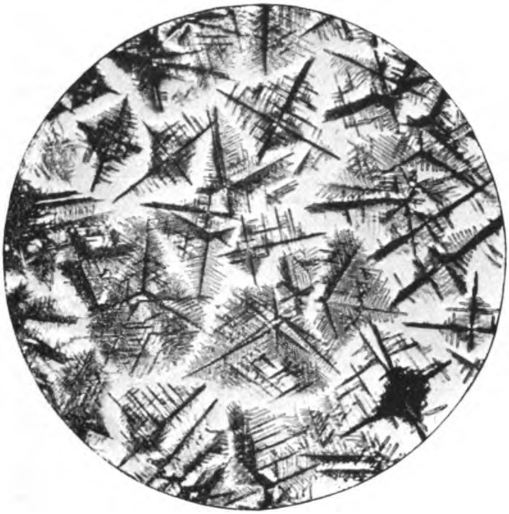

Fig. 1.—Crystallization of Ammonium Chloride.

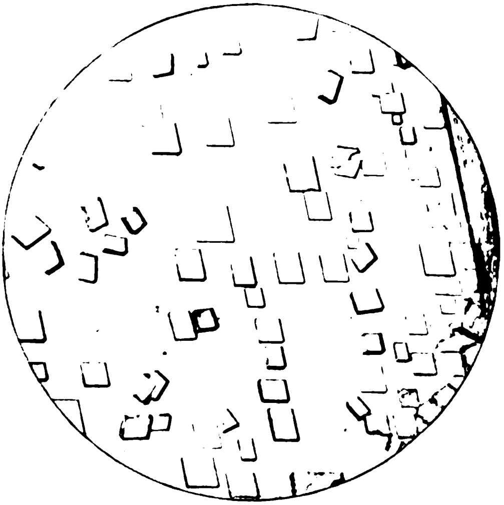

Fig. 3.—Parallel Growths of Sodium Nitrate on Calcite.

positions (the rotation being also indicated in Fig. 2 by the dotted ellipses). In a similar manner the dimensions of the structural units—the molecular volume (that of the "brick" regarded as a molecule) and its expression in the three dimensions of space (which we now have a means of determining, by combining the density and the crystallographic axial ratios, and which are known as topic axial ratios)—vary in regular progression as functions of the atomic weight. Hence, the structural dimensions of the two extreme members of any of these groups of salts, the potassium and the cæsium salts, are most divergent, and Barker has shown that while the rubidium salt will in general form parallel growths with either the potassium or the cæsium salt, the potassium and cæsium salts themselves will never form satisfactory parallel growths on each other, clearly owing to the disparity in the dimensions of their structural units. Further, the ammonium salt of any group has the interesting property of forming crystals of which the molecular volumes and topic axial dimensions are almost identical with those of the rubidium salt of the same group. Now it is most important and conclusive that the ammonium and rubidium salts form the best of all parallel growths; they are, indeed, no longer merely parallel growths, but zonal growths and complete overgrowths. They also form excellent mixed crystals, and in every way which has yet been experimentally tested they show the nearest approach to true iso-structure. Some photographs of such parallel growths of rubidium and ammonium salts will render the matter clear. There is also one case of iso-structure investigated by Barker, that of calcite and sodium nitrate, which is of particular interest; for these are not chemically iso-morphous substances, but they happen to have similar rhombohedral symmetry and almost identical molecular volumes and topic axial dimensions. The slide of parallel growths of sodium nitrate on calcite (reproduced in Fig. 3, Plate I, by the kindness of Mr. Barker) is very striking and conclusive. Barker has recently suggested the nature of the structure of these two substances, two molecules forming the unit "brick," as represented in the slide exhibited. In the case of potassium sulphate and its analogues, one molecule forms the "brick," as represented by the next slide.

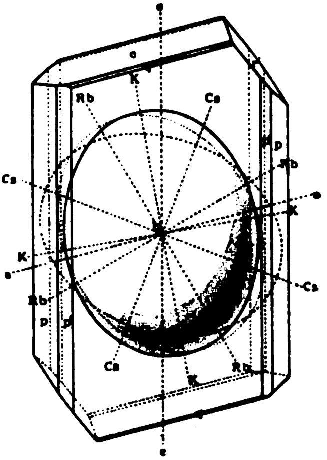

Fig. 2.—Rotation of the Optical Ellipsoid of the Double Sulphates on replacing Potassium by Rubidium or Cæsium.

It has thus been proved experimentally from the morphological side that a crystal has a definite structure, and that its unit "bricks" or "cells" have definite and measurable dimensions. But the fact is equally well demonstrated optically. We have only to pass a beam of light through a 60° prism of a non-cubic crystal, for instance, quartz, to see at once the radical difference of effect from that given by glass or other non-crystallized substance. For instead of the usual single spectrum produced by glass the quartz prism refracts two distinct spectra, unless it happens to have been cut so that the light traverses the unique direction of single refraction, coincident with the axis of the natural quartz crystallographic prism, which is also the axis of trigonal (threefold) symmetry. The quartz prism used in the experiment is cut at right angles to this direction, so that the axis and refracting edge of the cut prism are parallel to the natural axis, and the separation of the two spectra, corresponding to the two refractive indices of quartz, is thus at a maximum. Moreover, on placing a Nicol prism in the path of the refracted rays, we observe that the light producing the two spectra is oppositely polarized, one spectrum extinguishing when the Nicol has its vibration plane vertical, and the other when the Nicol is rotated so that the vibration plane is brought into the horizontal position. The crystal thus possesses a structure, which is capable of separating a beam of ordinary light into two beams, having definite and perpendicularly different vibration directions.

Again, we see proof of structure if we cut a plate out of the crystal and examine it in a converging beam of polarized light, especially if the crystal, say one of calcite, be cut perpendicularly to the singular axis (or to the bisectrix of the two such axes in the cases of biaxial crystals) of single refraction. A beautiful interference figure is produced, composed of spectrum-coloured rings and a black cross, that is, a figure symmetrical about the axis of trigonal symmetry and of single refraction. This evidence of structure can be most wonderfully reproduced by glass, if we strain the glass by heating and rapid cooling about a cylindrical axis, but an ordinary unstrained piece of glass affords no such effect at all. It is clear, therefore, that the calcite crystal has a symmetrical structure about the axis of single refraction. Similarly, the beautiful biaxial interference figure exhibited on the screen, afforded by a plate of rhombic potassium nitrate, is symmetrical about the centre of the double-looped figure of spectrum-coloured lemniscates.

Sufficient evidence will now have been brought forward that a crystal is endowed with a definitely organized structure. In the crystal of a pure substance we are dealing with a chemical element or compound, and if with the latter it may be of any grade of complexity, from a very simple binary compound to a most highly complicated one composed of a large number of atoms. If the crystal be that of an element the structure is obviously composed of the similar atoms of that element, while if it be a compound we have a structure composed of atoms of as many kinds as there are chemical elements present combined in the substance, and in the same relative proportion as is expressed by the chemical formula of the substance. In the case of a compound, moreover, the structure may also be considered to be that of the molecules of the substance, for they or a simple arrangement of a small number (group) of them form the grosser units of the structure, whilst the atoms are the ultimate units.

Suppose we now represent this molecular or polymolecular grosser structural unit by a point, and that such point be analogously situated within each unit. The essence of crystal structure then is that these points are so arranged in space that if they are joined by imaginary lines the latter form a "space-lattice" (German, "Raumgitter"), each unit cell of which may be conceived to be the "brick" already alluded to, and the domicile of the chemical molecule or group of molecules (indeed, it is immaterial whether the points are considered as placed at the corners or in the centres of the cells) or, in the case of an elementary substance, of a group of similar atoms. We may, therefore, define a crystal as follows:—

"A crystal of any definite chemical substance consists of a homogeneous arrangement of grosser units of matter, each consisting of one chemical molecule or a small group of molecules of the substance, and the kind of arrangement is such that these grosser units are all identically (sameways, parallelwise) orientated, and that their analogously chosen representative points, one from each such grosser unit, form a space-lattice (Raumgitter)."

There are fourteen kinds of space-lattices, slides of several of which are exhibited on the screen. Three possess full cubic symmetry, two are tetragonal, four are endowed with rhombic symmetry, and two are monoclinic; while triclinic, trigonal, and hexagonal crystals have each one space-lattice corresponding to their type of symmetry. In every case it is the full (holohedral) symmetry of the system which is present, no space-lattice possessing merely the lower degree of symmetry corresponding to one of the so-called hemihedral or tetartohedral classes of the system in question.

Now in the solid crystal, not only are the grosser units arranged so that their representative points are repeated in space with extraordinary accuracy of position, with production of unit cells or "bricks" of absolutely identical dimensions throughout the crystal, but the shapes of the grosser units themselves are identically similar and identically similarly orientated in space. Suppose, however, that the force of crystallization, the directive molecular force concerned in bringing the molecules together in this regular order of marshalling, is only adequate just to attain this marshalling of the grosser units into a space-lattice formation, without being able to fix the units about their own centres of gravity, a certain amount of wobbling about the latter being still permitted. We might, in such circumstances, expect that some of the properties of a crystal, dependent on the space-lattice formation on lines of definite symmetry, such as the optical property of double refraction and polarization of light, would be developed and exhibited, while the production of exterior plane faces would be either only partial, with rounded edges and the exhibition of plasticity and viscosity, or would not be achieved at all, the objects produced being still fluid. One cause of such a condition of partial success at crystallization might well be that the substance was composed of a large number of atoms arranged in a long chain, such as the well known "long chain compounds" of organic chemistry, which would offer considerable resistance to marshalling. The author believes that herein lies the explanation of the remarkable "liquid crystals" which Professor Lehmann has made the subject of his particular study, many of which are of just such long-chain character.

By the kindness of Professor Lehmann, who has sent over specimens of some of the most characteristic of his substances for the special purpose of this lecture, and of Mr. Poser, of Messrs. Zeiss, who construct an admirably convenient form of heating microscope and projection arrangement for demonstrating the formation of liquid crystals and their behaviour in polarized light, it is possible to exhibit some of the typical phenomena of these interesting objects on the screen. The substances in question are chiefly such as form two or more polymorphous forms, each stable within a limited range of temperature, and the liquid crystals are usually the second phase observed on allowing the truly liquid heated substance to cool; the liquid crystal phase is produced at a definite temperature during the cooling, and persists during a definite interval of temperature during the continued cooling. It either exhibits distinct attempts at the formation of pyramidal or prismatic crystals, more or less rounded at their edges, as in the case of ammonium oleate shown on the screen (Fig. 4), or manifests itself as doubly refractive and brilliantly polarizing drops or streams, such as the drops of para-azoxyanisol, which are set rotating independently by the addition of a little colophonium, as demonstrated in brilliant polarization colours on the screen; it then suddenly passes with further cooling into the final solid phase, often with the production of brilliantly coloured acicular crystals, as in the case of para-azoxyphenetol exhibited on the screen, or of beautiful star-like or flower-like apparitions, radiating from innumerable centres all over the field, as in the exceedingly beautiful case of cholesteryl acetate. The view here put forth is apparently in agreement with that of Lehmann himself, as most recently expressed both in letters to the lecturer and in a memoir of July 27, 1912, to the Heidelberg Akademie der Wissenschaften, in which he says that in all probability: "Die Rundung der Formen hänge zusammen mit der Plastizität der Stoffe und habe ihren Grund in unzureichender molekularer Richtkraft, welche wohl genügt, ein Raumgitter herzustellen, nicht aber regelmässige Treppenstufen, wie

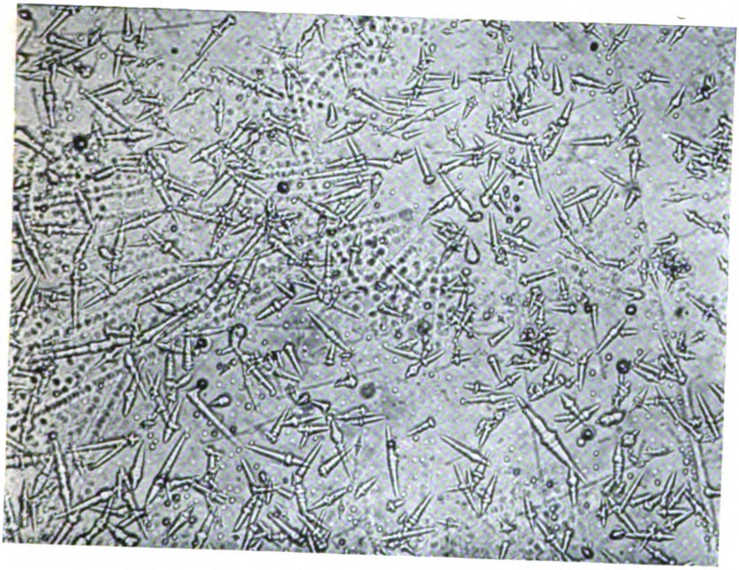

FIG. 4.—LIQUID CRYSTALS OF AMMONIUM OLEATE.

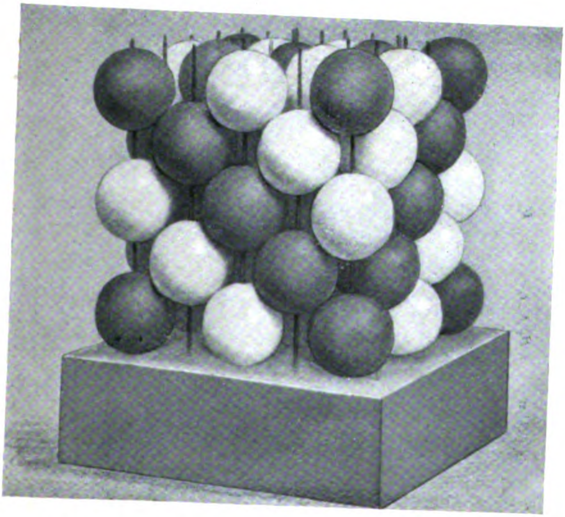

FIG. 8.—MODEL OF ARRANGEMENT OF ZINC ATOMS (SHADED) AND SULPHUR ATOMS (LIGHT) IN ZINC BLENDE.

es nach Hauys Theorie zur Bildung ebener Krystallflächen nötig wäre." The formation of regular stepped faces (of invisibly minute steps, "Treppenstufen") the lecturer considers to occur only when the grosser units become fixed about their centres of gravity or representative points, with production of a truly solid crystal.

But now let us pass to the consideration of the internal structure of the grosser or space-lattice units themselves. Their symmetry may be, in simple cases, similar to that of the space-lattice, but in general this will not be so. Whatever the stereometric arrangement of the chemical atoms in the molecule may be, and, if more than one molecule goes to form the space-lattice unit, whatever their mutual arrangement, and therefore, whatever be the outer configuration of the whole unit, when the crystal is a truly solid one, the force of crystallization (now no longer denied) is adequate to fix each space-lattice unit, not only considered as a point with reference to its neighbours, but as regards its shape and its whole character, parallelwise and sameways orientated with respect to its adjacent fellows, and as close as possible to them. Also if more than one molecule goes to each space-lattice unit, their mutual arrangement is achieved on a definite plan, and is the same for every space-lattice unit; these constituent molecules of the latter are also as closely packed as possible. The final result is thus to produce an assemblage of chemical atoms, in which not only the demarcation frontier between the space-lattice units disappears, but also that between the constituent molecules in the cases of polymolecular grosser units. We come, ultimately, in consequence, to a structure of atoms, each of which we may represent by a point.

Now, just as the genius of Frankenheim and Bravais revealed to us the 14 kinds of space-lattices, so Sohncke made us acquainted with 65 regular systems of points, including many of the 32 classes of symmetry, but not all, which von Lang had shown crystals to be capable of possessing. Later the number was brought up to 230 by simultaneous and wonderfully concordant geometrical researches by Schönflies in Germany, von Fedorow in St. Petersburg, and Barlow in England, and among these 230 all the 32 crystal classes are represented, and no others.

Hence, we come to the conclusion that the skeletal framework of crystal structure is the molecular or polymolecular space-lattice, and the detailed ultimate structure the atomic point-system. The latter determines the class of symmetry (which of the 32 classes is exhibited) and therefore governs any hemihedrism or tetartohedrism, as the development of less than full systematic symmetry used to be called. But it is the space-lattice which governs the crystal system, that is, which determines whether the symmetry is cubic, tetragonal, rhombic, monoclinic, triclinic, trigonal, or hexagonal, and which also determines the crystal angles and the disposition of faces in accordance with the law of rational indices, the law which limits the number of possible faces to those which cut off small whole-number relative lengths from the crystal axes. Indeed, it is because only those planes which contain the points of the space-lattice are possible as crystal faces that the law of rational indices obtains. For any three points of the space-lattice determine a plane in which similar points are analogously regularly repeated, and which is a possible crystal face obeying the law of rational indices. Moreover, those facial planes which are most densely strewn with points are of the greatest crystallographic importance, being what are known as the primary faces, either parallel to the crystal axes or cutting off unit lengths therefrom, as well as being usually the planes of cleavage.

As the space-lattice units are all sameways orientated, any one atom of the molecular or polymolecular grosser unit might be equally well chosen as the representative point of the lattice, so long as a similar choice were made in every space-lattice unit, and the resulting space-lattice would be the same whichever atom were so selected. Consequently, the space-lattice is afforded by the similarly (identically) situated atoms of the same chemical element throughout the crystal structure. The combined point-system (one of the 230 possible point-systems) may thus be considered to be built up of as many identical but interpenetrating space-lattices as there are atoms in the space-lattice grosser unit. These facts are concisely expressed in the definition of crystal structure which was stated as follows by Professor von Groth at the Cambridge meeting of the British Association in 1904.

"A crystal—considered as indefinitely extended—consists of *n* interpenetrating regular point-systems, each of which is formed from similar atoms; each of these point-systems is built up from *n* interpenetrating space-lattices, each of the latter being formed from atoms occupying parallel positions. All the space-lattices of the combined system are geometrically identical or are characterized by the same elementary parallelepipedon."

Having thus arrived at a comprehensive idea of crystal structure on the assumption of each atom and each grosser space-lattice unit being only a point, as far as which we are on safe and assured ground, we may proceed to the consideration of the various ideas advanced concerning the character of the units of structure thus represented by points, that is, concerning the mode in which the space around the point is more or less filled up.

The valency theory of Barlow and Pope considers the atomic point to be expanded into the sphere of the atom's influence, the relative size of which in any one substance is supposed to be proportional to the fundamental valency of the chemical element of which the atom is composed. The spheres are further assumed to be pressed together on crystallization until they fill space, becoming thereby deformed into polyhedra. The theory of von Fedorow, on the other hand, considers the grosser or space-lattice units to be parallelohedra; besides those corresponding to the 14 space-lattices there are 9 other parallelohedra (making 23 in all) composed of simple Sohnckian point-systems compounded of interpenetrating space-lattices. All the 23 parallelohedra are arranged parallelwise, and fill space without interstices. There are, however, only four types, namely, the cube, the rhombic dodecahedron (which has a second vertically elongated variety), the cubo-octahedron, and the hexagonal prism, the first three being all of cubic symmetry, and the fourth of obviously hexagonal symmetry. They are shown, including the second variety of the dodecahedron, in the next screen picture (Fig. 5). He further considers that all four may be homogeneously deformed into analogous parallelohedra of lower orders of symmetry, without ceasing to fill space when closely packed. Hence, von Fedorow concludes that all crystal structures are of either cubic or hexagonal type, including not only truly cubic and hexagonal crystals, but their deformed derivatives. The cubo-octahedron (*e*, Fig. 5) is identical with Lord Kelvin's "tetrakaidekahedron," the most general parallel-faced cell (a heptaparallelohedron) into which space can be regularly partitioned, and possessing the minimum surface for a given volume.

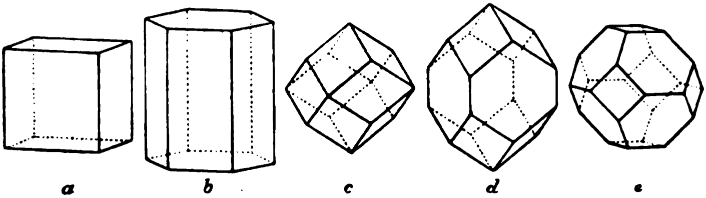

Fig. 5.—Fedorow's Types of Parallelohedra.

Unlike the atomic polyhedra of Pope and Barlow, these parallelohedra of von Fedorow are either molecular or polymolecular, in the latter event being made up of a small number of identically or symmetrically similar sub-polyhedra, termed by him "stereohedra," which represent the chemical molecules, just as already explained, when the grosser space-lattice unit is polymolecular, the stereohedra being arranged to build up the main polyhedron (the space-lattice unit) on a definite plan, which may involve mirror-image juxtaposition. For example, a rhombohedral system of stereohedra is shown on the screen (Fig. 6), consisting of two kinds, R and L, one sort being the mirror-image of the other. Each rhombohedron representing the combined system is composed of six stereohedra, three of each kind, and a series of points, similarly situated one within each stereohedron R, would constitute a Sohncke point-system, while a "double-system" is obtained by adding a series similarly situated one within each stereohedron L. If a single point were taken to represent analogously each rhombohedral set of six stereohedra, we should have a rhombohedral space-lattice produced.

The valency theory of Barlow and Pope may or may not in the sequel prove to be correct, and some facts have recently been brought forward by Barker which tend to show that it will not hold in many cases of inorganic substances. Barker, who has had the good fortune to have worked in St. Petersburg with von Fedorow for more than a year, shows that, as the lecturer has always held, the true unit of volume is the molecular or atomic volume, as determined for the particular substance itself. The molecular volume is determinable by dividing the molecular weight of the substance by the specific gravity of its crystals at a definite comparable temperature, such as 20° C., but the determination of the atomic volume offers peculiar difficulty, and so far only comparative and indirect methods have been employed, chiefly by Sollas. By taking the volumes of the spherical units to be proportional to the atomic volumes (not those of the element in the free state, as enormous compression occurs on combination), and also determining the amount of free interstitial space by comparative methods of calculation, Sollas has achieved some remarkable explanations of the crystallographic characters of the two polymorphous forms of silver iodide and of the three forms of titanium dioxide, rutile, anatase, and brookite. We have as yet no guidance from von Fedorow as to the nature of the atomic units and the volumes which they occupy. It would not be surprising if the valency volumes of Barlow and Pope, in the cases of those elements for which their theory appears to work in a satisfactory manner, turn out to be identical with the atomic volumes as determined by the method of Sollas. As regards the compounds of carbon and hydrogen, Barlow and Pope have been most successful in accounting for crystallographic and chemical relationships, and it is at least significant that both Le Bas, from experimental work on the molecular volumes of liquid hydrocarbons, and Traube from an entirely different point of view, coincide in assigning the relative volumes 4 and 1 to carbon and hydrogen atoms in combination respectively. If Traube's results

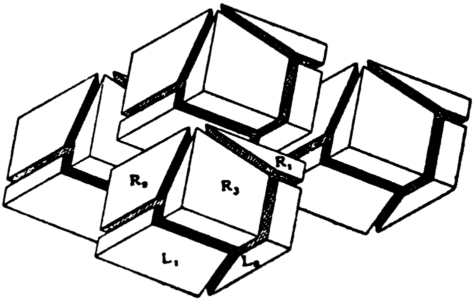

Fig. 6.—Fedorow's Stereohedra.

for carbon and hydrogen be accepted, so also must those for the relative volumes of the atoms of the halogens, sulphur, oxygen, and nitrogen, his values being: F = 1; Cl, Br, and I = 7 each; S = 6; O = 2; and N = 3. As regards oxygen and nitrogen, he agrees with Barlow and Pope, but the latter take all the halogens as of unit valency volume, and sulphur as of valency volume 2. Barker shows that while in the binary sulphides, such as zinc sulphide ZnS, the sulphur is probably of volume 2, in the sulphates, such as K_2SO_4 and $BaSO_4$, it is probably 6, as Traube insists; this conclusion is also in agreement with other work of Barker on some extraordinary cases of isomorphism, including that of barium sulphate with potassium perchlorate $KClO_4$, potassium permanganate $KMnO_4$, and the extraordinary compound potassium borofluoride KBF_4.

While it would thus appear that the atomic volume (in the substance itself, and including any interspace) is the true effective volume concerned in crystal structure, and that it may be only a coincidence that, in the cases of a few prominent elements, it happens to be approximately proportional to the valencies of those elements (as certainly appears to be true in the cases of hydrogen and carbon, and possibly oxygen and nitrogen), there is a very considerable amount of the joint work of Barlow and Pope which is of permanent value. Their explanations of the preponderating cubic and hexagonal crystalline forms of the elements themselves, and of binary compounds such as ZnS, are doubtless correct, and it will be of great interest, in view of the next development to which attention must be called, to illustrate the case of zinc sulphide, and also the structure of that most interesting simple compound, silicon dioxide SiO_2, quartz, which has been worked out in a very complete manner by Barlow.

Barlow and Pope's idea of the structure of zinc blende, which merely assumes that the volumes of the atoms of zinc and sulphur are approximately equal, is that 16 molecules ZnS go to form the grosser units of the crystal structure, the combined system or space-lattice unit—that is, 16 atoms of zinc and 16 of sulphur. Only one zinc or one sulphur atom in every 16 is sameways orientated, and if we adopt von Groth's definition, we may give the structure of zinc blende as follows: The crystals of zinc blende consist of two interpenetrating regular point-systems, one formed from zinc atoms, and the other from sulphur atoms; each of these two point-systems is built up from 16 interpenetrating space-lattices, each of the latter being formed from zinc atoms or from sulphur atoms occupying parallel positions. All the 32 space-lattices of the combined system are geometrically identical.

Barlow and Pope have shown that the space-lattice in zinc blende is the third cubic one, in which a point is situated at each cube corner and also in the centre of each cube face. For this is the space-lattice corresponding to an assemblage of spheres of equal volume in closest packing. The space-lattice in question is shown on the screen (Fig. 7), and a pair of models of the arrangement are illustrated in the next two pictures, in the first of which the points are expanded into spheres of considerable size, and in the second they appear still further expanded into actual contact. The third stage, in which the expansion proceeds until all interstices are filled up and the spheres are converted into polyhedra, is left to the imagination. In the second picture (reproduced in black and white in Fig. 8) the mutual arrangement of the spheres of the two elements in zinc blende, zinc and sulphur, is indicated by the yellow colouring of the sulphur spheres and the grey tinting of those of zinc. The tetrahedral mode of derivation of the structure, accounting for the observed hemihedrism, is also shown in another slide (Fig. 9). The eight larger cubes which together form the grosser unit are each supposed to be occupied by four smaller cubes of the same element, arranged tetrahedrally, and of zinc and of sulphur alternately in different larger cubes; on replacing the little cubes by spheres in contact the model represented in the second picture (Fig. 8, Plate II.) is produced.

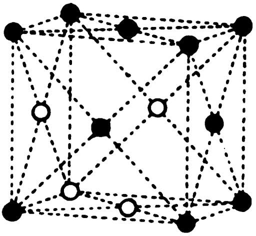

Fig. 7.—Space-lattice of Centred-face Cube.

Barlow's conception of quartz affords us an example in which the symmetry is trigonal, and of which there are two kinds possible, one of which is the mirror-image of the other, the two being helical in character and of the nature of right- and left-handed screws respectively. The two structures, as thus conceived, are represented in the next two slides (Figs. 10 and 11), the white spheres representing silicon atoms and the black ones oxygen atoms, of which there are twice as many, corresponding to the formula SiO_2. The helical character is clearly shown, the white spheres being obviously arranged in a right-handed screw in one picture and a left-handed screw in the other. Right-

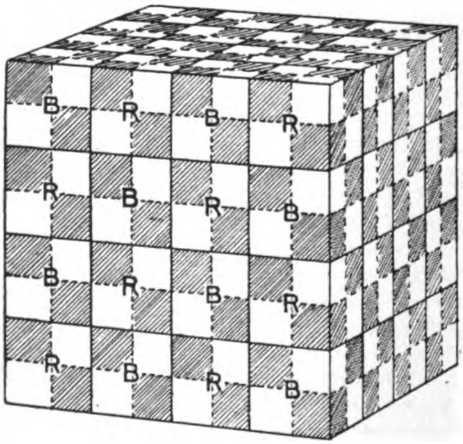

Fig. 9.—Scheme of Tetrahedral Arrangement of Zinc (B) and Sulphur (R) Atoms in Zinc Blende. Unshaded Cubes unoccupied.

and left-handed quartz crystals are so well known that it will not be necessary to do more than illustrate the fact by one or two experiments. First of all, two slides are shown (reproduced together in Fig. 12), one of a right-handed crystal and the other of a left-handed one, each exhibiting the characteristic little faces *s* and *x* of the right and left trigonal bi-pyramids and right and left trigonal trapezohedra, modifying the right and left corners formed by the meeting of the faces of the rhombohedron *r* and hexagonal prism *m*. That the right-handed crystal shows rotation of the plane of polarization of light to the right, and the left-handed crystal optical rotation to the left, may be beautifully demonstrated by cutting a plate 1 mm. thick out of each crystal perpendicularly to the axis, and using the device of 24 mica

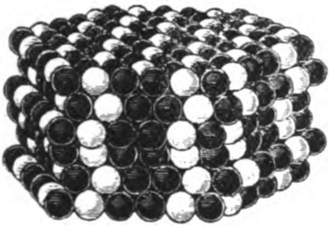

FIG. 10.—BARLOW'S CONCEPTION FOR RIGHT-HANDED QUARTZ.

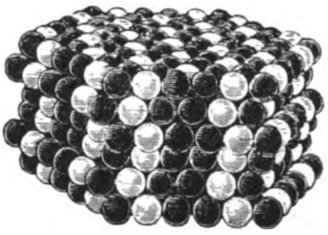

FIG. 11.—BARLOW'S CONCEPTION FOR LEFT-HANDED QUARTZ.

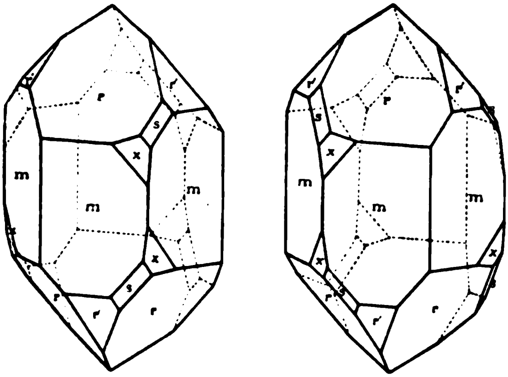

FIG. 12.—THE TWO VARIETIES OF QUARTZ CRYSTALS, LEFT-HANDED AND RIGHT-HANDED.

sectors, due to S. P. Thompson, which between crossed Nicols shows a black cross, the arms of which are deviated one sector to the right or left when the thin quartz plate is interposed, in accordance with the right- or left-handed nature of the plate. Also the phenomenon of the bi-quartz, and of the natural biquartzes often produced by twinning of right and left individuals, may be illustrated, terminating with some beautiful cases of repeated twinning, including amethyst. The imitation of the phenomena of the two varieties of quartz, by crossing strips of mica in a right-handed or a left-handed pile, as shown by Reusch, may also be referred to and demonstrated, as affording some definite proof that the mineral does possess an analogous helical structure, complementarily opposite in its two varieties.

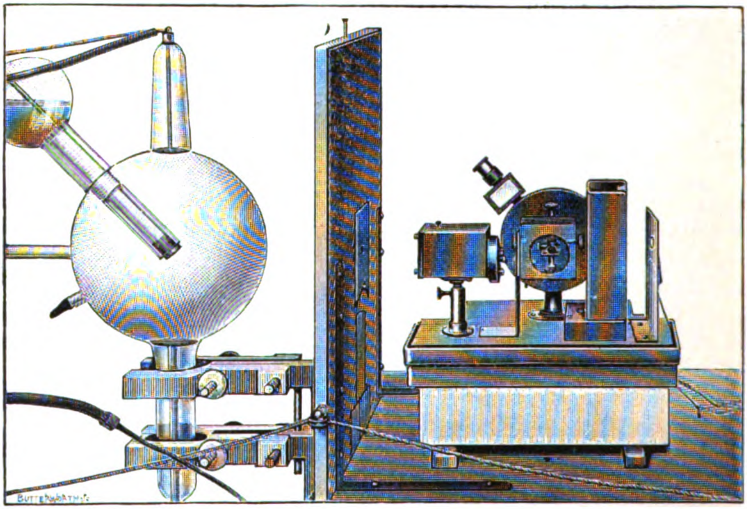

FIG. 13.—APPARATUS OF FRIEDRICH, KNIPPING AND LAUE FOR PASSING X-RAYS THROUGH CRYSTALS AND PHOTOGRAPHING THE EFFECT.

Now these two crystalline minerals, zinc blende and quartz, have been chosen advisedly as examples of crystal structure. For a remarkable series of experiments have recently been carried out by Laue, Friedrich and Knipping at Munich, where the lecturer had the advantage of seeing some of the first photographic results last summer. In these experiments X-rays were passed through crystals of various substances, notably zinc blende, and, in more recent experiments by Laue at Zurich, quartz. The issuing rays were received on a photographic plate, on which they recorded a pattern of spots having the symmetry (full holohedral) of the space-lattice present as the foundation of the crystal structure. These interesting photographs thus afford the first experimental and visible proof of the truth of the structure assigned to crystals by geometricians and crystallographers. By the great kindness of Professor Laue it is possible to exhibit the original photographs obtained with zinc blende, and one just obtained with quartz, the results for which Professor Laue has not yet published. Dr. Friedrich has also most kindly sent four excellent lantern slides expressly for this lecture.

In view of this further evidence of the richness of the original discovery by Sir William Crookes of the famous tube which is universally known by his name—producing under suitable circumstances of exhaustion the cathode rays when excited by the intermittent current from a Ruhmkorff coil, which rays, on their striking solid matter such as the soda glass walls of the tube, in turn give rise to the X-rays of Röntgen—it is fitting that some examples of Crookes tubes should be exhibited in action, and Sir William Crookes has been so very kind as to arrange a display of some of his most beautiful productions, including tubes showing cathode rays impinging on a diamond and a ruby respectively, with glowing phosphorescence under the bombardment of the electronic corpuscles, in brilliant colours in each case. By the kindness of Messrs. Newton, it is also possible to exhibit in action two recent X-ray bulbs of the particular kind used by Laue and his colleagues.

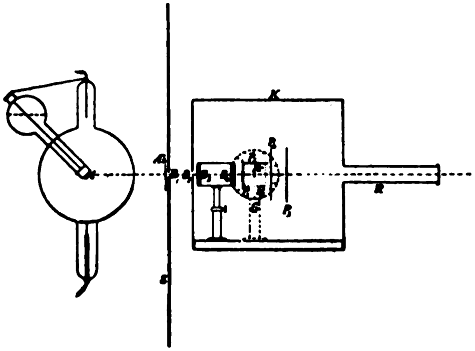

FIG. 14.—DIAGRAMMATIC REPRESENTATION OF APPARATUS OF FRIEDRICH, KNIPPING AND LAUE.

A, Anticathode of X-ray bulb; B_1, B_2, B_3, B_4, Diaphragms of Lead; K, Leaden box screen with tubular termination R; S, Large leaden screen; G, Goniometer; P_1, P_2, P_3, P_4, P_5, Photographic plates; Kr, Crystal; Al, Aluminium plate.

Two pictures of the actual apparatus employed (one of which is reproduced in Fig. 13), most kindly also sent for the purpose of this lecture, and an explanatory diagram of it (Fig. 14), will enable the precise nature of the experiment to be grasped. A plate, 1 cm. square and 0·5 mm. thick, was cut from a good crystal of zinc blende parallel to a cube face, and adjusted on the crystal holder of a goniometer in the path of a very narrow pencil of X-rays from the bulb, isolated by their passage through a succession of lead screens (lead being impervious to X-rays) pierced by small holes. The last screen, which gave the final form to the pencil of rays, was a plate of lead 1 cm. thick, pierced by a cylindrical hole 0·75 mm. in diameter, and fitted with a delicate means of adjustment so that the axis of the boring could be brought exactly perpendicular to the crystal plate. The beam of pure X-rays of circular section, after passing normally through the crystal plate, was received on a Schleussner-Röntgen photographic plate, which was afterwards developed with rodinal.

The developed plate showed an intense circular spot at the centre, caused by the direct X-rays, and a considerable number of other spots of elliptical shape, arranged in a geometrical pattern. Three of these original photographs are exhibited on the screen (and are also reproduced in Figs. 15, 16 and 17). If a series of such photographic plates be used, at different distances from the crystal (as in Figs. 15 and 16), the fact is revealed that the spots are formed by rectilinear pencils of rays spreading in all directions from the crystal, and some of them inclined over 45° to the direction of the incident rays. These deflected beams show similar properties to the original X-rays, ionizing air and helium just like the latter, and with the same degree of variation with the pressure. Hence, there can be no doubt that the character of these deflected rays issuing from the crystal is that of unaltered X-rays, and that they are due to the reflection of X-rays by planes situated at different angular positions in the interior of the crystal. In short, we are in face of reflection of X-rays from planes of atoms in the crystal.

Now a study of the spots reveals the further interesting fact that the pattern shows the full symmetry (that of class 32) of the cubic system to which the crystal belongs, although zinc blende exhibits the slightly lower symmetry of the hexakis-tetrahedral class (31), one of the formerly so-called hemihedral classes of the cubic system. This clearly proves that it is the planes of similar and similarly situated (sameways orientated) atoms in the crystal that are producing the reflections, in other words, the planes of the space-lattice.

At first Laue, who published a separate memoir on the theory of the experiments, considered that it was the space-lattice due to similarly situated zinc atoms which afforded the spot-pattern, as he had been engaged with Prof. Summerfeld in experiments relating to the action of zinc on X-rays. But there appears no reason why the sulphur atoms should not be similarly capable of producing reflections of these extremely fine vibrations or corpuscles, and as the space-lattice is the same for both elements, according to all versions of the geometrical theory of crystal structure, there is really no reason why

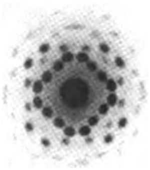

Fig. 15.—Spot Photograph afforded by Zinc Blende.
Incident X-Rays perpendicular to a Cube-face, and parallel to a Tetragonal Axis of Symmetry.

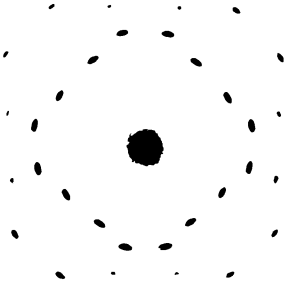

Fig. 16.—Spot Photograph afforded by Zinc Blende.
Incident X-Rays also perpendicular to a Cube-face, and parallel to a Tetragonal Axis of Symmetry, but Photographic Plate twice as far from Crystal.

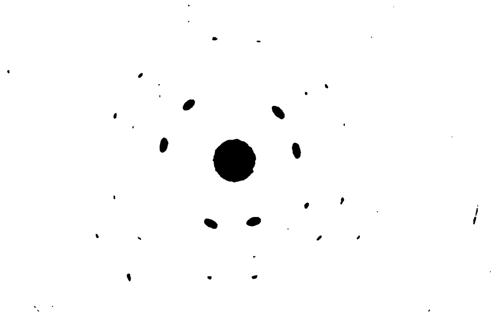

FIG. 17.—SPOT PHOTOGRAPH AFFORDED BY ZINC BLENDE. INCIDENT X-RAYS PERPENDICULAR TO AN OCTAHEDRON FACE, AND PARALLEL TO A TRIGONAL AXIS OF SYMMETRY.

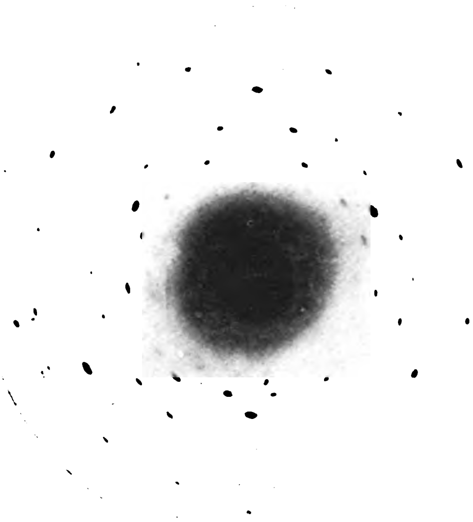

FIG. 20.—SPOT PHOTOGRAPH AFFORDED BY QUARTZ. INCIDENT X-RAYS PARALLEL TO THE TRIGONAL AXIS.

we should not consider the reflections as due to the general space-lattice of zinc blende. Laue considered the "molecules" of the crystal to form a three-dimensional grating—that is, a Raumgitter—and that each molecule is capable of emitting secondary vibrations when struck by incident electromagnetic waves from the X-ray bulb; also that the molecules are arranged according to the simple cube space-lattice (No. 1). The incident waves being propagated parallel to one of the cube axes (edges), the wave-surfaces will be parallel to the plane of the other two cube edges. He then considers the spots to be interference maxima of the waves scattered by the orderly arrangement of the molecules in the crystal. The equations of condition were next found for interference maxima of direction cosines α, β, γ, and for incident wave-length λ, and from the position of each spot the direction cosine of the pencil of rays which formed it was calculated, assuming all the transmitted pencils to come from the centre of the crystal. Thirteen spots in each quadrant were investigated, and in every case Laue's equations were satisfied; hence the conclusion that the spots are due to interference of secondary Röntgen radiation appears to agree with the positions of the spots, provided only radiations of certain definite wave-lengths are present in the incident rays.

The lecturer pointed out, in an article in 'Nature,' of November 14th, 1912, that the structure of zinc blende was probably not so simple as had been assumed by Laue, and that the space-lattice with a point at the centre of each side of the cube (No. 3) was the more probable one, the structure being that assigned to it by Barlow and Pope, as already described in this lecture.

A satisfactory explanation has since been advanced by W. L. Bragg, which does accord with this structure and with other essential conditions referred to by the lecturer, which altogether avoids the assumption of only a few wave-lengths, and which agrees with a simple reflection of unchanged X-rays from the planes of points of the general space-lattice of zinc blende. He regards the incident radiation as composed of a series of independent pulses, which, falling on a number of atoms definitely scattered in a plane, are separately reflected, each atom acting as a centre of a secondary wave, and the whole building up a wave-front. The interference maximum is thus due to the reflection of the incident pulses from a system of parallel planes of similar atoms, that is, from one of the parallel series of planes of the space-lattice. Now besides the principal planes of the space-lattice, the cube planes, the points of the space-lattice also lie in a considerable number of other planes, all of which are possible crystal faces corresponding to rational indices. For instance, the octahedral planes are very easily traced, as also those of the rhombic dodecahedron. A minute fraction of the energy of a pulse traversing the crystal will be reflected from each parallel plane in succession, and the corresponding interference maximum will be produced by a train of reflected pulses. The crystal thus actually manufactures rays of definite wave-lengths, just as a diffraction grating does, the only difference being here in the extremely short length of the waves, which is the very reason why X-rays can penetrate in this manner into the Raumgitter structure. Each incident pulse produces a train of pulses, resolvable into a series of wave-lengths, λ, $\lambda/_2$, $\lambda/_3$, $\lambda/_4$, etc., where $\lambda = 2\,d \cos\theta$, d being the shortest distance between successive identical parallel planes in the crystal, and θ the angle of incidence of the primary X-rays on the plane of points of the space-lattice. The intensity of any spot depends on the energy in the spectrum of the incident radiation characteristic of the corresponding wave-length, and this varies considerably so that certain parts of the spectrum are much more pronounced than others. Also it depends on the number of reflecting atoms in the plane, that is, on the reticular density of the possible crystal face corresponding to the plane. Hence, the greater the reticular density, the more intense the spot produced in the photograph. As reticular density is also proportional to importance of face, the primary faces having the greatest reticular density, it follows that the most important facial planes reflect the intensest spots, a fact which may prove of great value in enabling us to discover the real primary planes in doubtful cases. Each spot reflected by a plane (considered as passing through the origin and two other points) lies at the intersection of two ellipses, and the figure on the screen (reproduced in Fig. 18), showing an analysis of one of the spot photographs, exhibits this clearly. Indeed, the plane of atoms corresponding to any spot can be found from the two ellipses; for each ellipse is the section of a cone by the plane of the photographic plate, the axis of the cone being the line joining the origin (centre of the triaxial system, and considered as one of the three points determining the plane) and the particular atom (the second or third point of the three, of definite co-ordinates), and the generator of the cone being the incident beam.

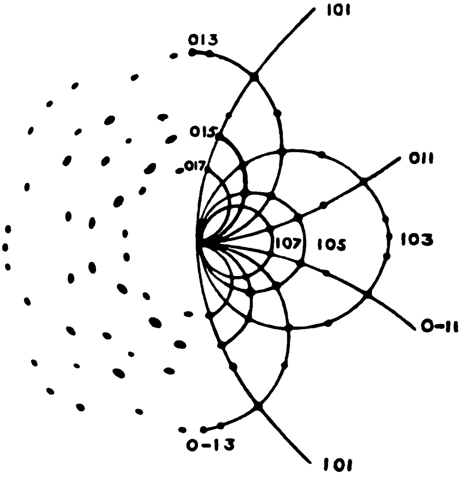

Fig. 18.—Analysis of Spot-photograph (Fig. 16) of Zinc Blende.

The interesting results of Bragg are in full accord with the assumption of the centred-face cubic space-lattice (No. 3), but not with either the simple-cube or the centred-cube space-lattice (Nos. 1 and 2). They also account for the elliptical shape of the spots. The amount of ellipticity depends on the distance of the photographic plate from the crystal. When the two are very close the spots are round, but they become more and more elliptical as the plate is receded (compare Figs. 15 and 16). The phenomenon is due to the fact that the initial rays are not strictly parallel, and the effect will be clear from the next slide (Fig. 19). The vertically diverging rays striking the reflecting planes of the upper part of the crystal meet them at a less angle of incidence than those of the lower part, and so the reflected rays converge. Horizontally diverging rays, however, diverge still more on reflection. Hence the section of the reflected beam is an ellipse with major axis horizontal.

It is of importance to note that the centred-face cubic space-lattice is characteristic both of the arrangement of identically (sameways) orientated and environed atoms of the same element, zinc or sulphur, and of the atoms of both elements regarded as equal spheres in contact. In the slide already shown (Fig. 8, Plate II), of Barlow and Pope's model, the spheres of sulphur are coloured yellow to distinguish them from the grey-coloured spheres of zinc. If we ignore the colour, and consider them as similar spheres, we see that they form the centred-face cubic arrangement. The hemihedral nature of zinc blende is, however, very likely connected with some real difference of volume between the atomic spheres. As the spot figure is holohedral it would appear to be due to the space-lattices of similarly placed atoms of either (but in each lattice only one) element, rather than to the spheres of the combined system of atoms.

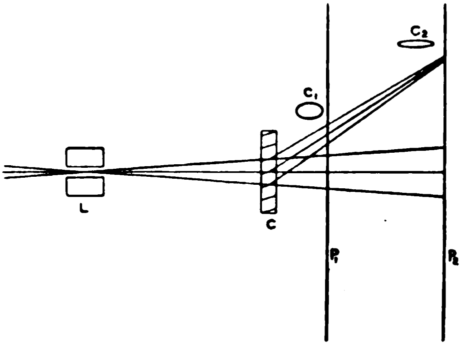

Fig. 19.—Explanation of Elliptical Shape of Spots.

This latter conclusion is further borne out by the result of the new work by Laue on quartz. The photograph now shown (Fig. 20, Plate IV), so kindly sent by Professor Laue, exhibits the trigonal nature of the symmetry very clearly, and Professor Laue informs me that the same figure is afforded by both right and left quartz, so that it does not reveal the hemihedral character of quartz which has been demonstrated this evening, but possesses the full holohedral symmetry of the trigonal space-lattice, and exhibits the threefold nature of the axis of symmetry which is perpendicular to the plate and along which the X-rays were directed.

Professor Laue has also experimented with the crystals of a number of other cubic substances, and, like zinc blende, they all show holohedral symmetry about a tetragonal axis.

W. L. Bragg has found that stronger photographs of the same nature can be obtained from mica, using nearly grazing incidence, and it is by use of this fact that Mosely and Darwin have been able to study the reflected rays electrically, and found them to resemble ordinary X-rays. By the kindness of Mr. Bragg, a diagram of his apparatus and a positive lantern slide of one of his mica spot photographs are exhibited on the screen. The large white spot is due to the direct rays ; the next brightest but smaller spot considerably above the direct one is due to rays reflected from the cleavage plane, and the other spots to reflections from other possible faces and planes of the monoclinic space-lattice ; all the spots are such as are compatible with this lattice, and with the well known fact that the monoclinic angle is almost 90°. The spots lie on two ellipses intersecting at the central direct spot and at the upper bright one, each ellipse corresponding to a set of parallel rows of points, the crossing of the two sets forming rhombic parallelograms, the basal edges of the monoclinic prismatic space-lattice cell of mica.

Incidentally these experiments appear likely to throw light on the much-debated question of the nature of the X-rays. As all the experiments unite in indicating that a fraction of the X-rays suffers reflection at the planes of atoms parallel to the more important possible crystal faces, all being planes of atomic points of the space-lattice, it would appear that the X-rays are some type of wave-motion, or at any rate some kind of pulse with an extended wave-front. Yet after reflection they retain the same corpuscular character which Prof. W. H. Bragg has shown they possess. For the liberation of a high-speed electron from an atom traversed by the X-ray cannot be explained, according to Rutherford, unless it be supposed that the energy of the X-ray is concentrated over a minute volume, and can be given up in an encounter with a single atom. Hence these experiments show that the X-rays possess at the same time the apparently opposite properties of extension over a wave-front and concentration in a corpuscular point.

It appears to the lecturer that the simpler explanation is that we are truly dealing with waves, but that the wave-lengths of the X-rays are excessively short, approaching atomic dimensions. This view that the X-rays are waves is further supported by the results of some experiments just completed by Barkla, in which a diverging pencil of X-rays was directed on a crystal of rock salt, and the issuing rays received on a photographic plate in the same manner as in the experiments already described. The developed plate shows a new phenomenon, namely, striation of the spots obtained by reflection from the planes of atoms of the space-lattice, especially in the reflections from the cubic cleavage planes. The striations are, in fact, true interference bands, due to interference of the reflections from equally spaced parallel planes of the space-lattice. By the kind courtesy of Prof. Barkla, two of these interesting photographs are projected on the screen. On the assumption that the X-rays are waves, and that the reflecting plane is one passing through corresponding portions of single NaCl molecules—which agrees with the choice of a representative point from each simple molecular grosser unit, or of a similarly situated atom of one of the two chemical elements present in each molecule NaCl to act as such representative point of the space-lattice—Barkla has calculated that the wave-length is the one hundred and sixty millionth of a millimetre, $0 \cdot 6 \times 10^{-8}$ mm. If the grosser unit be polymolecular, the wave-length works out larger, being proportional to the cube root of the number of atoms in the molecule. If 8 molecules form the grosser unit of sodium chloride crystals, as suggested by some chemists, the wave-length is found by Barkla to be twice this value, namely, $1 \cdot 2 \times 10^{-8}$ mm.; and if 16 molecules of NaCl are comprised in the grosser unit, as would be the case if Barlow and Pope's structure for the cubic binary compounds be correct (the space-lattice in the case of rock salt being that of the simple cube, No. 1), the wave-length would be still longer, about the seventy millionth of a millimetre, $1 \cdot 5 \times 10^{-8}$ mm. Now it is very interesting that these values are of the same order as those derived from determinations of the velocity of electron ejection, which varied from 1 to 2×10^{-8} mm.

The most trustworthy recent estimations of the size of a molecule of rock salt indicate a diameter about 3×10^{-7} mm. Hence the diameter of a crystallographic molecule 8NaCl would be 6×10^{-7} mm., and of 16NaCl about $7 \cdot 5 \times 10^{-7}$ mm.

It should be emphasized, in concluding the account of this fascinating new field of research, that all these reflections occur in the body of the crystal, and are not surface effects. Cleavage planes usually afford stronger results merely because they are generally primary planes of high reticular density. The effect is sometimes heightened by conducting the X-rays at nearly grazing incidence; but this is by no means necessary, and in Laue's experiments several of the planes were inclined as much as 30° to the incident rays.*

The experimental proof of the existence of the space-lattice imparts all the more confidence in approaching the other great advance which has lately been achieved. The completion of the catalogue of crystallographically measured substances by Prof. von Groth, provokes the question: What more is needed in order to enable a crystallized substance described in the book to be recognized by means of a few measurements on the goniometer? For it is now proved up to the hilt that, except in the cases of cubic crystals identical in angles in accordance with their perfect symmetry, every solid crystallizable substance is characterized by its own peculiar crystalline form and interfacial angles. This is quite true, even to the last minute of angular measurement, when the conditions of crystallization are ideal. When thus perfect, even isomorphous substances show differences among themselves to the extent of a definitely measurable number of minutes. But such perfection of growth is not easy to attain, and, in ordinary crystallization without special precaution against disturbance, is rarely found. The essential crystallographic measurements can, however, be made in an hour's time, provided use be made of the two- or three-circle form of goniometer, such as the excellent one devised by Dr. Herbert Smith. This form of goniometer enables all the needful measurements of the interfacial angles to be made with a single setting of the crystal on the wax of the holder. But practical difficulties have hitherto still stood in the way. Excellent as is von Groth's classification—and the most suitable for a work of reference of the full and comprehensive character of this permanent monument of the master's industry and wide knowledge of chemistry, related compounds being arranged and compared in close proximity—the very nature and size of such a work renders it unsuitable for the purpose of discovering rapidly the chemical composition of a substance from its geometrical elements. An index of substances arranged in the order of their symmetry and the numerical values of the crystal constants within the system is what is needed, and this has now for the first time been drawn up for the ten thousand measured substances by Professor von Fedorow.

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* See Appendix, last page.

Another difficulty then presents itself. It often entirely depends on how a crystal is held in space, that is, which direction in it is to be the vertical axis, which the right-and-left axis, and which the front-and-back axis, as to what the nature of the crystal constants (elements) will be. Moreover, even if two different observers choose these similarly, they may select a different parametral plane (a fourth face other than the three faces parallel to the axes, and cutting off unit lengths from the latter) to determine the axial ratios. Hitherto, beyond a few arbitrary rules—for instance, that the right-and-left axis of a rhombic crystal shall be longer than the front-to-back one—there has been no definite guiding principle for the determination of the setting. Professor von Fedorow has now given us one, by means of which we can be sure which are the real vertical faces (prismatic or pinakoidal), which is the basal plane (the pair of top and bottom faces), and which set of pyramid faces are the important ones fixing the relative axial lengths. The true setting has been determined by Professor von Fedorow for every one of the substances in his index, and the crystal elements for such setting calculated.

The mode of classification adopted in this index-catalogue is based on the values of the five fundamental angles which, in general, characterize the crystals of any specific substance. A cubic crystal has definite angles which are entirely fixed and rendered invariable by reason of the perfect symmetry. At the other extreme come triclinic crystals, the general case, in which all five fundamental angles are different and quite independent of each other. On monoclinic crystals there are three independent angles, from which the other two can be calculated. Rhombic crystals have only two independent angles, which, if measured, enable the other three to be calculated. Hexagonal, tetragonal, and trigonal crystals possess only one angle independent of the symmetry, determinative of the relative length of the unique axis of hexagonal, tetragonal, or trigonal symmetry.

The first object of von Fedorow in order to arrive at the correct setting, is to decide which are the primary axial-plane and parametral faces; and he is wonderfully aided here by the discovery of the fact that the faces most extensively developed under ideal conditions of growth are those over which the points of the space-lattice are most densely strewn. Hence, von Fedorow tries to discover the faces of greatest reticular density by calculation. For it is a well-known fact that the most diverse habits—due to different faces being most prominently developed under different conditions of environment—are shown by the crystals of the same substance. A capital example is afforded by the double sulphates of the monoclinic series crystallizing with $6H_2O$. One of the commonest of these salts, ammonium ferrous sulphate, $(NH_4)_2Fe(SO_4)_2 . 6H_2O$, exhibits a particular form (consisting of a pair of parallel faces), *r'*{$\bar{2}01$}, not really a primary one, so prominently, the crystals being tabular upon it as illustrated in the lantern slide (Fig. 21), that Wulff has actually taken it as the basal plane, rather than the form *c*{001}, which is considered by the lecturer, following older observers, to be the true basal plane for this whole series of isomorphous salts. For the very large development

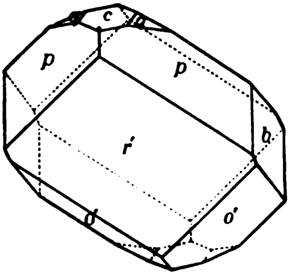

FIG. 21.—CRYSTAL OF AMMONIUM FERROUS SULPHATE.

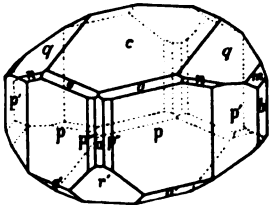

FIG. 22.—CRYSTAL OF POTASSIUM NICKEL SULPHATE.

of r' is only a feature of this particular salt of a very large isomorphous series, and the form $c\{001\}$ is in general the plane clearly and prominently announcing itself as the basal plane. The slide (Fig. 22) of a crystal of another member of the series, potassium nickel sulphate, will show this. Hence, Prof. von Fedorow naturally does not rely on the fortuitous relative development of faces, but calculates the relative reticular density of all the principal faces present, in order to discover the true primary faces by their superior reticular density.

Having thus determined the correct setting, and measured the principal angles, including the five fundamental ones, the results are recorded in the index-table in an abbreviated symbolic form if the substance be a new one, or, if it has previously been measured and therefore appears in his index-table, he discovers the fact at once by the identity of the elements found with those of a substance given in the table. The average time occupied in all this by Prof. von Fedorow or one of his skilled assistants is about two hours. Mr. T. V. Barker, who studied with Prof. von Fedorow before acting as Demonstrator of Mineralogy at Oxford, has been of considerable help in submitting the new method to a very severe test, from which it has emerged with flying colours. He collected, at Prof. von Fedorow's suggestion, fifty specimens of substances which had been crystallographically examined in this country and described in the recognized publications. Five of these were furnished by the lecturer, six others by Prof. Armstrong with the aid of Messrs. Colgate and Rodd, others by Drs. Chattaway and Drugman, and Mr. Marsh at Oxford, and the remainder by Mr. Barker himself. Each specimen was only marked by a number, no name or formula being given, on its dispatch to St. Petersburg. The result was that Prof. von Fedorow identified without any difficulty 48 of the 50 substances. The crystals of one of the two others were too imperfectly developed to be of use, and the fiftieth specimen was that of a substance which it was afterwards discovered had never hitherto been measured, a fact which was first indicated by its elements not tallying with those of any substance mentioned in the table. This latter occurrence confers even greater confidence in accepting the new method.

It thus appears that in Prof. von Fedorow's hands, or those of his pupils, the method is practically infallible, provided the crystals are well developed and not of cubic symmetry. If the latter perfect symmetry be developed, reference must be made to the optical properties, which the lecturer has always insisted have been far too much neglected, and are here seen to be indispensable. The optical methods themselves, moreover, as regards their use with small crystals on the polarizing microscope, have been further perfected by von Fedorow, his Universal Stage placing the rapid methods of two- and three-circle goniometry at the disposal of the microscopist. It must also be remembered that Prof. von Fedorow's method does not discriminate between the members of isomorphous series, as the crystals usually available are not of the high degree of perfection requisite in order clearly to substantiate the last few minutes of any particular angle; for the differences of angle between the members of series formed by metallic family analogues have been shown by the lecturer to be very minute, although unmistakable given the most perfect crystals, and have also been found to obey the law of progression according to the atomic weight of the metal. For instance, ammonium zinc sulphate was simply returned by Prof. von Fedorow as a member of the isomorphous series of monoclinic double sulphates and selenates crystallizing with $6H_2O$. Qualitative analysis would be necessary after all, in order to discover the actual member of the series present. Moreover, there are certain features of Prof. von Fedorow's own peculiar version of the theory of crystal structure, such as his idea about pseudo-cubic and pseudo-hexagonal types, and his dealing in consequence with many substances as being deformations of a higher symmetry than they actually show, which to the lecturer appear unnecessary complications likely to discourage the use of the new method. But these defects can, and doubtless will, be eliminated as the method becomes practically applied.

In conclusion, it must be obvious that a great advance has really now been made in crystallography. For the geometrical theory of crystals as homogeneous structures, based on the 14 space-lattices as the grosser structures and the 230 point-systems as the ultimate atomic structures, has been not only theoretically perfected, but proved by direct experiment to be an actual fact, by the epoch-making work of Laue, Friedrich, and Knipping. The descriptions and chemical relationships of all the ten thousand measured substances have been brought together in the great book of Prof. von Groth, and the material further sifted, reduced to correct setting, and arranged according to symmetry and elements by Prof. von Fedorow, in a tabular form immediately available as a reference index for identification purposes, thus providing the material for a true crystallo-chemical analysis. The science of crystallography is thus now placed on a secure foundation, supported equally by mathematics, geometry, and experiment, and its natural data are rendered available for chemists and physicists alike.

It must be obvious that these significant results place the science in such a strong position that it can no longer be ignored as in the past by both physicists and chemists, and that to both an elementary knowledge of the subject will be imperative and absolutely indispensable in future. That crystallochemical analysis will ever replace qualitative chemical analysis is neither to be expected nor desired, even if alone on the ground of the admirable training and experience in chemical operations and principles which chemical analysis affords. But so powerful an aid cannot with impunity be neglected, and the knowledge of crystallography required of the chemist who desires to be armed with the new weapon of research will be of the utmost benefit to him, in the sense of giving him a wider grasp of the nature both of the particular chemical solid matter under his own immediate investigation, and of the nature of solid matter, the most highly organized of all the forms of matter, in general.

[A. E. H. T.]

APPENDIX.

Since this lecture was delivered, the following further experiments with X-rays and crystals have been described in "Nature" (1912, Vol. 91, pages 111, 135, and 161). H. B. Keene has obtained with crystals of galena, mica, and rock salt analogous results to those of Laue, Friedrich, and Knipping, the spot diagrams corresponding to the holohedral systematic symmetry in each case. T. Terada has found that the transmitted rays may be rendered optically visible by means of an ordinary fluorescent screen, provided the pencil of rays be from 5 to 10 mm. in diameter and the crystal adequately transparent to the rays; this latter he found to be the case with crystals of alum, borax, cane-sugar, fluorspar, mica, rock crystal, and rock salt, in thicknesses of 4 to 10 mm. M. de Broglie has obtained spot diagrams similar to those of Laue, Friedrich, and Knipping, with fluorspar, magnetite (using an octahedron face), and rock salt; but all the spots were striated with parallel fringes. Finally, Owen and Blake have obtained what appears to be a line spectrum of X-rays by using the surface of a crystal of gypsum as a diffraction grating. The lines were always the same with different crystals, using the same X-ray bulb, but the different lines varied in intensity with the hardness (degree of vacuum) of the bulb. The evidence from the action of crystals on X-rays is thus accumulating that the X-rays are waves of exceedingly short wave-length. —A. E. H. T.

April 22, 1913.

WEEKLY EVENING MEETING,

Friday, April 4, 1913.

Alexander Siemens, Esq., M.Inst.C.E., Vice-President, in the Chair.

James J. Dobbie, Esq., M.A. LL.D. D.Sc. F.R.S. M.R.I.

The Spectroscope in Organic Chemistry.

Somewhat more than half a century ago, while engaged, with the assistance of Faraday, in preparing experiments for a Friday evening discourse in this Institution, Stokes observed that the spectrum of the electric light extended to five or six times the length of the visible spectrum when he employed prisms and lenses of quartz instead of glass. This extension occurs at the violet end of the spectrum, and consists of rays of high refrangibility to which the eye is insensitive, but which can be made apparent by means of a fluorescent screen.

At the time of this discovery, and in the years immediately following it, attention was being directed to the absorption of light by coloured solutions, and to the possibility of identifying coloured substances by the number and position of the dark bands in the spectrum of light transmitted through their solutions. Stokes saw that by his discovery of the extension of the spectrum beyond the visible region, this method of investigation might be applied to colourless as well as to coloured substances. In a paper communicated to the Royal Society in 1862, he says :—"The mode of absorption of light by colourless solutions as observed by the prism, affords, in many cases, most valuable characters of particular substances, which, strange to say, though so easily observed, have till very lately been almost wholly neglected by chemists." . . . "Having obtained the long spectrum above mentioned I could not fail to be interested in the manner in which substances—especially pure, but otherwise imperfectly known organic substances—might behave as to their absorption of the rays of high refrangibility." He proceeded, therefore, to study the action of various organic solutions on the ultra-violet rays, and found that the mode of absorption generally was so constant and so characteristic that by this single property many substances could be identified. The substances which he examined included the best known vegetable alkaloids, all of which, with the exception of morphine and codeine, he was able to distinguish from one another by means of their absorption bands. This was a very remarkable anticipation of the results of later work on the subject, when we remember the great difficulties that must have been encountered in observing and measuring accurately the position of bands on a luminous screen.

While Stokes was engaged in these researches, Prof. William Allen Miller was simultaneously at work in the same field, and Stokes left the further development of the subject in his hands. Instead of a fluorescent screen, Miller employed a sensitive photographic plate for the reception of the spectrum. The rays of light were by this means made to register their own position and intensity, and a permanent record was obtained which could be studied at leisure. Miller failed, however, to "trace any special connexion between the chemical complexity of a substance and its diactinic power." Struck by this fact, W. N. Hartley—now Sir Walter Hartley—commenced a systematic investigation of the whole subject, and it is to his researches, extending over a period of more than thirty years, that we owe, not only most of the knowledge which we now possess of the relation between the structure of organic substances and the action of such substances on the ultra-violet rays, but the elaboration of the convenient and elegant methods by which such investigations are now conducted.

The light derived from an ordinary source of illumination, such as an electric lamp, consists of waves of all degrees of refrangibility, and its spectrum shows a continuous band of colour ranging from red to violet. The limits of this visible spectrum lie between the wave-lengths 7600 and 3900.

If, instead of the electric light or other ordinary source of illumination, we employ the light emitted by one of the metals when raised to a high temperature, the spectrum is seen to consist of a series of lines of different colours and intensities lying within the same limits as the visible spectrum. But there are rays beyond the red end of the spectrum and rays beyond the violet end which excite no sensation of luminosity in the eye. By allowing the spectrum to fall upon a screen which has been coated with a fluorescent substance, such as sulphate of quinine or a salt of uranium, these rays are rendered visible for a short distance beyond the violet. But it is only when we replace the glass apparatus, with which we have hitherto been working, by a quartz prism and lenses, and substitute a photographic plate for the eye, that the full extent of the spectrum beyond the violet is revealed (Fig. 1). This is the ultra-violet region —the region which Stokes opened up to investigation, and it is with the behaviour of organic substances towards the rays of this part of the spectrum that we have mainly to do this evening.

When light is transmitted through a coloured solution certain rays are absorbed, and dark bands corresponding to these rays appear in the spectrum The importance of these bands as a means of distinguishing coloured substances has long been recognized, and, as we have already seen, considerable progress had been made with their study fifty years ago. As the bands in this case are in the visible spectrum, no special means are required for their observation.

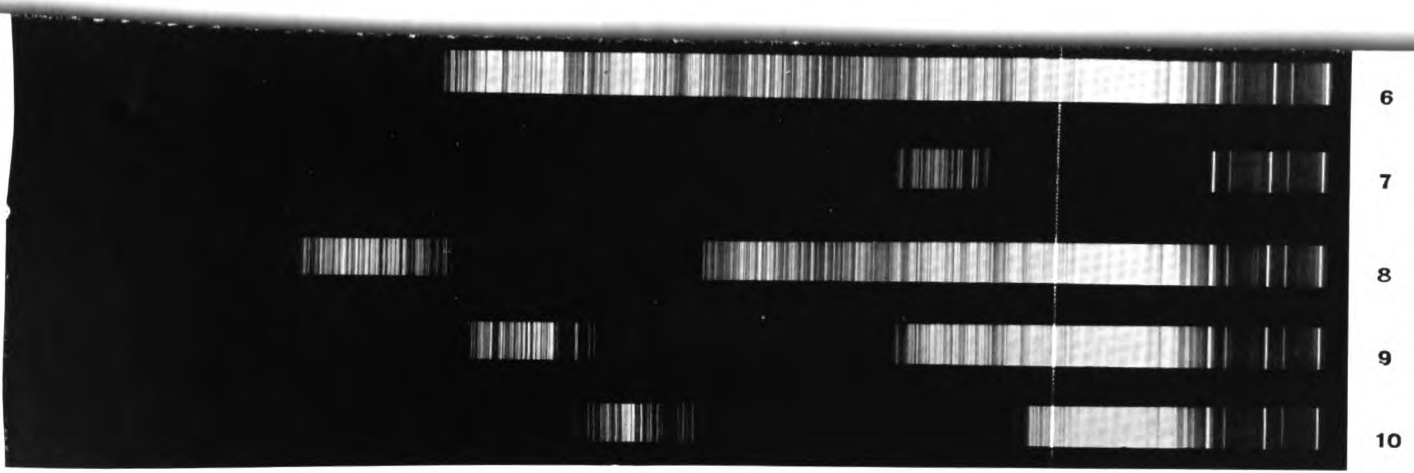

FIG. 2.

(1) Spark spectrum of nickel and iron; (2) and (3) the same after the light has passed through water and solution of cane sugar respectively; alcoholic solutions of (4) pinene (5) thiophen (6) citric acid (7) isatin (8) phenol (9) salicylic acid (10) quinine hydrochloride; (4) (5) and (6), illustrate general absorption; (7) (8) (9) and (10) illustrate selective absorption.

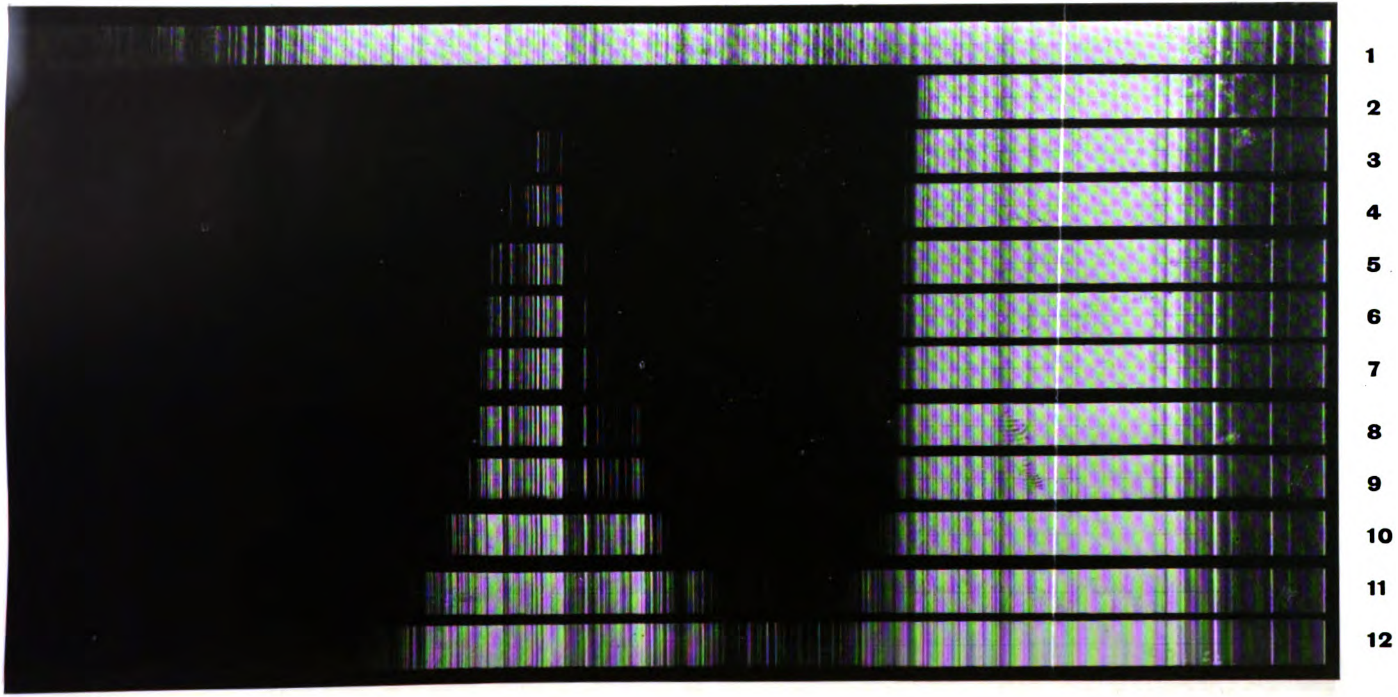

FIG. 3.

(1) Spark spectrum of nickel and iron; (2–12) the same after the light has passed through layers of 0·001 normal solution of salicylic acid from 90 to 4 mm. thick.

But when we extend this method of investigation to colourless substances, we are dealing with phenomena which lie hidden from the unaided eye, and our investigations are necessarily carried out with the help of photography.

The instrument employed in the study of absorption spectra consists of a spectroscope in which the eye-piece of the telescope is replaced by a camera. The photographic plate is set at such an angle as to bring all the rays emanating from the source of light into focus at its surface after they have passed through the resolving prism, and for this purpose it is necessary that the plate should have a very slight curvature. The prisms and lenses of the apparatus are made of quartz, which, unlike glass, is readily permeable by the ultra-violet rays. The source of light usually employed is that obtained by sparking one of the metals, such as iron, or a combination of metals, such as cadmium alloyed with lead and tin, selected so as to give a large number of lines spread as uniformly as possible over the whole ultra-violet region. The advantage of such a spectrum over a continuous spectrum is that the position of absorption bands, when they occur, can easily be determined with reference to the metallic lines whose wave-lengths and place in the spectrum are known with great accuracy. In using the apparatus a photograph is first taken of the spectrum of the source of light. A layer of the substance to be examined, which, if a solid, must be dissolved in a suitable diactinic solvent, such as alcohol or water, is then interposed between the source of light and the slit of the collimator, and another photograph is taken. By comparison of the two photographs it is seen what effect, if any, the substance has had upon the transmission of the light.

When organic substances are examined in this way it is found that some allow light to pass freely through them. Others shorten the spectrum by absorbing the rays at the ultra-violet end to a greater or less extent, and are said to show general absorption. Others, again, possess the remarkable property of absorbing rays of a particular wave-length, thereby producing gaps or bands in the spectrum; these are said to show selective absorption (Fig. 2).

In studying these phenomena in their relation to the chemical characters of a substance, it is of importance to determine not only the extent of the general absorption and the number and position of the absorption bands, but their degree of persistence, i.e. the range of concentration within which they are exhibited. It is necessary, therefore, to vary the concentration of the solution or the thickness of the layer so as to cover the whole phenomena of absorption. This is done by simply diluting the solution, or diminishing the thickness of layer on the one hand, until the entire spectrum is transmitted; and on the other hand, by increasing the concentration or the thickness of the layer until no further characteristic absorptive effect is produced. Photographs (Fig. 3) are taken at each concentration, and a curve is drawn connecting the concentration and the absorption as measured with reference to the lines of the metal employed as a source of light. The values of the spectral lines are usually stated in wave-lengths, but in these curves it is preferable to express them in reciprocals of wave-lengths. It is also convenient to make use of the logarithms of the numbers representing the thicknesses of the layers examined, rather than the numbers themselves (Fig. 4.)

If we now enquire whether the substances which affect light in one or other of the different ways already indicated have themselves anything in common, we find that it is with those which possess the structure characteristic of benzene and its derivatives that the power of absorbing the rays of particular parts of the spectrum is most frequently, although not exclusively, associated

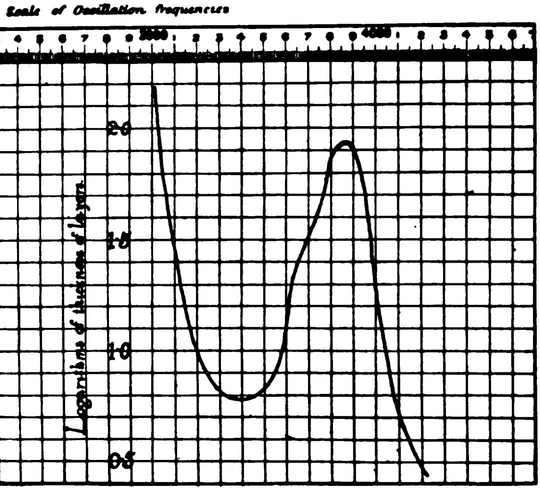

Fig. 4.—Salicylic acid 0·001 normal solution.

Organic compounds, or compounds containing the element carbon, are divided into fatty or aliphatic, in which the carbon atoms are united in an open chain, and cyclic, in which the carbon atoms form a closed chain or ring. Hexane, which is a constituent of liquid paraffin, may be taken as an example of the first class. This substance possesses six carbon atoms united together in a single chain and having attached to them fourteen hydrogen atoms, giving as the formula of the compound C_6H_{14}. This substance is highly diactinic or transparent to the ultra-violet rays, and nearly all compounds belonging to the same division of organic chemistry such as alcohols, sugars, and fatty acids are either equally transparent to light, or only cut off a portion of the extreme ultra-violet rays of the spectrum.

If we now remove one atom of hydrogen from each of the two end carbon atoms of hexane, these atoms are in a condition to unite directly with each other, thus closing the chain. The substance so formed belongs to the cyclic division of organic compounds. It is known as cyclohexane, and has the formula C_6H_{12}, each carbon atom having two hydrogen atoms attached to it. This substance resembles hexane generally in its chemical properties, and behaves towards light in the same way, that is to say, it is practically diactinic or only cuts off some of the rays of light at the extreme ultra-violet end of the spectrum. Speaking quite generally, it may be said that bodies constituted like cyclohexane absorb more of the ultra-violet rays than those constituted like hexane.

But a wholly different condition is brought about if we suppose one atom of hydrogen removed from each of the six carbon atoms of cyclohexane. One linkage is thus set free in each of the six carbon atoms, and we obtain benzene. How these linkages are actually employed in benzene has never been determined with certainty. Sometimes they are represented as mutually neutralizing one another, sometimes as effecting a double link between the alternate pairs of carbon atoms. However this may be, the structure which bears the relation that I have indicated to the structure of hexane and cyclohexane is characteristic of the large group of organic substances of which benzene is the type. It is to this division of the cyclic compounds that the great majority of substances which show selective absorption, i.e. produce breaks or dark bands in the spectrum, belong. Here, then, we have a very important and a very general relation between the structure of organic substances and their absorption spectra.

The difference in the behaviour of organic bodies towards the ultra-violet rays, as exemplified in hexane and cyclohexane on the one hand, and benzene on the other, is brought out very clearly when we examine some of their derivatives. If we replace an atom of hydrogen in hexane or cyclohexane by the monovalent group hydroxyl, we get substances belonging to the class of alcohols, and these substances are, like their parent substances, highly diactinic. If, on the other hand, we replace an atom of hydrogen in benzene by the same group we get carbolic acid or phenol, which, like benzene, exercises selective absorption on the ultra-violet rays, but gives a spectrum widely different from that of benzene.

Having dealt with the most general relation that has been observed between the structure of organic substances and their action on the ultra-violet rays, I propose to illustrate some of the more special relations by examples from the phenomena of isomerism. By replacing an atom of hydrogen in carbolic acid or phenol by the nitro-group we obtain three distinct nitrophenols. The ultimate particles or molecules of these nitrophenols are all composed of the same elements carbon, hydrogen, oxygen and nitrogen, and of the same number of atoms of each element. Such substances are said to be isomeric, i.e. they are made up of equal parts, although they do not possess the same properties. The difference between them lies in the arrangement of the parts relatively to each other; in this case in the position of the nitro-group in relation to the hydroxyl-group. On comparing the spectra of the three nitrophenols we find that they differ in quite a marked manner from one another, and afford an illustration of the important general rule that substances which have the same composition give different spectra if they differ in structure, i.e. in the arrangement of the atoms of their molecules.

It will have been noticed that the substitution of the nitro-group for hydrogen in phenol has the effect of shifting the absorption band nearer to the visible region. One of the three nitrophenols has a yellow colour, and in this case the gap in the spectrum cuts a little way into the violet end of the visible region. By the addition of soda to the solution the colour is changed to red, and on examining the spectrum of this solution we see that the gap now extends far into the visible region. This example will serve to illustrate the close connexion that exists between the study of absorption spectra and the origin of colour, an interesting branch of the subject with which, however, it is impossible for me to deal within the limits of this discourse.

In the nitrophenols we have an example of what is known as structural isomerism, or position isomerism, because the phenomenon depends upon differences in the position or arrangement of the atoms within the molecule—in other words, on the internal structure of the molecule. But it is possible to have two substances of the same composition and structure not identical, but related to one another as an object is to its mirror-image. Substances so related are termed optical isomers or stereo-isomers. The spectra of isomers of this class, unlike those of structural isomers, do not differ. This leads to an important application of absorption spectra in chemical investigations. If two substances have the same composition but different spectra, we know that they must be structurally different; if, on the other hand, they have the same composition and the same spectra, and yet are not identical, there is a strong probability, although not a certainty, that they are optical isomers. To take an example. We know three substances of vegetable origin—papaverine (from opium), canadine (from the roots of *Hydrastis canadensis*), and tetra-hydro-berberine (a derivative of the principal alkaloid obtained from species of the barberry)—which have the same chemical composition represented by the formula $C_{20}H_{21}NO_4$. Papaverine and canadine differ widely in their spectra and in their structure; canadine and tetra-hydro-berberine have identical spectra, from which we should infer that they are in all probability stereoisomers, a conclusion which has been independently established by the most satisfactory chemical evidence.

In applying this reasoning in the investigation of bodies of unknown structure, it has always to be borne in mind that while we may safely infer from dissimilarity of spectra, dissimilarity of structure, we cannot always infer similarity of structure from similarity of spectra.

The study of absorption spectra has proved of special value in the investigation of substances capable of existing in two forms which may pass from the one into the other. It is rarely the case that both forms of such substances are stable, and it is often extremely difficult, or altogether impossible, on account of this instability to determine by the ordinary chemical processes which of the two possible forms the substance as we know it possesses. Such substances, however, frequently give rise to two series of stable isomeric methyl or ethyl derivatives, whose structure can be ascertained by chemical investigation. The parent substance, if not a mixture of the two forms, must correspond in structure with one or other of these derivatives, because it is a well-established fact that the introduction of the methyl or ethyl group into a substance in place of an atom of hydrogen does not appreciably alter the spectrum.

An example of this is afforded by the three substances isatin, methyl-isatin, and methyl-pseudo-isatin. The structure of methyl-isatin and of methyl-pseudo-isatin has been determined by chemical methods, but the structure of the parent substance isatin cannot be determined in this way. Is it constituted like methyl-isatin or like methyl-pseudo-isatin ? Inspection of the photographs of the spectra of the three substances shows that while there is a wide difference between the spectra of isatin and methyl-isatin, the spectra of isatin and methyl-pseudo-isatin are almost identical, as we should expect them to be on the view that they are constructed alike.

This phenomenon, which is known as tautomerism, is due to the fact that some substances contain an atom of hydrogen, or it may be a hydroxyl group, which readily shifts its position within the molecule, leaving its union with one atom to attach itself to another. Another example of this is afforded by cotarnine, a substance found in opium. The molecule of cotarnine possesses an atom of carbon which is directly combined with an atom of nitrogen, and has also united to it a hydroxyl group. Under the influence of certain reagents the hydroxyl group leaves the carbon atom and attaches itself to the nitrogen atom, but can readily, by an alteration of the conditions, be enticed back again to the carbon atom. The shifting of the position of the hydroxyl group is accompanied by other changes which, however, it is not necessary that we should take into account for our present purpose. In this case both of the tautomeric forms are, under certain conditions, stable. The form in which the hydroxyl is attached to the carbon is colourless, while the form in which it is attached to the nitrogen is yellow. The two forms have totally distinct absorption spectra. When one of the forms passes into the other under the influence of the appropriate reagent, the amount of change is proportional to the quantity of reagent added. It is possible, therefore, by taking photographs after the addition of each successive quantity of reagent to trace the progress of the change through all its phases, and to ascertain how much of each form is present at any time. This is done by comparison with a series of reference plates prepared by photographing mixtures in various definite proportions of two derivatives of cotarnine which possess the same spectra as the two parent forms.

Incidentally I may mention that these changes afford a basis for the measurement of the relative strength of alkalis and acids. For example, the change from the yellow to the colourless form of cotarnine takes place under the influence of alkalis, and by comparing the amount of change produced by equivalent quantities of different alkalis we obtain a measure of their strength.

The spectrographic method of investigation has been applied with marked success to the study of the vegetable alkaloids. Many of these substances, such as quinine, morphine, strychnine, and atropine, possess powerful and valuable physiological properties, and their careful investigation is of the greatest importance to medical science. The number of atoms present in the molecule of an alkaloid is nearly always large, frequently as many as fifty or sixty. It is obvious, therefore, that the task of determining their exact relation to one another must be one of no little difficulty, and as a matter of fact the elucidation of the structure of an alkaloid is one of the hardest problems that the organic chemist can set himself. Any method, therefore, which assists the chemical examination is of peculiar value in this branch of organic chemistry.

In investigating an alkaloid or any organic compound, the first thing that the chemist has to do is to determine the elements of which it is composed, and the number of atoms of each of those elements present in one of its molecules or ultimate particles; in a word, what is known as its molecular formula. This is a comparatively easy operation: the difficulty comes when he attempts to determine the structure of the molecule, i.e. the exact manner in which the atoms are arranged with respect to one another. Now it may happen that two or more alkaloids have the same, or nearly the same, composition. The question arises, are they constructed on the same plan? It is in attempting to answer this question that the value of the spectroscope asserts itself. If on dissolving the alkaloids in some diactinic solvent and photographing their absorption spectra, we find that the spectra are wholly different, we may conclude that in spite of the similarity, or even identity in their composition, the substances are not intimately connected with one another. If, on the other hand, we find that the spectra are the same, or very similar, it is probable that the substances are closely related. If the composition is the same and the spectra are the same, the substances are almost certainly optical isomers. Of bodies related in this way we have already had an example in papaverine, canadine, and tetra-hydro-berberine.

Amongst the rare alkaloids of opium are two, to which the names of laudanin and laudanosin respectively have been given. They have nearly, but not quite, the same composition, laudanosin containing one atom of carbon and two atoms of hydrogen more than laudanin; in other words, it is a homologue of laudanin. Substances so related, hardly differ in their spectra, the replacement of one atom of hydrogen by the group of atoms CH_3 having very little disturbing influence, as we have already seen, upon the structure of a substance or upon its power of absorbing light. The spectra of laudanin and laudanosin being practically the same, it was inferred that they possessed the same structure and were probably homologous, before the fact had been ascertained by the usual chemical examination.

I must here advert again for a moment to Stokes work on absorption spectra. I mentioned at the outset that he had been able to distinguish all the alkaloids he examined from one another by means of their absorption bands, except morphine and codeine. At that time the relation of these alkaloids to one another was unknown. We now know that they are homologues, and in that fact have the explanation of Stokes' failure to distinguish them by their absorption bands, and at the same time a striking testimony to the accuracy of his observations.

The examination of the spectra of laudanin and laudanosin not only revealed the fact that the two substances are homologues, but gave a clear indication as to the group of alkaloids to which they are most closely allied. When the spectra were compared with those of other alkaloids, they were found to resemble the spectra of corydaline and the related alkaloids so closely as to leave very little room for doubt that they are built on the same plan as these alkaloids. At the time that this conclusion was arrived at, practically nothing was known as to the chemical structure of laudanin and laudanosin, but the inference as to their close association with corydaline was subsequently fully justified by chemical investigation.

Apart from their structure there is another point of view from which the study of the spectra of the alkaloids is of interest and importance. These substances generally have very characteristic spectra by means of which they can be distinguished with certainty from one another, except when they are homologous or otherwise very closely related. The spectroscopic method may, therefore, be used with great advantage in examinations for the presence of alkaloids to confirm the results obtained by the usual chemical tests. The chemical tests are no doubt as a rule sufficiently distinctive, but considering the gravity of the circumstances under which they have frequently to be applied, it is unnecessary to insist on the value of the confirmatory evidence which can be obtained by the use of the spectroscope.

The minutest quantities of alkaloids can be detected by this means, the method rivalling the colour reactions for the alkaloids in delicacy. Thus, with a quantity of strychnine not exceeding $\frac{1}{300}$ of a grain, a clearly defined spectrum of the alkaloid can be obtained. The photograph of morphine already shown was obtained with $\frac{1}{200}$ of a grain of the alkaloid (Fig 5), and that of nicotine with $\frac{1}{100}$.

The spectroscope serves not only for the detection, but for the estimation of alkaloids; by comparing the spectrum obtained with an unknown quantity of an alkaloid with a series of spectra obtained by photographing known quantities of the same alkaloid, it is possible to make quantitative determinations of a high degree of accuracy.

The use of the spectroscope in the detection and estimation of alkaloids in cases of poisoning, possesses certain advantages of the highest importance. One is that the material is not destroyed. The solution which has been employed for the spectroscopic examination can be used afterwards for the chemical examination. Another is that a permanent record is obtained which is always available for reference.

So far my illustrations have been confined almost entirely to colourless substances, because it is in connexion with the investigation of such substances that most of the recent advances in the subject have been made.

As my last example, I shall take the case of a coloured substance in which the method has been applied within the last year with marked success.

It will be remembered that considerable uneasiness was caused when it became known some time ago that nitrogen peroxide is sometimes employed to bleach flour. In the course of an enquiry into the subject, it became necessary to determine the nature of the colouring matter naturally present in flour. It was known that many of the yellow and orange pigments, so widely distributed throughout the vegetable kingdom, are either closely connected or identical with carrotene, the orange colouring matter of carrots, and it had been suggested that the colouring matter of unbleached flour might be identical with, or belong to the same class of colouring matters as this substance. It was impossible, however, to prove this by the usual chemical methods, because the amount of colouring matter in flour is so minute that its isolation in a pure state, and in sufficient quantity for chemical analysis, was hardly practicable. Carrotene, however, can be prepared in a pure state, and the happy idea occurred to Dr. Monier Williams, of the Local Government Board, who was conducting the investigation, to photograph its absorption spectrum and compare it with that of the colouring matter

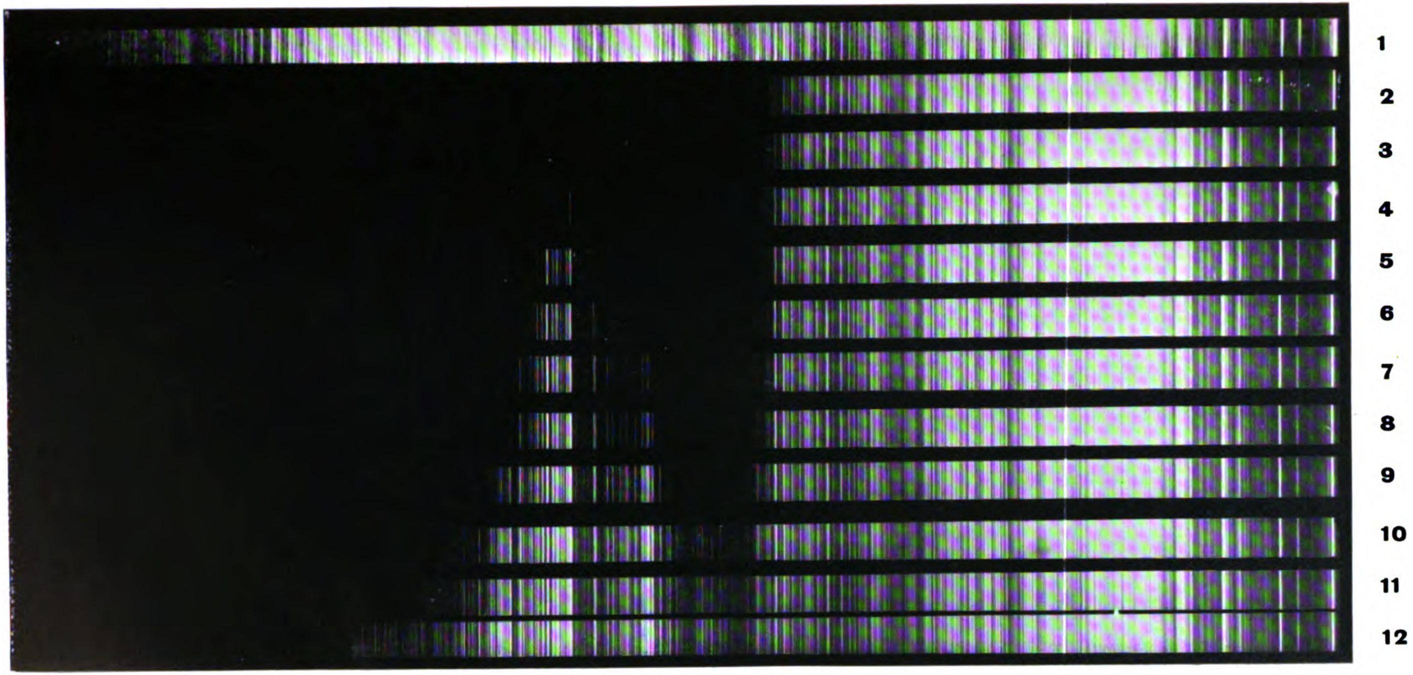

FIG. 5.

(1) Spectrum of nickel and iron ; (2–12) the same after the light has passed through layers from 60 to 4 mm. thick of an alcoholic solution of morphine containing $\frac{1}{200}$ grain of the alkaloid.

of flour, which could easily be obtained in the minute quantity required for this purpose. Inspection of the photographs shows that the spectra are very similar. There cannot, therefore, be any doubt that the colouring matter of flour, if not identical with, is closely allied to carrotene.

The underlying causes of the relations between chemical structure and absorption spectra have been the subject of much speculation. Hartley considers that the phenomena of absorption are intimately connected with the vibration of the atoms within the molecule. This view has much to recommend it, but obviously its application is limited by the present imperfect state of our knowledge of the internal mechanism of the molecule. A theory has been advanced according to which absorption bands are only exhibited by compounds whose molecules are in a state of change or flux. In the case of benzene, for example, the old idea is revived that the carbon atoms are linked together by alternate double and single bonds, which are perpetually changing positions. The presence of the bands in the spectrum of benzene, is supposed to be in some way connected with these changes. Substances, however, are known which have banded spectra in regard to which such an explanation is inadmissible.

Quite recently a promising attempt has been made to bring the production of banded spectra into relation with the theory of electrons.

It must be confessed, however, that no satisfactory explanation of the phenomena of absorption has yet been formulated, and that the theoretical development of the subject lags behind its practical application.

[J. J. D.]

GENERAL MONTHLY MEETING,

Monday, April 7, 1913.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Walter L. Allcroft, Esq.
Sir Robert Filmer, Bart., J.P.
Arthur Schuster, Esq., Ph.D. D.Sc. LL.D. F.R.S.
Colonel Archer Lloyd Marischal Turner, R.A.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday, April 11, 1913.

The Right Hon. Lord Rayleigh, O.M. P.C. D.C.L.
LL.D. F.R.S., in the Chair.

Charles J. P. Cave, Esq. M.A. J.P. M.R.I.

The Winds in the Free Air.

It was noticed in very early times that the wind in the upper air may be very different from what it is on the surface. Lucretius says: "See you not too, that clouds from contrary winds pass in contrary directions; the upper in contrary way to the lower." Bacon advocated the use of kites in studying the winds; but it is only in quite recent years that any systematic attempt has been made to investigate the free air above the surface of the earth. Kites have been flown to a height of four miles, but it is a matter of some delicacy to get even as high as two miles.

The temperature of the free air may be recorded by a meteorograph attached to a small rubber balloon, which continues to ascend until the pressure of the gas inside bursts the envelope, and the instrument descends again to the surface. The beautiful instrument constructed by Mr. W. H. Dines, F.R.S., the pioneer of upper air research in this country, is so light that the torn fabric of the balloon is sufficient to act as a parachute and check the speed of descent.

The general result of the observations has been to show that the temperature of the air decreases with height up to a certain point, above which the temperature distribution is nearly isothermal; however much higher the balloon may ascend, there is little further change of temperature. This upper layer, discovered by M. Teisserenc de Bort, whose recent death meteorologists of every country lament, is called the Stratosphere; the lower part of the atmosphere is the part that is churned up by ascending and descending convection currents and is called the Troposphere. The height at which the Statosphere is reached, as well as the temperature of the layer, varies from day to day and from place to place. In these latitudes it is met with at heights varying from about 8 to 14 kilometres, with temperatures varying from $-40°$ to $-80°$ Centigrade.

It is not, however, with temperatures that I am chiefly concerned to-night, but with the wind currents in the different layers of the atmosphere. If one of the balloons carrying instruments, or a smaller pilot-balloon, is observed with a theodolite, its position from minute to minute can be determined, and from its trajectory, or its path, as it ascends, the winds that it encounters can be calculated.

The theodolite used is constructed specially for the purpose; a prism in the telescope reflects the light at right angles, so that the observer is always looking in a horizontal direction, even if the balloon is overhead. It is important that the observer should be in as comfortable a position as possible, for an ascent sometimes lasts over an hour and a half, during which time the observer can only take his eye from the telescope for a few seconds at a time, otherwise he may lose sight of the balloon and be unable to find it again.

The balloon having been started from one end of the base, observations are taken from both ends at exactly the same times, usually every minute. From the positions of the balloon at each successive minute, which are plotted on a diagram, the run of the balloon during the minute can be measured, and hence, the wind velocity during that minute can be obtained. After the wind velocities have been measured off, and the wind directions obtained from the directions of the lines on the diagram, another diagram is constructed showing the relation of the wind velocity and direction to the height.

It is not necessary, however, to have two observers if the rate of ascent of the balloon is known; in such a case, the complete path of the balloon can be calculated from the observations of one theodolite. It is not, however, possible to know the rate of ascent with complete accuracy, as up and down currents in the air will affect the normal rate. In practice, especially in clear weather, the method is fairly satisfactory. The method of one theodolite requires less preparation, and the subsequent calculations of the path of the balloon are less laborious, than in the case of observations taken with two theodolites from opposite ends of a base line.

The best time for observations is towards sunset, so that the balloon reaches its greatest height after the sun has set on the surface of the earth; at such times the balloon, still illuminated by the sun, shines like a planet, and on one occasion I should have found it impossible to tell which was the balloon and which was Venus, except for the movement of the balloon. The distances at which balloons may be seen through the telescope of the theodolite are remarkable. A striking instance was when the flash of the sun on the small meteorograph, was seen, not once, but repeatedly, when the balloon was about nine miles above the sea and at a horizontal distance of about thirty miles.

In considering the structure of the atmosphere, as it has been revealed by the observations I have carried out, principally at Ditcham on the South Downs, we may divide the subject into two parts: firstly, the wind structure in the lowest kilometre, and secondly, the general wind distribution up to the greatest heights reached by the balloons.

It is a matter of common observation that the wind increases above the surface, and in these days of aerial navigation it is important to know the law of this increase. It seems that at Ditcham, the increase in velocity is at first linear or nearly so, and that the line representing the linear increase, passes through zero velocity at sea level. That is to say, if we plot the wind velocity at the surface and draw through it a line from zero velocity at sea level, the wind velocities at other heights, up to half a kilometre to one kilometre, will lie very nearly on this line; this approximately linear increase has been found to agree with observations at several land stations, but over the sea other conditions probably prevail.

But there are occasions when this state of things does not apply at all; this is often the case in light breezes, and at times when the surface wind is very shallow, giving place to an entirely different wind regime in the first kilometre of height. At such times it often happens, that the wind velocity is greatest a very little way above the surface. The fact that there are two separate conditions emphasizes the danger of taking means. By taking the mean value of a number of separate observations we might get as a result that the wind neither increased nor decreased in the first kilometre of height, which in reality is only true on very rare occasions. As has been truly said, "La méthode des moyennnes c'est le seul moyen de ne jamais connaître le vrai!"

Another question of great importance to aviators is the effect of hills upon the winds blowing over them. The balloons used in my investigations ascend at the rate of 500 feet per minute, and in a few minutes are carried beyond the reach of ground eddies; in some cases, however, I have found that a balloon rose with more than its normal velocity when passing over hills if a strong wind was blowing, and the effect is visible sometimes even when the balloon is more than a kilometre above the surface; on other occasions very little effect has been observed. More light is being thrown on this question by the observations of Mr. J. S. Dines on slowly ascending balloons.

The lower layers of the atmosphere up to one or two kilometres are the most important to aviators. To meteorologists the higher layers offer problems of greater interest. In considering the winds in the free air it is convenient to have some datum to which to refer them. The observed surface wind is not convenient for this purpose, being too much affected by local conditions near the ground. A better datum is what is known as the gradient wind. Under the influence of the barometric gradient the air is being pressed towards the areas of low pressure, but the wind is actually blowing more or less along the isobars at right angles to the force. In much the same way, water in a basin, when allowed to escape through a hole in the centre, and when given a slight movement of rotation, moves round the basin at right angles to the forces which are pressing it towards the centre. In the case of the atmosphere the turning movement is given by the rotation of the earth under the moving air. For any pressure condition to be maintained the air must be moving with a certain definite velocity, depending on the shape of the isobars and the steepness of the barometric gradient. This rate can be calculated for the conditions obtaining at the time, and the wind so calculated is called the gradient wind. It has been found that there is a fairly good agreement between the wind so calculated and the observed wind at a height of ½ kilometre or so, but owing to friction the surface wind is usually of a smaller velocity, and directed more towards the low pressure.

In order to show in a clear manner the changes of wind at different levels, I have prepared some models which give a better mental picture of the conditions than a diagram. The atmosphere is supposed to be divided up into layers each 1 kilometre thick, and the average wind in each layer is represented by a coloured card; the length of the card represents the velocity of the wind 1 centimetre representing 1 metre per second, 1 metre per second being about 2¼ miles per hour; the direction of the card shows the direction of the wind, the arrow flying with the wind. The red cards represent winds that may be supposed to come from an equatorial direction, that is winds from east-south-east through south to west-north-west, the blue cards winds that may be supposed to come from a polar direction.

For convenience I have divided the wind structures into five types; they are perhaps rather artificial, as I shall show later, but it is convenient to make some sort of classification, even when further knowledge must change it. In the first three types of wind structure, the wind increases above the surface and equals the gradient velocity at a height of ½ kilometre or so; above this in the first class the wind remains more or less equal to the gradient velocity, up to a height of 7 or 8 kilometres; in the second class the wind in the upper air greatly exceeds the gradient wind, and in the third class it falls off again to a lesser value; but in all three classes the direction remains much the same as that of the gradient wind.

The first type may be called the solid current; it does not seem to be associated with any particular type of isobars, but in a preponderance of cases the wind is easterly, and the remaining cases are nearly all westerly; it is rare to find the solid current with winds from the north, or from the south.

In rare cases there is hardly any wind up to the greatest heights reached, and the little wind there is often blows from varying directions in different layers; this type, which may be looked on as a sub-class of the first type, sometimes occurs in still anti-cyclonic conditions in summer.

In the second class the gradient wind, after being reached at a height of about ½ kilometre, is greatly exceeded in the upper air; in some cases the wind at 2 or 3 kilometres is double the gradient value, or even more. This type is likely to occur when there is a low pressure to the north of the station and when there is a strong temperature gradient, such that the low temperatures correspond to the low pressures, and vice versa; such conditions should theoretically cause an increase in wind velocity in the upper air, but it is not possible to calculate what the effect should be without knowing the temperatures, not only on the surface, but in the upper air over the region in question. One may, however, calculate what effect surface temperatures would have on the isobars at, say, 3 kilometres, assuming that the vertical temperature gradient is the same at every point; a map constructed to show the isobars, which have been thus calculated, must be looked on as a rough approximation only to the real conditions. A map of the isobars at 3 kilometres for May 11, 1907, shows how much steeper was the gradient on this day in the upper air than it was on the surface, a fact which quite accounts for the rapid increase in wind velocity from 2 metres per second at the surface to 19 metres per second at 3 kilometres.

Winds belonging to this class may come from any point of the compass.

The third class comprises those cases in which the wind, after reaching the gradient velocity in the first ½ kilometre or so, falls off more or less rapidly in the upper air. This class is almost entirely associated with easterly winds on the surface, when there is high pressure to the north and low pressure to the south. An east wind is usually, though not always, a shallow one; a south-west gale increases in the upper air, but when an easterly gale is blowing, causing such high seas and such dangers to shipping, it is curious to reflect that such a short distance up we should meet with light breezes, or even a complete calm.

We now come to the class of reversals when the wind in the upper air is very different in direction from that near the surface, and when it often bears no relation to the surface pressure distribution. In a typical case, after an initial increase for a short distance above the surface, we find the wind gradually decreasing as we ascend, till a layer is met with, in which there is a complete calm; above this we find an entirely different wind, which usually increases as we go higher, as in the case of winds in the second class. It looks at first sight as though there were a discontinuity in the atmosphere, but I hope to show later that this is more apparent than real. A typical example of a reversal occurred on November 6, 1908, when the surface wind was easterly with a velocity of 17 metres a second, just below one kilometre; above this it fell off to a complete calm at 3 kilometres; at 4 kilometres there was a light north-west wind, which increased to a wind of 15 metres per second at 10 kilometres, The weather map for this day is remarkable: over this country there is no sign on the surface of the westerly wind above, but it appears that in Germany, where the pressure was highest, the westerly wind must have been descending and must have divided into two currents, one flowing on as a westerly wind over Eastern Europe, the other flowing back as the easterly wind recorded in this country.

There are other cases of reversal which are not so simple as the one described above. In many cases this type is associated with small depressions, or with small areas of high pressure which seem to be relatively shallow. The surface winds are related to these shallow systems, while the upper winds are controlled by larger areas of high and low pressure, shown on the weather maps at places lying farther from the point of observation.

On September 30, 1908, for instance, a southerly surface wind, after remarkable backing, gave place to a calm at 3 kilometres; above the calm another southerly wind is met with; in this case the surface wind is probably related to the high pressure system over Germany; the upper wind to the depression approaching from the Atlantic. There was another somewhat similar case on November 16, 1908, though with winds from a different direction; the northerly surface wind backed, and a calm was met with; above this, very unexpectedly, came a thin stream of southerly wind, above which again was a north wind, increasing in velocity with height. In this case the lowest wind was part of the circulation of an anticyclone which was approaching these islands from the Atlantic; the intermediate southerly wind was perhaps the last remaining effect of the anticyclone over the Continent, while the upper wind was the outflow from above a depression near Iceland, a wind which belongs to another class to be noticed later.

In cases of reversal we find that the warm wind flows over the top of the one that comes from a colder region; there must somewhere be a line where the warm current is rising, where it must be cooled dynamically, and where its moisture may condense into cloud or rain. It is interesting to note that in most cases rain occurs somewhere in the region of the reversal, and in summer, thunderstorms are frequent. Thunderclouds may often be seen to be in a wind coming from a contrary direction to the wind on the surface, and it seems possible that for anything like a sustained thunderstorm something in the nature of a reversal must exist; it is difficult to see how a difference of potential, sufficient to produce lightning, can be kept up unless winds from different directions are bringing masses of air at different potentials near to one another.

It has been noticed in Hampshire that when the sound of gun-firing in the Channel is distinct, it is, in summer, a sign of thunder; an explanation may be hazarded: if there is a reversal so that the upper wind is coming from the south, the sound waves travelling from this point with a slight upward tendency will be refracted on entering the upper current, and thus, instead, of being dissipated in the upper air, may again reach the surface at a considerable distance from their point of origin. Such conditions of wind are those productive of thunderstorms. This may also possibly account for the superstition that gun-firing produces rain ; the sound of guns is only carried to great distances under the conditions I have mentioned, which are precisely the conditions favourable for heavy rains.

A north-east wind with rain lasting many hours is a common and a very unpleasant type of weather ; it is not obvious where the moisture comes from with such a wind, for the air from the dry regions of the Continent could hardly become saturated in its short passage over the North Sea. I believe the moisture comes from the Atlantic in a south-westerly wind in the upper air. Balloons cannot be followed for any length of time in such weather, but I have sometimes observed that the north-easterly wind slackens considerably below the cloud level, and sometimes, when breaks in the clouds have enabled balloons to be followed a little farther, there have been unmistakable signs of reversal. A careful watch for upper clouds, seen through rifts in the lower cloud sheet, will often indicate an upper southerly wind. So sure do I feel of these facts that though living only 12 miles from the Channel I never hesitate to send up an instrument-carrying balloon in rainy weather with a north-easterly wind, feeling sure that, though the balloons may go towards the sea at first, they will ultimately return and fall on dry land. My confidence is usually rewarded, the balloons coming to earth in the Midlands, or eastern counties.

The last type of wind structure to be considered is the outflow that seems to take place from the upper layers over a low pressure system, causing west to north winds in the upper air on the east and south sides of the depression. Depressions out in the Atlantic, which cause south-west winds on the surface, give rise to west or north-west winds in the upper air over England ; even cyclones as far off as Iceland produce such winds, and as they pass along the Arctic Circle, between Iceland and Norway, they show their presence by an upper northerly wind over this country. As the upper wind is often quite different from that on the surface, reversals are frequent in this class, and are associated as usual with rains, and with thunderstorms in the summer. It may be that much of the rain that falls in the cyclonic depression is due to the rising of this outward flowing current over the very different surface current on the east and north-east sides of the depression.

In connexion with the subject of reversals, I may mention the wave and ripple clouds that form such beautiful skyscapes. It used to be supposed that these were formed by winds from different directions flowing over one another and setting up waves ; but the observations of pilot balloons have shown that between two currents from different directions there is either a layer of calm, or else the wind changes round gradually ; two very different currents are not found in close juxtaposition : there is no abrupt transition between them.

To show the relation of the different types of wind structure to the surface pressures, a model has been prepared; on the map are shown a depression and an area of high pressure, with arrows to show the wind directions; above the map is a sheet of glass to represent the first 5 kilometres of the atmosphere; on this are marked the winds one would expect to meet with at this level under the conditions of pressure supposed; above this sheet of glass is another representing the thickness of the atmosphere from the 5 kilometre level to the stratosphere. The model is on the scale of one-millionth, the vertical scale through the glass being approximately the same as the horizontal scale.

The churning up of the air resulting from the heating of the surface layers by contact with the earth heated daily by the sun, does not presumably reach into the stratosphere; there being no vertical movements we should expect to find only such horizontal movements as are consistent with a suitable distribution of density. In the simplest cases the wind increases in velocity till a maximum is reached just below the stratosphere; above this the wind begins to diminish, and sometimes falls off in a very marked manner. There are occasions when all real wind seems to have ceased, and the balloon as it ascends through this curious region moves first in one direction and then in another, so that the relation of wind direction to height can only be represented on a diagram by a disconnected series of points.

What takes place still higher? Does this region of calm extend to the very confines of the atmosphere? We have practically no evidence to go on. In February, 1909, a meteor left a magnificent streak which was visible for two hours and a half; this trail, which was some 40 miles above the surface of the earth, moved in a manner suggesting very high wind velocities, with sudden variations in the different layers through which it passed. But it is possible that the streak of a meteor may partake of the nature of an aurora, whose luminous patches sometimes move in a remarkable way, and probably under forces other than those of the winds.

Having for purposes of classification divided the wind structure of the atmosphere into different classes, I must now attempt to put them together, and to show that some of the types that seem very different are in reality closely connected.

Following on enquiries made by Mr. W. H. Dines on the correlation between the surface pressure and various meteorological elements at a height of 9 kilometres, it was suggested by Dr. W. N. Shaw, F.R.S. that the changes of pressure to which our changes of weather are due, have their origin, not near the surface of the earth as hitherto supposed by many meteorologists, but just below the level of the stratosphere at a height of 9 kilometres or so above the surface. This view is in accordance with the observed facts of the wind distribution in the different layers of the atmosphere.

Supposing that on a certain day there is a pressure distribution just below the stratosphere, which at that level produces a westerly wind of a certain strength ; this pressure distribution will be transmitted through all the lower layers of the atmosphere, and unless modified by other conditions will produce a west wind at the surface ; the velocity of this wind will, however, be only about one-third of that at the 9 kilometre level owing to the greater density of the air near the surface. If, however, the air to the north at every height were at a lower temperature than the air at a corresponding height over the place of observation, there would be at all levels a tendency for easterly winds. This will have the effect of reducing the westerly wind as we descend through the atmosphere, and when the surface is reached the west wind will have a much lower value than it would have had were it only for the increased density of the air. If the wind at the 9 kilometre level is not very strong, or if the tendency to produce an easterly wind is strong, as would be the case if the air to the north were very cold, we may get a calm at the surface, or the calm may even be reached at some distance above the surface, in which case the tendency for easterly winds may actually produce such a wind, which will increase in velocity as we descend towards the surface under the layer of calm, and be strongest a little above the surface of the earth, at a point where surface friction begins to cause a diminution of velocity.

If, again, at the 9 kilometre level there is a pressure distribution producing an easterly wind, cold air to the north will produce a tendency for an increase of easterly wind as we descend through the atmosphere ; but the greater density of the air at the lower levels will produce a decrease of wind velocity from whatever direction the wind may be coming ; the two tendencies may neutralize one another, in which case we get a solid current of east wind between the stratosphere and the ground level.

If there is no wind at the 9 kilometre level, cold air to the north will produce easterly winds in the lower levels, in which case we should find easterly winds increasing in velocity as the surface is approached.

These considerations give some idea of the mechanism by which the different types of vertical wind structure may be produced. The wind increasing in height, the solid current, the wind decreasing with height, are seen to fall into their places. The reversal, with an east wind near the surface and a west wind higher up, is only an extreme case of the slackening of the westerly wind near the surface ; and the point of reversal, far from marking a point of discontinuity in the atmosphere, is seen to be merely the result of forces extending right through the lower part of the atmosphere, between the stratosphere and the earth.

If the winds are resolved into components at right angles to each other, that is north-south, and west-east components, it is found that in most cases the west-east component decreases below the stratosphere and is a minimum near the surface, an east wind in this case being considered as a negative west wind. This is what should be the case if the ideas I have been considering are correct, for the air to the north is generally colder than the air over this country. In the case of the north-south component we find no such general rule, but this also is as it should be, for the air to the east and west may be either of the same temperature, or warmer, or colder than the air over the station, in other words, there is a normal north to south temperature gradient but not a normal west to east gradient, in our islands.

The supposed cases mentioned are of course simple types, and it can be readily understood how varying conditions of pressure and temperature may in similar ways produce varieties of vertical wind distribution. In considering the pressure distribution just below the stratosphere as the regulator of the winds and the weather in the lower part of the atmosphere, I fear I have nothing to add concerning the laws governing these pressure distributions; the idea is a new one and has yet to be worked out in its details, and to stand the test of criticism and fuller investigation.

[C. J. P. C.]

WEEKLY EVENING MEETING,

Friday, April 18, 1913.

SIR FRANCIS LAKING, BART., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

THOMAS MARTIN LOWRY, ESQ., D.Sc. F.C.S.

Applications of Polarized Light.

ON November 30, 1812, just over 100 years ago, the French physicist Biot communicated to the Institute of France a memoir "On a new kind of oscillation which the molecules of light experience in traversing certain crystals." In this paper, which extends over 371 pages of the printed memoirs, the phenomenon of "Rotatory Polarization" was described for the first time. This phenomenon, as most of you will be aware, depends on the property which certain substances possess of taking a beam of polarized light and imparting a twist to the plane of polarization: the beam of light enters with all the vibrations compressed, say, into a vertical plane; it emerges apparently unchanged, but careful examination shows that the component vibrations are no longer vertical, but inclined either to the right or to the left. The importance of this discovery to physicists and to crystallographers was immediately obvious. In our own generation its fertility has been realized also by chemists, who have found in the polarimeter an instrument which promises to render to the science services not less notable than those which have been accomplished with the help of the spectroscope.

A.—SOURCES OF POLARIZED LIGHT.

If one were to ask what progress has been made in the facilities for applying polarized light to the study of chemical and physical problems, the answer would be two-fold. On the one hand it must be acknowledged that the "Iceland spar," by means of which Huyghens in 1678 first detected the polarization of light, is still the best substance for producing this effect. But the increasing demand for the spar has not been accompanied by any corresponding increase in the supply, and large clear pieces of the mineral are becoming increasingly difficult to procure. It may indeed be doubted whether large polarizing prisms such as those which have been handed down as heirlooms at the Royal Institution could now be purchased at any price, in view of the "spar-famine" which has prevailed for some years.

Considerable advance has, however, been made in the direction of improved methods of illumination. The solar light, which figured so largely in the experiments of the earlier workers, is too precarious to satisfy the ardent worker of to-day, and in any case could render no direct assistance in illustrating a Friday Evening Discourse.

When Faraday on Friday, January 23, 1846, delivered his discourse on "The Magnetization of Light" to an audience of 1003 persons, the source of light in the experiments which he described was an Argand gas-burner. Professor Silvanus Thompson in 1889 was able to use the electric arc, which was then just beginning to come to the front as a commercial illuminant. With this unrivalled source of light he was able to show for the first time in a public lecture a large number of the properties of polarized light which had been reserved hitherto for individual observation in the laboratory.

The remarkable effects which are seen when light of one single colour or wave-length is substituted for white light were shown by Spottiswoode in 1878, with the help of a powerful sodium-lamp which had been devised by Sir James Dewar. His lecture was aptly described as "A Nocturne in Black and Yellow."

During several years I have taken a special interest in seeking to discover other sources of monochromatic light for use in experiments on polarization, and have been particularly concerned to proclaim the merits of the mercury arc as an illuminant for everyday use in optical investigations. On account of the predominance of three pure colours, the few experiments with the mercury arc, which I hope to show you this evening, might perhaps be described as a "Pastoral in Yellow, Green and Violet."

THE MERCURY ARC.

The spectrum of the light produced by passing an electric discharge through mercury vapour was described by Wheatstone in 1835 in a report to the British Association "On the Prismatic Decomposition of Electric Light"; but it was not until twenty-five years later that a real mercury-lamp was invented by Professor Way. This consisted of an intermittent jet of mercury which was directed into a cup half an inch below. The current from a battery of Bunsen cells was passed through the jet and developed an intense light. The spectrum of the light was examined by Dr. J. H. Gladstone, and described in a paper "On the Electric Light of Mercury," published in the 'Philosophical Magazine' of 1860 (Vol. XX. pp. 249–253).

The first use of the mercury arc as a source of light in polarimetry appears to have been made just ten years ago by two German workers, Disch and Schönrock, working independently (Disch, Ann.

Phys. 1903 (IV), Vol. XII. 1155; Schönrock, Zeit. Vereins Deutsch. Zuck. Ind., Tech. Part, 1903, Vol. LIII. 652). Through the personal kindness of Mr. Bastian, I was enabled about three years later to make use of the same source of light in what is still, perhaps, its most convenient form. The glass Bastian lamp was designed to burn with the coils of the arc in a horizontal plane, and was arranged to light automatically in this position. It was with great delight, therefore, that I discovered that, in spite of all warnings to the contrary, the lamp would continue to burn for any length of time with the coils raised into a vertical plane; in this position one of the straight portions of the arc could be focused by a condenser directly on to the slit of a spectroscope, and so used to illuminate the field of a polarimeter. The lamp consumed very little current, and could be connected directly to the ordinary lighting circuits without any risk of "blowing" the fuses; it was cheap to purchase, and as the resistances formed part of the holder of the lamp there was no need for any auxiliary apparatus whatever. In view of its special suitability for polarimetric work, it is to me personally a matter of some regret that this pioneer lamp has been displaced completely by the more powerful arcs, encased in refractory silica glass, which now adorn the exteriors of so many places of amusement. I use the word "exteriors" advisedly. Dr. Gladstone in 1860, in describing the properties of the mercury light, "was struck by the strange manner in which it modified the apparent colours of surrounding objects, and especially with the ghastly purple and green hues which it imparted to the faces and hands of the spectators." These green and purple hues would not be likely to find favour with a modern audience, and it is therefore not surprising that this powerful and economical lamp is more valued as a source of light for factories and dockyards than for exclusive use in lecture theatres and drawing-rooms.

When using the mercury arc as a source of violet light, account must be taken of the greatly reduced sensitiveness of the eye to light of such short wave-length. It is here that the silica mercury lamp has proved of such great utility. I am indebted both to Mr. Lacell, of the Silica Syndicate, and to the Brush Electrical Engineering Company for allowing me, for experimental purposes, to distort their well-considered designs for commercial mercury arc lamps. Here, for instance, is a horizontal lamp which has been altered so that the arc can be seen at its greatest intensity in an end-on position. At first the light was liable to be obscured by globules of condensed mercury. But by recessing the window it was kept sufficiently hot to prevent condensation, and this difficulty was effectively overcome. Even then, however, the arc was not so convenient as one arranged in a vertical plane, like the upturned Bastian lamp. It was at this stage that I persuaded the Brush Company to modify for me their "Quartzlite" lamp by twisting one of the terminal U-tubes into such a position that it did not empty itself when the lamp was raised into a vertical plane. The "end-on" lamp and the vertical Quartzlite lamp have been described in the Transactions of the Faraday Society (1912, vol. VII. pp. 267–270), and were exhibited at the Optical Convention of June 1912. The lamp which I am using to-night has not been described previously. It combines the merits of both of the preceding patterns, and can be used either horizontally or vertically, and either in a side-on or in an end-on position.

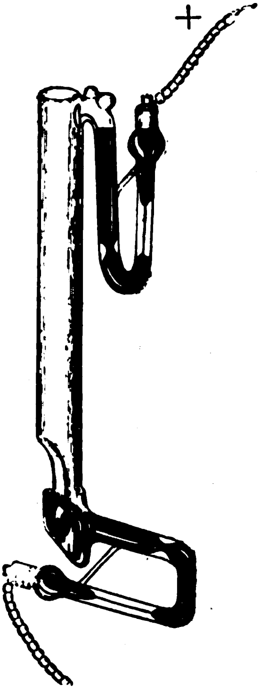

Fig. 1.—Mercury Lamp for use in a Horizontal or Vertical Side-on or End-on Position.

The "Pinch Effect."

One feature of the silica mercury-lamps is sufficiently remarkable to deserve attention. When the arc is first struck by tilting the lamp it fills the whole of the bore of the half-inch tube which encloses it; but, in accordance with Faraday's observation that currents travelling in the same direction attract one another, the parallel threads of current are drawn together until finally, as you see, the arc is "pinched" together into a thread occupying only about one-third of the diameter of the tube. This pinching together of the arc contributes substantially to its efficiency as an illuminant in polarimetric and spectroscopic work, and enables me to-night to use the arc as a linear source of light without introducing any auxiliary slit. The shape of the arc is, however, altogether unsuitable for projection, which demands, as a condition for successful work, a powerful point-source of light. I must therefore apologize for the small scale on which I have been obliged to project my illustrations, and should like to add that even these imperfect experiments have only been rendered possible by using the enormous Nicol prisms bequeathed to the Royal Institution by Warren de la Rue and by William Spottiswoode.

If the current in the mercury arc is increased, the pinching effect may extend to the point of breaking the threads of current completely and so extinguish the arc.

It may be of interest to some if I refer here to the well-known fact that the "pinch effect," which I have exhibited on a small scale in one of these tiny mercury-lamps, is of great importance in the electrical melting of steel on a large commercial scale. In that case

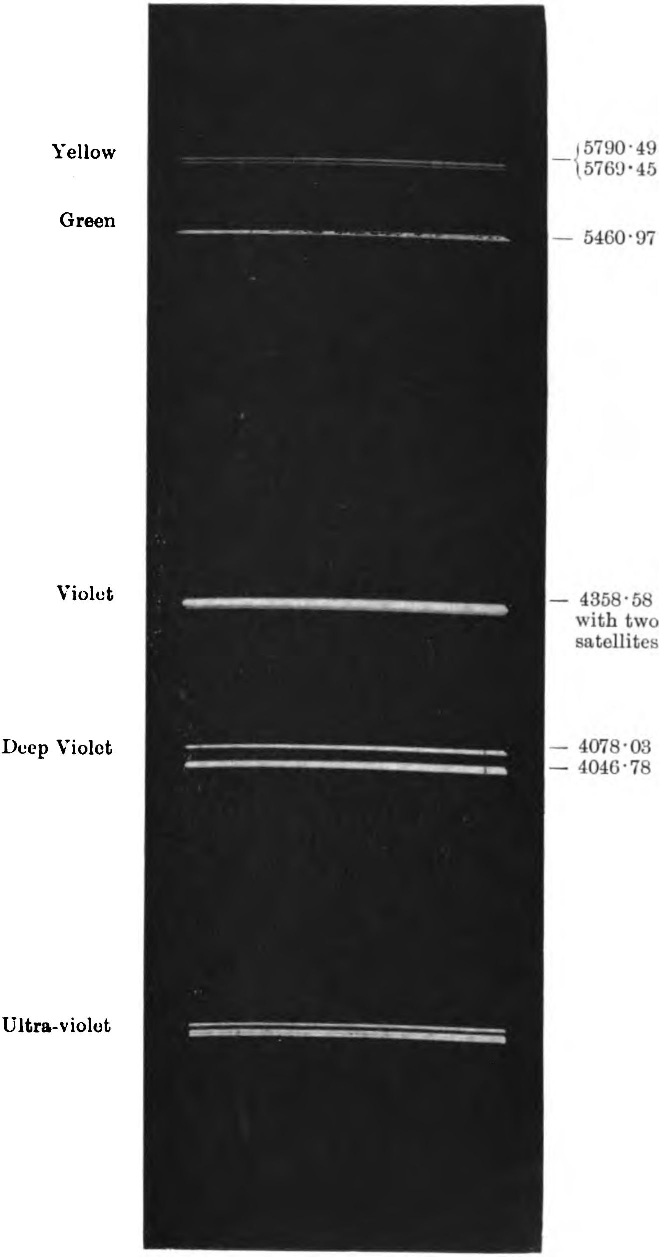

FIG. 2.—THE VISIBLE SPECTRUM OF MERCURY.
(By courtesy of Messrs. A. Hilger, Ltd.)

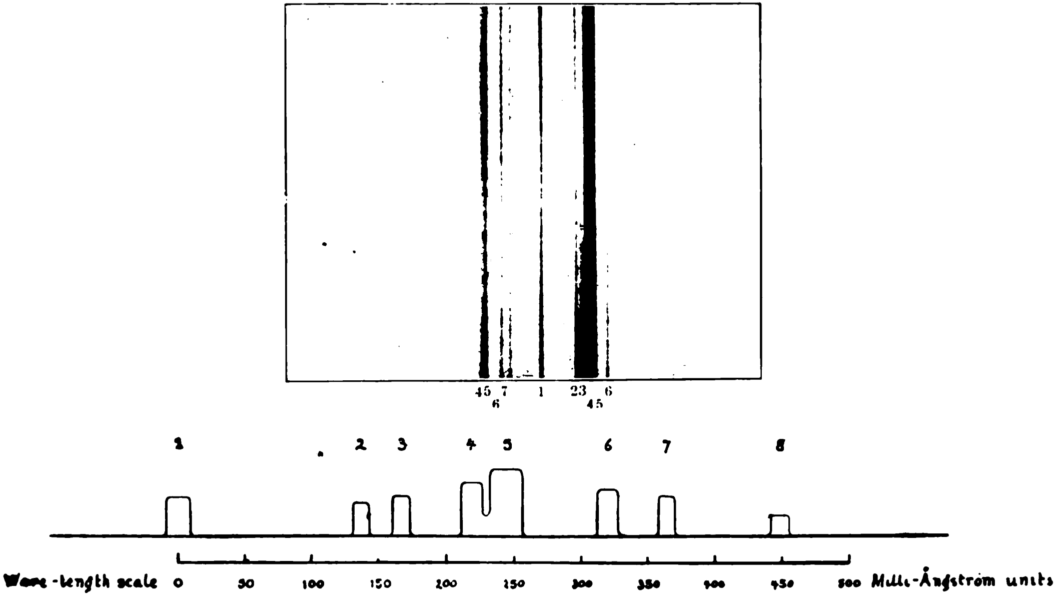
45 7 6 1 23 6 45
1 2 3 4 5 6 7 8
Wave-length scale 0 50 100 150 200 250 300 350 400 450 500 Milli-Ångström units

FIG. 3.—RESOLUTION OF THE MERCURY-GREEN LINE BY THE ECHELON SPECTROSCOPE.
The actual distribution of the components is shown by the diagram.
(*By courtesy of Professor Stansfield.*)

a current of great magnitude, flowing through a mass of molten steel enclosed in a circular channel, sometimes causes the metal to pinch together to such an extent that the circuit is actually broken. The "pinching" apart and running together of the mass of molten metal, which may weigh as much as three or four tons, produce a somewhat thrilling display.

THE MERCURY SPECTRUM.

The mercury arc differs from the carbon arc in giving an extremely simple line-spectrum, the chief features of which are a yellow, a green, and a violet component. The yellow component contains two lines, separated by about 20 units of wave-length as compared with six units for the yellow sodium doublet; it shows up well in the spectrum, but on account of its duplex character it is not suitable for use in exact measurements.

By means of powerful high-resolution apparatus, such as the echelon spectroscope, the green line of the mercury spectrum has also been shown to be complex; but in this case the components are so close together that they do not in any way reduce the value of the line as a source of monochromatic light. The extreme brilliance of this green line, its high spectroscopic purity, and the ease with which it can be produced, have given to it an unrivalled position amongst the various sources of monochromatic light which are now available for polarimetric work. I can say with confidence that no one who has worked with the mercury lamp will ever wish to return to the sodium flame, which it is rapidly displacing both in scientific and in technical laboratories.

Dr. Gladstone directed special attention to the strength of the violet lines in the spectrum, of one of which he said that "this ray is situated far beyond what is ordinarily considered the limit of the luminous spectrum." This deep-violet component contains two lines which are clearly visible in the spectroscope; but they lie so near to the limit of visibility that their presence can be shown most clearly with the help of a fluorescent screen. The bright violet line is, from the scientific point of view, one of the most valuable features of the mercury spectrum. The main line is accompanied by two satellites of greater refrangibility; but these are so close to the principal line, and are of so much smaller intensity, that they do not diminish appreciably the unique value of this line, which still remains the most powerful source of monochromatic light for work at the violet end of the spectrum. Actual measurements in which the violet line has been used both with and without the satellites have shown that the errors introduced by the presence of the latter do not exceed one part in 10,000 on the readings of a polarimeter. This error would, therefore, be quite inappreciable in the case of all readings of less than 100°.

The spectrum that I have shown to you does not by any means exhaust the usefulness of the mercury arc. In addition to the lines of the visible spectrum, there is a powerful series of ultra-violet lines which are freely transmitted by the glass of the silica lamps. These are of value for a number of scientific purposes, and have found an important technical application in the sterilization of water.

To touch on a subject that is perhaps less generally familiar, I have found that, on the other side of the spectrum, the magnificent though invisible line at wave-length 10,140 is of unique value as a starting point for calibration work in the infra-red. It will also be remembered that some of the longest waves of light that have yet been detected were discovered by Rubens in the radiation from a mercury lamp.

RESOLUTION OF THE MERCURY SPECTRUM.

One merit of the mercury arc as a source of light consists in the readiness with which the three main components may be separated. A direct vision prism of quite moderate dispersive power, placed in front of the eye-piece of a polarimeter, produces a separation of the three images which is sufficient for most purposes. The lines may also be separated by means of coloured screens prepared from gelatine films stained with suitable dyes. The efficiency of these screens may be shown by interposing them one by one in the path of the beam which forms the images which are now thrown on the screen. The lines selected for transmission are weakened in intensity, but the others are absorbed so completely that the transmitted light can be obtained spectroscopically pure.

Occasionally it is desired merely to reduce the glare of the green line in order to render the violet line more distinctly visible. An ideal dye for this purpose, which was suggested to me a year ago by Dr. Mees, is xylene red.

This substance fluoresces so beautifully under the light of the arc that I cannot resist the temptation to show it to you in the striking form which Sir James Dewar has used to exhibit this effect in the case of other dyes.

If I have dwelt at some length on the merits of the mercury arc, I may perhaps be excused on the ground that to one's scientific colleagues the description of a novel tool, which they may be able to use in their own work, is usually of greater interest and importance than any record of the work for which it may already have been used.

B.—ROTATORY POLARIZATION.

The phenomenon of rotatory polarization was first discovered in the case of quartz. Arago in 1811 (Mem. Inst., 1811, pp. 93–134) found that a plate of quartz interposed between a polarizer and analyser was capable of depolarizing the light in such a way that transmission took place where previously there had been complete

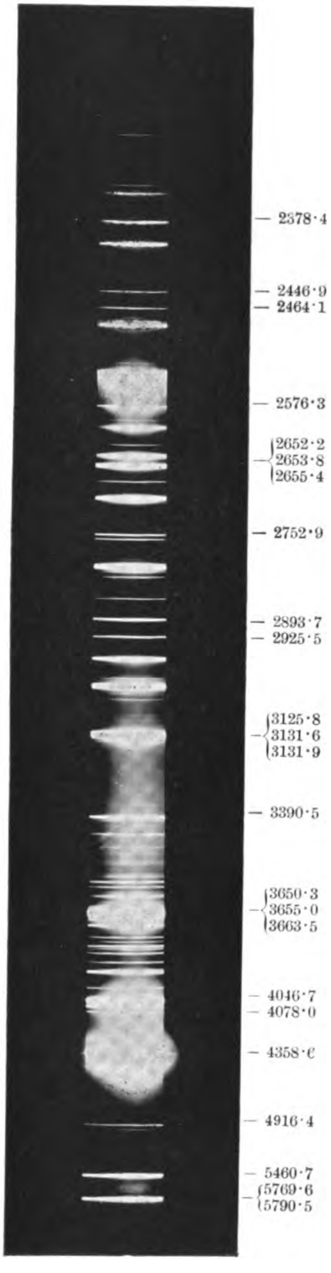

FIG. 4.—THE ULTRA-VIOLET SPECTRUM OF MERCURY.

(By courtesy of Messrs. A. Hilger, Ltd.)

extinction. When plates of suitable thickness were used the transmitted light was no longer white, but beautifully tinted, the colour of the light varying with the thickness of the plate. Thus with increasing thickness we have progressively yellow, orange, rose-red, violet, blue, and green.

These colours were shown by Biot to be due to a rotation of the plane of polarization, which increased (A) with the thickness of the plate, (B) with change of colour from red to violet. It is therefore impossible when a beam of polarized light has passed through a quartz plate to extinguish all the colours simultaneously.

The tints which Arago observed were due to the selective extinction of light of different colours by the mirror which he used as an analyser. This selective extinction may be shown by inserting a direct vision spectroscope in front of the apparatus: the plate which produces the pale yellow colour has rotated the violet light through 180°, so that it is extinguished exactly as if no quartz plate were present; the yellow tint is the complementary colour to that extinguished. As the thickness of the plate increases, the same effect is produced with light of longer wave-length; as the extinction moves from violet to red the complementary colour changes from yellow to orange, red, blue, and green. When the bright yellowish-green is extinguished a grey "neutral tint" is produced which is extremely sensitive to small rotations of the plane of polarization, and was at one time used very largely in polarimeters illuminated with white light.

When the mercury arc is used as a source of light, the colours are mainly two—green and violet; but the violet colour, especially, is so beautiful that I cannot refrain from showing you a few of Spottiswoode's experiments as they appear when the light of a mercury arc is substituted for the white light which he used in illustrating his Friday evening discourses.

When monochromatic light is used—as, for instance, when a green screen is placed in front of the mercury arc—the light can be extinguished completely even after it has passed through a very long column of quartz. Using green light purified by a spectroscope and rods of quartz cut from a crystal of extraordinary beauty, I have obtained a perfectly sharp extinction with a column of quartz half a metre in length, giving an actual rotation of 12,789·20° ± 0·01°. I have also been making experiments with the same material to determine accurately what rotation is produced by quartz in light of different wave-lengths, not only in the visible spectrum, but also in the infra-red and ultra-violet regions; but as the work is still incomplete, I will not attempt to describe it, but pass on at once to other ways in which rotatory polarization may be produced.

Three years after his discovery of rotatory polarization in quartz, Biot was astonished to find that the same property was possessed by certain liquids, turpentine and laurel-oil rotating the plane of polarization to the left, and oil of lemon and camphor (dissolved in alcohol) rotating it to the right.

In the case of quartz, Biot had attributed the rotation of the plane of polarization to the crystalline structure of the material.* The correctness of this view was proved when it was shown that rotatory polarization no longer took place when the crystalline structure of quartz was destroyed by melting it or by dissolving it in alkali. In the case of liquids this explanation was no longer possible. Rotatory polarization must here be attributed to some lack of symmetry in the structure of the molecule rather than of the crystal. It is in such cases that the polarimeter has proved its supreme value in the investigation of molecular structure. In this connexion it will be sufficient if I refer to the classical researches of Pasteur, van't Hoff, and le Bel, and to the brilliant contemporary work of Pope, Kipling, Smiles, and Mills in our own country, and of Meisenheimer and Werner on the Continent. In each of these investigations the development of "optical activity" has been accepted as a conclusive proof of molecular asymmetry, and no firmer basis for theories of molecular structure has yet been found than that which rests upon the use of the polarimeter to detect rotatory polarization.

I should have liked, if time had permitted, to refer to Faraday's discovery of the rotatory polarization induced in inactive substances, such as glass and water, by exposing them to the influence of a powerful magnetic field. I had also hoped to be able to demonstrate this phenomenon in one of the pieces of heavy glass prepared by Faraday himself, using for this purpose the large electro-magnet employed by the late Sir William Perkin in his investigations. But a passing reference to this third method of producing rotatory polarization is all that is possible to-night.

C.—MUTAROTATION.

In 1846, thirty years after Biot had discovered that rotatory polarization might occur in liquids as well as in crystals, a remarkable discovery was made by the French chemist Dubrunfaut in reference to the rotatory power of aqueous solutions of grape-sugar or glucose. Dubrunfaut found that by using freshly-prepared solutions of the sugar he could observe a transient rotatory power which was twice as great as that observed in solutions which had been prepared a few hours previously. To this remarkable phenomenon he gave the name Birotation.

The same phenomenon, which is now generally known as Mutarotation, has since been observed in the case of nearly all the

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* In associating rotatory polarization with double refraction, he was wrong, as crystals of sodium chlorate, which show no double refraction, are still capable of rotating the plane of polarized light.

"reducing" sugars. Many explanations were given to account for so mysterious a change, but nothing in the way of proof could, as a rule, be offered in support of these suggestions. In 1890, however, Emil Fischer discovered that similar changes of rotatory power occurred when gluconic lactone was dissolved in water and thus partially hydrolysed to gluconic acid—

$$\underset{\text{Gluconic lactone}}{C_6H_{10}O_6 + H_2O} \rightleftarrows \underset{\text{Gluconic acid.}}{C_6H_{12}O_7}$$

He therefore suggested that a similar explanation might be given of the mutarotation of glucose, thus—

$$\underset{\text{Glucose}}{C_6H_{12}O_6 + H_2O} \rightleftarrows \underset{\text{Glucose hydrate.}}{C_6H_{14}O_7}$$

MUTAROTATION OF NITROCAMPHOR.

In 1896 a happy accident led me to the discovery that very marked changes of rotatory power occur in freshly-prepared solutions of nitrocamphor. But, unlike the case of glucose, these changes could be observed in a large range of solvents. The change varied greatly in the numerical values involved, but was always in the same direction—from left towards right.

The cause of the mutarotation was not difficult to discover. It could not be due to hydration, nor indeed to any direct chemical action of the solvent, but must be attributed to some change of structure in the molecule of the nitrocamphor itself. In view of the fact that the nitro compound is able to simulate the properties of an acid, giving rise to strongly dextrorotatory salts, there could be little doubt that the change of rotatory power was caused by a partial conversion of the nitrocamphor into its acidic form—a conversion which can be rendered complete by the addition of alkali. This view was immediately confirmed by the discovery of a dextrorotatory anhydride, which could be prepared from nitrocamphor merely by evaporating its solutions on a water-bath.

This interconversion of isomeric compounds, which we have called Dynamic Isomerism, could also be used to explain the mutarotation of glucose, of which two isomeric forms are known; but there is good reason to believe that the hydrolysis suggested by Fischer is also an important factor when aqueous solutions of the sugar are under consideration.

In the case of bromonitrocamphor two isomeric forms of the substance can actually be isolated, thus affording direct evidence that the mutarotation observed in the case of this compound is due to a reversible isomeric change.

FORM OF THE CURVES.

In most cases the change of rotatory power proceeds according to a very simple law, the rate of change being directly proportional to the distance still remaining to be traversed.

But I have recently found a number of cases in which the curves are far more complex. In such instances it is necessary to assume a series of successive isomeric changes; but this assumption presents no difficulty, as the substances in question can all be formulated in at least five different ways.

ACCELERATION BY CATALYSTS.

The mutarotation of glucose is accelerated to a moderate extent by acids and very largely indeed by alkalis. Similar observations have been made in the case of nitrocamphor. Piperidine added to a solution of nitrocamphor in benzene produces a remarkable acceleration which can be detected even at a concentration of N/10,000,000, i.e. 1 part in 100 million or 1 centigram per ton. Aniline is 100,000 times less active.

ARREST OF ISOMERIC CHANGE.

The fortunate selection of chloroform as one of a series of solvents led to the discovery of one of the most important facts that has come to light in the course of fifteen years' work on mutarotation. In the very earliest stages of the work it was found that solutions in chloroform behaved in a very irregular and surprising way; the mutarotation in this solvent seemed sometimes to "hang fire" until set going by some accidental stimulus. These observations were evidently important as proving that isomeric change was not spontaneous, even after the nitrocamphor had been dissolved. But for ten years no explanation was forthcoming to show why this phenomenon was observed in chloroform, and in chloroform only. About five years ago, however, an arrest of isomeric change was again observed in the case of chloroform solutions to which a trace of acid had been added. These solutions (the rotatory power of one of which "held up" absolutely during twenty-four days) acquired a pungent and horrible odour, and had evidently undergone marked decomposition. It was not long before the odour was recognized as being due to carbonyl chloride—a well-known and (in anæsthetic chloroform) a dangerous impurity, formed by oxidation of the chloroform according to the equation $CHCl_3 + O = COCl_2 + HCl$. This substance has the property of attacking ammonia and organic bases such as piperidine and converting them into neutral ureas, as shown by the equations:

$$COCl_2 + 2NH_3 = CO(NH_2) + 2HCl$$

$$COCl_2 + 2NC_5H_{11} = CO(NC_5H_{10})_2 + 2HCl.$$

The next step was obviously to try to arrest the isomeric change by the addition of carbonyl chloride to a solvent which did not naturally contain it. This was done with marked success. A solution of nitrocamphor in purified ether showed a change of rotatory power extending over about a day; by the addition of carbonyl chloride the period was increased to eighteen days in a glass vessel, and to sixty-one days when a silica vessel (free from alkali) was used to contain the solution. In the case of benzene, to which acetyl chloride was added, the period was increased from sixteen days to sixty-four days in glass, and to two years in a silica vessel. Finally, by the addition of carbonyl chloride to a solution of nitrocamphor in benzene contained in a silica vessel the period was increased from sixteen days to six years.

ACTION OF LIGHT.

A convenient method of studying the effect of light on isomeric change has recently been devised in which the polarimeter plays a leading part. The solution to be studied was enclosed in a silica tube, surrounded by a silica water-jacket, and exposed to the light from a silica mercury lamp. In seven cases out of nine, however, no acceleration whatever could be detected as a result of this extremely powerful "insolation."

I have attempted to give some account of a few instances in which polarized light has been applied to the solution of chemical and physical problems. In each case the observations have taken the form of measurements of rotatory polarization. Measurements such as these have supplied to the chemist a key which has enabled him to unlock the strong-room in which many of the secrets of molecular structure were stored. The physicist, too, following in the footsteps of Faraday, has found in rotatory polarization a link between the sciences of magnetism and optics, and has obtained valuable hints as to the way in which light is propagated through matter. The hundred years which have elapsed since Biot announced his great discovery have therefore served only to enhance its brilliance, and to reveal it as one of the most illuminating disclosures even of the splendid period in which it was made.

[T. M. L.]

WEEKLY EVENING MEETING,

Friday, April 25, 1913.

His Grace The Duke of Northumberland, P.C. K.G. D.C.L. LL.D. F.R.S., in the Chair.

Professor John Garstang, B.Litt. D.Sc. M.A. F.S.A.

Meroë:

Four Years' Excavations of the Ancient Ethiopian Capital.

On behalf of the University of Liverpool, and aided by the support of private benefactors, the lecturer has been at work for four years in scientifically uncovering the ruins of the once famous Ethiopian capital. When his first expedition arrived upon the scene, there was little to suggest the great extent and interest of the city which has now come to light, in fact only one wall and a few objects of sculpture were visible above the soil. Now, however, a number of temples, palaces and public buildings have been laid bare; the walls of the Royal City have been traced; and during the past season's work, from which the lecturer has just returned, a considerable portion of this enclosure has been excavated so that a visitor may enter by the city gate and walk along the ancient streets, turning right or left at will into the different buildings.

First amongst the greater buildings of the site is the Sun Temple, which is designed in a series of ascending ambulatories with stone-built cloisters, the sanctuary being found on the highest platform, in the middle. A contemporary representation of the building upon its own walls has enabled Mr. W. S. George, the able architect of the expedition, by comparison with actual measurements, to attempt a reconstruction. In character and situation this temple corresponded to the "Table of the Sun" mentioned by Herodotus. An even larger building is the Temple of Ammon, the main axis of which is 430 ft. in length; the high altar and the special enclosure for sacrificing animals, and other interesting features of the temple, are well preserved. Other monuments excavated include an extensive palace presumed to be of Roman period, two small temples, one of which was dedicated to a Lion-deity, an ancient temple of Isis, later reconstructed, pottery kilns of Meroitic times, and several hundred tombs of the necropolis. All these features appear to have been outside the chief or royal enclosure, and it appears that there is still untouched by the excavators' spades a much larger area than has yet been attacked, including the ancient township itself which abutted against the walls of the royal city. The explorer is of the opinion that without a substantial increase in the annual sum available for this work, which up to the present has been almost entirely privately contributed by a few generous benefactors, it will be hardly possible to complete the undertaking even in ten or fifteen years.

For the last two seasons the excavation has been almost entirely concentrated upon the Royal enclosure, in which remarkable discoveries have been made. In one of the royal palaces a hoard of gold treasure and ornaments was found; and the royal baths adjacent, which are on an extensive scale, illustrate in their details the character of the Meroitic arts better than any other features of the city.

Under the threshold of another public building, carefully buried in sand, amid the debris of a building, there was found a beautiful bronze head of Augustus, which is now permanently deposited in the British Museum. A short distance from the spot are the remains of a small temple of Roman style; and the lecturer believes that this bronze head of the divine emperor had once formed the cult object in this temple. Two passages from Pliny seem to have been overlooked by those who have discussed the possibility of a Roman occupation at Meroë. From these it would appear that the imperial soldiers under Petronius had not only reached Meroë, but had passed up the Nile a further 100 miles. During the past winter a bronze coin of Augustus and an increasing number of small objects were discovered, all of which tend to indicate that, for a brief time at any rate, Roman troops actually occupied the city. In this way the fact and circumstances of the discovery of the bronze head would be satisfactorily explained. When Augustus commanded the Roman troops to withdraw, the head was removed from the temple and carefully buried out of danger of violation.

Two main culture periods are traceable in the history of Meroë previous to the Roman occupation. The first was that of its foundation under King Aspelut and his contemporaries, about the seventh century B.C. In this period Egyptian influence in art is freely apparent. The second phase began with an influx of Greek ideas, which may be roughly dated to the third century B.C., corresponding to a record by the historian Diodorus of great reformations instituted by Ergamenes, who had himself been educated in Greek thought in the schools of Alexandria. It is the second phase which is the most striking in the history of Meroë, and most of the visible buildings and monuments of the site belong to this period. The Roman occupation left little permanent impress upon the civilization of the locality, but previous and subsequent to the Expedition of Petronius there must have been already some influence of Roman contact, which manifests itself in various ways.

Thereafter the history of Meroë became that of a local and somewhat barbarous civilization, reflecting only faintly the Greek and

Roman culture with which it had been earlier infused. A record of the fourth century A.D. tells us how it was sacked by a King of Axum; but as late as the seventh century it would appear that invaders from the same district (Eritrea) overran the city and threw the statues and pictures of the gods into the river.

Previous to the discourse there will be exhibited some painted vases of Hittite fabric from Northern Syria, and a series of Ethiopian antiquities, including ceramics, jewels, etc., discovered during the course of the lecturer's excavations at Meroë.

[J. G.]

ANNUAL MEETING,

Thursday, May 1, 1913.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1912, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Thirty-nine new Members were elected in 1912.

Sixty-three Lectures and Twenty Evening Discourses were delivered in 1912.

The Books and Pamphlets presented in 1912 amounted to about 242 volumes, making, with 737 volumes (including Periodicals bound) purchased by the Managers, a total of 979 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.

Treasurer—Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.

Secretary—Alexander Siemens, M.Inst.C.E. M.I.E.E.

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A. Henry Savage Landor.
Carl E. Melchers.
Charles E. S. Phillips.
William N. Shaw, M.A. LL.D. D.Sc. F.R.S.
Harold Swithinbank, J.P. F.R.S.E.
Arthur J. Walter, K.C. LL.B.

WEEKLY EVENING MEETING,

Friday, May 2, 1913.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

HENRY GEORGE PLIMMER, ESQ., F.R.S. F.L.S. F.R.M.S. M.R.I.

Blood-Parasites

[ABSTRACT.]

YOU will remember that Mephistopheles, when he insists upon the bond with Faust being signed with blood, says, "Blut ist ein ganz besonderer Saft" (Blood is a quite special kind of juice.) Goethe would probably not have used the word "Saft" had he been writing 'Faust' to-day instead of in 1808, for at that time the cellular elements of the blood—although they had been seen and described by Leeuwenhoek in 1686—were believed to be optical illusions, even by so distinguished a person as the Professor of Medicine of that time at the Sorbonne. The incredulity of scientific men as to what they see is proverbial and astounding, fortunately; but it is probably because Science is really quite sure of nothing that it is always advancing.

I have the privilege this evening of trying to show you the barest outlines of our present knowledge of the parasitology of the blood. It is a subject of great practical and economic importance, as many grave diseases of man and beast are caused by these parasites, which, on account of their minuteness, enormous numbers, and very complex life-histories, are very difficult to eradicate or to deal with practically. On this account there is a good deal of the enthusiasm of the market-place mixed up with this subject, which, although a new one, has advanced with great rapidity, and has revolutionized pathology, and medicine as far as possible. From our point of view it began in 1880 with the discovery by Laveran, in the military hospital of Constantine, of the parasite which causes malaria. This caused the protozoa, to which order most of these parasites belong, to oust bacteria from the proud position they then occupied of being the cause of all the ills we have to bear, and to reign in their stead; not an altogether desirable change; for when you have seen what I shall show you, you will agree with me that sufficient unto life is the evil thereof. It has had all the disadvantages of a new subject, and since that time floods of work have been poured into Journals, Annals, Proceedings, etc., some of it of the best, with much of it that is indifferent, temporary, and bad; so that at times it seems as if this branch of science were in danger of being smothered in the dust of its own workshop, or drowned in the waters of its own activity. We do not, nowadays, keep our ideas and scraps of work to ourselves until they are either established or, as is more likely, dissipated, so we have a huge mass of what is called "literature," filled with many trivial, fragmentary and doubtful generalizations, many of which we have with pain and trouble to sweep into the dust-bin: Nature's blessed mortmain law taking too long to act. You remember Carlyle complained—to use a mild term—of Poggendorff's 'Annalen,' and I feel sure that, if he had had to study blood-parasites now, he would have said that it was a much over be-Poggendorffed subject. Blood-parasites are afflicted, too, with terrible names, and with large numbers of them; some have as many as ten or even fifteen different names, perhaps on the Socratic principle, that naming saves so much thinking. And they are in Latin, too, so that the terminology of this subject is a perfect museum of long Latin and hybrid-Latin names. The terminology generally of our later biology is, as one has said, "the Scylla's cave which men of science are preparing for themselves, to be able to pounce out upon us from it, and into which we cannot enter." This will be my excuse if I should use words you do not understand.

I will just remind you of the structure of the blood, that it consists of an extraordinarily complex fluid—the plasma—which holds in suspension living cellular bodies, called cells or corpuscles. These are of two kinds, red and white corpuscles. The red are by far the more numerous, and in man there are about 5,000,000 of them to a cubic millimetre of blood, but this number varies enormously under the influence of parasites. To these red corpuscles is due the red colour of the blood, and they are the carriers of oxygen, acquired by the aeration of the blood in the lungs, to the tissues. We breathe in order that they may breathe, for we only care about oxygen in so far as they care about it.

The other kind of corpuscles are the white, or leucocytes, and of these in health, there are about 7500 per cubic millimetre. A few years ago it was enough to know that there were red and white corpuscles, but now we have to know more. Through the work of Ehrlich we know that there are at least five different kinds of leucocytes in normal blood, which I will just indicate to you.

(1) Lymphocytes.—These are the smallest cells, and contain a relatively very large nucleus.

(2) Large Mononuclears.—These are large, and are called macrophages, as they possess the power of being able to absorb and digest parasites and other foreign bodies.

(3) Polynuclears.—These are characterized by the irregular, moniliform aspect of their nucleus, and they are called microphages for the same reason that the large mononuclears are called macrophages. Both of these are also called generally, phagocytes, on account of their power of ingesting and digesting foreign bodies.

(4) Eosinophiles.—These are characterized by a bilobed nucleus, and by granulations which colour deeply with eosin and other acid colours.

(5) Labrocytes or Mastzellen.—These are rare, and are characterized by large granulations which stain with basic colours.

In parasitic diseases these corpuscles are profoundly modified and altered, numerically and morphologically, and other new elements may make their appearance in the blood.

The blood is essentially the same in all animals, but it varies within certain limits. For instance, the red corpuscles are not of the same size and shape in every animal, and in birds and fishes they are nucleated; in us they are only nucleated in fœtal life and in disease. The mononuclear and polynuclear leucocytes are really separate organisms living in us, and they have qualities which it is very difficult to call anything else but consciousness; so that it is a subtle distinction to draw the line between the parasites—which these leucocytes are, in a way—which are part of us, and those that are not. When the balance of power is well preserved amongst our leucocytes, when they are working well together, then all is well with us; if we are ill, it is because they are quarrelling with themselves or with an invader, and we send for Sir Almroth Wright to pacify or chastize them with his vaccines.

So that, as Darwin said: "An organic being is a microcosm, a little universe, formed of a host of self-propagating organisms, inconceivably minute and numerous as the stars in heaven"—as we ourselves are but parts of life at large.

The three main functions of blood are: that it is a means of respiration, a means of nutrition, and a defence against invading organisms.

And now to these latter. A blood-parasite proper is a living being, vegetable or animal, passing part or the whole of its existence in the blood of another living being, upon which it lives, this being obligatory and necessary to its life-cycle.

It was in 1841 that the first blood-parasite was seen by Valentin in the blood of a fish, and two years later Gruby gave the name Trypanosoma to an organism he found in the blood of a frog. But since Laveran's discovery of the malarial parasite in 1880, we have learnt to differentiate many other parasites as causal agents of such diseases as I shall mention later in connexion with the various parasites. But we know as yet dangerously little about most of them, so that we have strenuously to resist the temptation to make our account of them sound too harmonious, before we have found half the notes of the chord we are trying to play. We speak, as it were, with authorized uncertainty, and there are parts of our science which, after all, are only expressions for our ignorance of our own ignorance. These parasites have a very complicated life-history; part of their life-cycle is passed in the blood of man or beast, and part in various parts of the body of some blood-sucking invertebrate, such as a fly, mosquito or tick, which transfers the parasite to another animal whilst feeding from him. It was thought formerly that blood-parasites would be a restricted order, but the work of recent years has shown that they have an enormous distribution both geographically and as regards their hosts. For instance, during the last five years I have had the opportunity of examining all the animals (in the large sense of the word) which have died in the Zoological Gardens. I have examined the blood of over 8000 animals, coming from all parts of the world, and I have found parasites in the blood of 587 of them, that is in about 7 per cent, and in 295 species of animals I have found them for the first time. I mention this just to give you some numerical idea of their occurrence and distribution.

It will be better to take first those parasites which live in the plasma, and then those that live in the corpuscles, rather than to attempt to take them in their at present rather uncertain biological order; and I will begin at the bottom, biologically speaking, that is with the bacteria which are plants. These only require mention, since they do not live in the blood as parasites proper, but only as accidental parasites—that is, parasitism is not necessary to their life-cycle; they get into the blood in the later, or in certain, stages of certain diseases.

An example is the blood of a Senegal turtle dove which died in twenty-six hours from fowl cholera. This bacillus was discovered by Pasteur, and is interesting, as it was his work upon it which led to his discovery of the attenuation of a virus, and of its transformation thereby into a protective vaccine.

The first parasites proper I shall mention are the Spirochetes. These have at present rather an insecure position in our idea of Nature; they were formerly classed close to the bacteria, but now they are placed tentatively among animals, and they are not yet quite sure of their place. But they, nevertheless, although insecure of their place in the books, produce grave diseases, such as relapsing fever, tick fever of man, the spirochetoses of horses, oxen and birds, syphilis, and yaws. They, with the exception of the last two, are carried by, and developed in, ticks and bugs; and in tick fever the parasite is also found in the nymph form of the tick, and this is one of the rare instances of heredity of a parasite.

The spirochete of relapsing fever in man was discovered by Obermeier in 1868, and he died from inoculating himself with the blood of a patient with the disease. He was one of the first scientific martyrs; he established our knowledge of the cause of this disease at the expense of his own life.

We will now take a long jump to the Filariæ. These are nematode worms, the embryo forms of which live in the blood; the parent forms, being too large to get through the capillaries, live in many other parts of the body. The larval form lives in the body of some invertebrate—in a few known cases in a mosquito, or in a crustacean. The microfilariæ were discovered by Demarquay in 1863. Many of them show a remarkable periodicity, some appearing in the blood at an exact hour at night, and some in the day, for which phenomenon there is at present no satisfactory explanation.

Some are short, and some long, and some are encapsuled, others not. Filariæ cause various diseases, probably elephantiasis, and certainly enormous varicosities of the lymphatics, chyluria, chylous dropsy, Calabar swelling, and certain tumours.

We now come to the Trypanosomes. They are flagellated organisms, which are the cause of many deadly diseases in men and animals; such as sleeping sickness, nagana (or tsetse-fly disease), surra, mal-de-caderas, dourine, and others. They are transferred from animal to animal by biting flies, fleas, lice and leeches, in which the sexual part of their life-cycle takes place. The first one was seen in the blood of a frog by Gluge in 1842.

A type example is T. Lewisi in the blood of a rat. This was discovered by Lewis in 1878, and is found in about 25–29 per cent of wild rats. Some die, but most recover and become immune; it is a very specific parasite, and cannot be transferred to any other kind of animal.

The T. Brucei, causing nagana or tsetse-fly disease, probably exists in the wild game of South Africa, much as the T. Lewisi does in the wild rats, but when it is carried by the tsetse-fly to domesticated animals it kills them one and all in enormous numbers.

The T. Gambiense, which causes sleeping sickness, was first seen by Dutton in 1902, and is carried by another species of tsetse-fly.

Nature attempts to fight against these invaders by phagocytosis. The parasites, however, multiply so rapidly that this method of attack is not very effectual; it can only be so in very early infections, and probably it then often is, that is, before the parasite has had time to start dividing. At the present time the question of trypanosomosis amongst man and animals is, for many countries which have colonies, of the greatest economic importance, so that a great deal of work has been done in the attempt to find a cure. A great many drugs, new and old, have been tried, and some good has been done. The first drug which was found to be of service was arsenic, first in simple and then in complex combination, and the Sub-committee of the Royal Society, formed for the purpose of supervising experiments in this direction, suggested the trial of antimony in these diseases, on account of its near chemical relationship to arsenic.

This has given better results than arsenic, and a commission is at present at work in Africa, in the Lado district, trying its effects on a large scale. We found that the salts of antimony were too rapidly eliminated from the body to be successful in the larger animals and man, and so we devised a very finely divided form of the metal itself which we put directly into the circulation, and this has given, so far, the best results. The leucocytes eat it up and transform it slowly into some soluble form, taking, in a horse, for instance, four days to dispose of one dose, and the effect of this is much more profound and lasting than that of the salts. But some trypanosomes always escape, since one dose is never sufficient for cure. In rats with nagana, in which the trypanosomes by the fifth or sixth day may number 3,000,000 per cubic millimetre of blood, the minimum number of doses for cure has been found to be four, and with this dosage it is possible to cure 100 per cent of rats. So there is still some hope.

It is interesting in this connexion to remember what Bacon, whose death, you know, was due to an experiment he undertook to prove the preservative action of intense cold upon animal bodies, says, "Laying aside therefore all fantastic notions concerning them, I fully believe, that if something could be infused in very small portions into the whole substance of blood . . . it would stop not only all putrefaction, but arefaction likewise, and be very effectual in prolonging life." His vision was prophetic!

The bird trypanosomes are very much larger than the mammalian variety, are very dense, and move much more slowly.

An example of an organism very closely allied to the trypanosomes which is only found in fishes' blood, and is called a Trypanoplasma, has two flagella, and the micro-nucleus is very large. They are probably transferred by leeches, but very little is yet known of them.

There are other flagellated organisms which may appear in the blood and live there as accidental parasites. There is a kind of inflammation of the intestines in reptiles (in the large sense) which causes the mucosa of the intestine to become permeable, so that some of the organisms which live in the intestine are able to get into the blood and live there. The only mention of these organisms in the blood is by Danilewsky, who in 1889 found hexamitus in the blood of a frog and tortoise. When in the blood they appear to excite a general œdema and ascites. I have found them now in nine cases. These are interesting as showing the power of adaptation to new surroundings possessed by these parasites.

I now come to the intracellular parasites.

Schaudinn thought that the bird trypanosomes had an intracellular stage, and if this were so they would form a bridge between the extracellular parasites, of which I have shown you types, and the intracellular parasites we are about to consider. But Schaudinn seemed, with his very brilliant attainments, to want a little more ballast of medical earth-knowledge. His work on this point has not been confirmed, and he was probably misled by a double, or even treble infection, so that we must think of these intracellular parasites as quite distinct from the others.

I will take first the Plasmodium præcox, the cause of the malaria in birds, as this parasite is of great historical interest; for it was Ross's work on this organism and his discovery of the rest of its life-cycle in the mosquito, which enabled him—on account of the great likeness between this and the parasite causing human malaria—to deduce from the one the etiology of the other, which was confirmed by Grassi and others. The Plasmodium præcox is, in many stages, so like human malaria that it can only be differentiated by the presence of the oval nucleus of the bird's red corpuscles. The life-cycle is very complex, part taking place in the blood of the bird, and another part (sexual reproduction) in the body of a mosquito. This parasite was first seen by Grassi in 1890; it is very widely distributed, and is very deadly to birds.

Human malaria has been known for centuries. Varro, who knew a good deal about what we should now call hygiene, more than a century B.C., thought that malarial fevers were due to invisible animals, which entered the body with the air in breathing, and Vitruvius, Columellus and Paladius were of the same opinion. Now we know that the mosquito is again the carrier, and that the sexual part of the parasite's cycle takes place in it, but whether the mosquito alone can account for all the phenomena of malaria is not yet quite certain.

There are three varieties of malaria in man—the tertian, quartan, and quotidian; in the tertian the cycle of the parasite in the body takes forty-eight hours, and in quartan seventy-two hours, and in pernicious malaria the fever is very irregular, but continuous. Whether there are three different parasites, or only one, which is altered according to its environment of host, climate, etc., is still apparently uncertain. Laveran and Metchnikoff believe in the specific unity of the parasite, whereas some observers want as many as five different species.

Just as in human malaria the pernicious form is distinguished by the elongated form of its gametes, so in birds there is a parasite which is distinguished, in the same way, from Plasmodium præcox by its very elongated gametes. This parasite is called Hæmoproteus Danilewski. Its development is unknown; it begins as a tiny irregular body in the red corpuscles of the bird, then it grows in the long axis of the cell and turns round the end of the nucleus. It is possible in these parasites to follow the process of impregnation, which normally takes place in some insect. By taking the blood when full of the long, fully-grown gametocytes, and keeping it for a time outside the body, this process can be followed.

First of all, the gametocytes escape from the blood-corpuscles and roll themselves up into a ball. Some of these remain quiet—the females, curiously, the macrogametocytes—whilst in the microgametocytes active movements are seen; then tailed processes are seen projecting from its surface, which at last get free and wander about in the blood, this constituting the origin of the microgametes from the microgametocyte. They then find a macrogamete, and penetrate into it and fertilize it. This fertilized macrogamete then alters its shape and becomes an oökinete, with the remains attached containing the pigment. It may enter a red corpuscle, but it usually breaks up, because it finds it is not in the stomach of the insect it intended to be in, but between two pieces of glass.

From Hæmoproteus it is easy to pass to a rare and undetermined parasite of the blood of birds called a Leucocytozoon. It occurs in the blood in the form of a long, spindle-shaped, unpigmented body. Very little is known of it except that it is found in its sexual forms. The earliest observers of this parasite—Danilewsky and Ziemann—believed the host-cell to be a leucocyte (hence the name), but Laveran has shown that it is a red corpuscle.

We now come to a group of parasites of great practical importance, the Babesias, formerly called Piroplasma, which are the cause of Texas fever or red-water fever, malignant jaundice, East Coast fever, and biliary fever amongst domestic animals. We know, again, little that is certain concerning this group, except that they are unpigmented parasites of the red corpuscles, and are carried by ticks. They are the most destructive to the blood of any we know. In an ox, I have seen the red corpuscles decrease from 8,000,000—the normal—to 56,000 per cubic millimetre in two days.

Another important group, the Leishmania, is still uncertain of its exact position. In the body they occur as small bodies with a nucleus and micro-nucleus, but when cultivated on artificial media they become flagellated organisms of a herpetotomas type. It is not quite certain what insect plays the part of carrier, but the different varieties of this group cause the diseases known as Kala Azar or tropical splenomegaly, Oriental sore, Delhi boil, Biskra boil, etc., and also infantile splenic anæmia.

The last class are the Hæmogregarines. These are parasites of the red corpuscles of reptiles principally, but they have been described in mammals and birds. We only know certain stages of the greater part of them; they are large, sausage-shaped bodies, not pigmented, and they are supposed to be carried by leeches, ticks, lice, and fleas. They generally have a capsule. In some instances the host-cell is enormously enlarged and entirely de-hæmoglobinized, but in most cases the host-cell is not enlarged.

I have now taken you over some examples of all the known types of blood-parasites, but, at best, the picture in your minds must be like that of a landscape taken from a railway carriage at full speed; and the result, I fear, only a kind of clarified confusion, but it will be something if I have succeeded in making it transparent at the edges. What must have struck you most is the smallness of our exact knowledge of many of these extraordinary organisms and the gaps that there are even in this. But the incitement to future work lies in this fact, for

> "Things won are done, joy's soul lies in the doing."

[H. G. P.]

GENERAL MONTHLY MEETING,

Monday, May 5, 1913.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Milner Brown, Esq.
Herbert Wildon Carr, Esq., D.Litt.
Emile Garcke, Esq.
J. Currie Hanson, Esq.
Henry Holloway, Esq., J.P.
Rudolph Lessing, Esq., Ph.D. F.C.S.
The Right Hon. The Duke of Rutland,

were elected Members of the Royal Institution.

The Secretary announced that His Grace the President had nominated the following gentlemen as Vice-Presidents for the ensuing year:—

Henry E. Armstrong, Esq., Ph.D. LL.D. F.R.S.
The Right Hon. Arthur J. Balfour, P.C. M.P. D.C.L. LL.D. F.R.S.
J. H. Balfour Browne, Esq., K.C.
Sir William Crookes, O.M. LL.D. D.Sc. F.R.S.
Donald W. C. Hood, Esq., C.V.O. M.D. F.R.C.P.
The Right Hon. Sir James Stirling, P.C. LL.D. F.R.S.
Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S. (Treasurer).
Alexander Siemens, Esq., M.Inst.C.E. M.I.E.E. (Secretary).

The following Resolution passed by the Managers at their Meeting held this day was read:—

> The Managers of the Royal Institution desire to record their grateful sense of the value of the services rendered to the Institution by Sir William Crookes as Honorary Secretary during the last thirteen years. His high scientific attainments have added distinction to the Office, to the duties of which he has given unsparingly of his time and attention, thereby largely contributing to the success which has attended the administration of the Institution. The Managers rejoice to know that Sir William Crookes is as a Manager to continue to give them and the Royal Institution the benefit of his assistance and advice.

The following Resolution of Condolence passed by the Managers at their Meeting held this day was read:—

> The Managers desire to record their sense of the loss the Institution has sustained in the decease on April 10, 1913, of William Flockhart, Esq., F.R.I.B.A., who was a Member since 1893, and the Advising Architect to the Royal Institution for twenty-four years; and to express their most sincere sympathy with Mrs. Flockhart and the family in their bereavement.

The Managers reported that they had received a sum of £128 14*s.* 9*d.*, being part of the Legacy of the late Miss E. S. Wolfe.

The Right Hon. Lord Rayleigh, O.M. P.C. D.C.L. LL.D. F.R.S., was re-elected Honorary Professor of Natural Philosophy.

Professor Sir J. J. Thomson, O.M. LL.D. D.Sc. F.R.S., was re-elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Accountants, Association of—Journal, Vol. VI No. 21, April, 1913. 8vo.
Agricultural Society, Royal—Journal, Vol. LXXIII. 8vo. 1913.
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  Conductivity and Viscosity in Mixed Solvents. By H. C. Jones. 8vo. 1907.
  Absorption Spectra of Solutions. By H. C. Jones. 3 vols. 8vo. 1909-11.
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  Observaciones en la Mina Aguila, 1909. 4to. 1911.
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East India Association—Journal, Vol. IV. No. 2, April, 1913. 8vo.
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  Amateur Photographer for April, 1913. 8vo.
  American Journal of Science for April, 1913. 8vo.
  Athenæum for April, 1913. 4to.
  Canada for April, 1913. 8vo.
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Philadelphia Academy of Natural Sciences—Proceedings, Vol. LXIV. Part 3. 8vo. 1913.
Photographic Society, Royal—Journal, Vol. LIII. No. 4. 8vo. 1913.
Physical Society of London—Proceedings, Vol. XXV. Part 3. 8vo. 1913.
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Thirty-third Annual Report. 8vo. 1912.
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WEEKLY EVENING MEETING,

Friday, May 9, 1913.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

FRANK BALFOUR BROWNE, ESQ. M.A. (Oxon.), F.R.S.E. F.Z.S.

The Life-History of a Water-Beetle.

THE life-history of a water-beetle can be outlined in a very few words. An egg is laid by the mother-beetle: an aquatic larva hatches out which feeds and grows, and, during the process of growth, moults several times. When full grown it leaves the water and burrows into the earth, forming a "cell," in which it changes to a pupa. After a time the pupal skin is cast off, and the perfect insect makes its way out of the cell and resumes its life in the water.

There are, however, all sorts of interesting details in the life-history, and these details often differ considerably in different types. There are differences in the egg-laying habits; differences in the method of development of the embryo; differences in the way the larva gets out of the egg; differences in the way it feeds and in the nature of its food, and so on; and it is these differences which are of importance to each species in enabling it to fit in among other species in the life of the community.

Although there are a number of widely separated species of beetles which inhabit the water, there are two groups which are usually referred to as "water-beetles," and these may be broadly distinguished as the swimming carnivorous group—the *Hydradephaga*—and the creeping herbivorous group—the *Palpicornia*, or *Hydrophilidæ*. The description of this second group is not strictly accurate, as the larvæ are, apparently without exception, carnivorous, and the perfect insects, although capable of subsisting upon a vegetable diet, in at least many cases enjoy animal food; and although they are somewhat differently constructed from the swimming water-beetles, some of them are very fair swimmers.

I propose to outline the life-history of a type of the *Hydradephaga*, and then to compare with it a type of the *Palpicornia*; and as a type of the former group I will describe a species of Dytiscus, *D. lapponicus*, whose life-history I worked out during last summer.

The male and female differ in general appearance, the former having smooth wing-cases, the latter having these grooved or fluted. The male has also a pad on each of the front legs, while the female has quite simple front legs. The slide also shows a full grown larva, and thus gives an idea of the relative sizes of these two stages of the species.

This species is extremely local in the British Islands, only having been found in a few localities in Scotland, and in one in north-west Ireland. It inhabits lochs, usually mere lochans, at altitudes of from 800 feet upward, and there are certain characteristics about its habitat which make it possible generally to tell at a glance whether a particular lochan is or is not likely to hold the species.

As a rule the habitat is a bare stony lochan, with very little vegetation; it has no stream flowing into or out of it, and trout and *lapponicus* are mutually exclusive. There are usually newts and fresh-water shrimps (*gammarus*), but otherwise there is always a marked scarcity of animal life. Very few other water-beetles are associated with *lapponicus*, which usually is abundant where it occurs.

The only place I have found the species in great abundance is in a lochan 950 feet above sea-level on the island of Eigg. Along its eastern side this lochan is strewn with large stones, and under these the beetle is to be found, often as many as four or five under one stone. It occurs in other lochans on Eigg, and has been found also in Rhum, Skye, Mull and Arran, but otherwise it is only known from Inverness-shire.

One place in Mull where it used to occur abundantly is a peculiar loch, situated in the top of a hill, about 800 feet behind Tobermory. The place looks like the crater of a volcano, but I believe is not so described by geologists. The species has apparently quite disappeared from this loch; it is probably slowly disappearing from our islands, being a remnant of the fauna which abounded when our climate was much colder than it is at present.

All my specimens came from the one lochan on Eigg, and they were placed in large tubs in my garden in the north of Ireland. The tubs are filled with water, but the bottom is covered by a thick layer of soil, and in the soil a few species of water-plants thrive, chiefly the common water-grass, *Glyceria aquatica.* The tubs are covered with wire-gauze to prevent the beetles from escaping.

Now the Dytiscus possesses a small apparatus capable of piercing the tissues of the water-plants, and each time this borer makes a hole in the water-plant one egg is deposited. In my tubs the *lapponicus* chose the water-grass as the receptacle for its eggs. In its native home this grass does not grow, the only water-plants being a common rush, a species of *juncus*, and the club rush *eleocharis*, both possessing round stems. Now, the grass possesses a round stem surrounded by leaves, each leaf consisting of a long sheathing base and a free lamina or blade. The sheath is keeled, and in every case the mother-beetle pierced the leaf-sheath, and always in the line of the keel, depositing the egg in the tissues of the sheath, and this shows the peculiar instinct possessed by the mother in the deposition of her eggs and the extreme sensitiveness of the borer or ovipositor. Although I examined very carefully the plants in the tubs, only twice did I find that the ovipositor had passed right through the sheath and dropped the egg between that and the stem.

Lapponicus, unlike our other species of Dytiscus, has a very definite egg-laying period, commencing in March and ending in June. From two of the British species I have had eggs in October, December and February, as well as in the summer months.

I collected a number of the eggs, dissecting them out of the leaf-sheaths, and placed them on wet cotton wool in tumblers and watched their development.

I do not intend to weary you with the details of the development of the embryo, but I wish to point out that the embryo first appears on a part of one side of the mass of yolk—it does not at first occupy the whole length of the egg—and it then extends first backwards and then forwards, and the sides grow up around the yolk until the embryo ultimately encloses it. The nerve-chord does not increase in length with the embryo, and consequently appears to shorten as the embryo extends in the egg.

The development of the embryo occupies about three weeks in June, but temperature affects the length of this embryonic period. In the case of another species, an egg laid in April matured in three weeks, while one laid in winter took six weeks to hatch.

Towards the end of the embryonic period the pressure of the embryo in the shell is very great. I accidentally punctured an egg with a needle when turning it over, and immediately a portion of the embryo bulged through, just as the inner tube of a pneumatic tyre tends to bulge through a tear in the outer cover. The pressure is also indicated by the changed shape of the egg during the final stages.

During the latter part of the egg-period, there are various slight movements of the embryo, but during the last few hours certain very definite movements become noticeable. In the first place, inside the head a spasmodic pulsation is visible, at first at long intervals, but later more or less continuously. I have observed this pulsation in eggs of other water-beetles, and also in those of the dragon-fly, and although I am not sure that the interpretation is the same in dragon-fly and water-beetle, I am satisfied in the latter case the pulsation is really a swallowing process.

The larvæ of all the water-beetles I have examined possess a special sucking apparatus known as a "pharyngeal pump," the use of which I shall describe directly, and in the embryo this pump apparently comes into use to absorb the fluid which surrounds the embryo in the shell; the embryo merely drinks this up.

After this sucking-pump begins to work, various other movements of the internal organs can be observed, including peristalsis, and also at infrequent intervals the whole body moves slightly in the shell, the tendency being to push the head into the end. One other movemend is to be noted, and that is an up-and-down motion of the head, at first very slight, but later becoming very marked.

On either side of the head is a small papilla, at the apex of which is a minute, slightly curved spine. When the embryo is at rest, this papilla lies in a slight depression, but when the sucking-pump is at work the papilla bulges outward, so that the spine touches the shell. Thus when the head moves up and down and the sucking-pump works at the same time, the two spines scrape along the inside of the shell and ultimately burst it open. They are, therefore, "hatching spines," and similar instruments differently situated have been observed in a few insect embryos of other orders.

You see, therefore, that the shell bursts open at the head end; immediately it bursts the compressed larva bulges out, and by slight writhing movements works its way clear of the shell, the whole operation taking less than two minutes. As soon as the larva is clear of the shell the tail straightens out, and the legs and mouth parts assume their natural position. In the embryo there is a peculiar fold in the upper part of each jaw, but within two or three minutes of the larva's escape this fold has completely disappeared.

From the moment the larva escapes it begins to grow in length and breadth. The long air-tubes in the body are flat, but have a bright silvery appearance, suggesting that some gas has been secreted in them; but the larva is heavier than the water, and therefore sinks to the bottom. For a time, half an hour or more, it rests quietly and shows no desire to get to the surface, but sooner or later it gets restless and swims to the surface, using its feathered legs as oars, and raises its tail to the surface film and remains suspended for a few minutes. After this the newly-hatched larva is buoyant, and cannot remain away from the surface without holding on to the submerged vegetation. The buoyancy is, however, only temporary, as older larvæ frequently require to swim to the surface to renew their air-supply.

In the insect, breathing and blood-circulation are normally not intimately associated as in other animals. In a human being or a fish, or even in a snail, air is taken into special organs—lungs or gills—where the blood takes up the oxygen and carries it through the whole body. In the insect the blood has usually nothing to do with the aeration of the different organs, the whole body being permeated by innumerable air-tubes.

In all the water-beetle larvæ which come to the surface to obtain their air, these innumerable air-tubes communicate with two large air-tubes which run the length of the body, one on each side, and these open on the last segment. Hence, when a larva requires to renew its air-supply it comes up tail first, bringing the openings of the two lateral tracheæ into communication with the air, and by contracting and expanding the body it exhales the used-up air and inhales fresh air.

For a day or so after hatching the larva is soft and is not hungry, but once its skin and jaws have hardened it begins to look about for food. I found that tadpoles and pieces of chopped worm were suitable food, but under natural conditions small newts, water-shrimps and insect larvæ—including brothers and sisters—constitute the normal diet. It is impossible to keep two larvæ together in one small vessel, as one invariably attacks and kills the other within a few hours. Even when I gave a tub to four specimens only one survived after a few weeks, so that in a small loch, where at least some thousands of these larvæ hatch out, the death-rate must be enormous.

The method of feeding of the larva is peculiar. The two long sharply-pointed jaws are each pierced with a fine tube, one end of which opens on the inner side just below the apex, and the other end of which opens on the upper side just near the base. When the jaws are closed the inner ends of these tubes communicate with the corners of the mouth, but when the jaws are open the inner ends of these tubes do not communicate with the mouth at all. The mouth itself is also peculiar. In a front view of the head it is visible as a long narrow slit between the bases of the jaws, but if this slit is examined it is found that across the lower side of it is a raised ridge which fits into a groove running across the upper side of it. When the jaws are wide apart the ridge and groove are separated, and the mouth is open, but as soon as the jaws come together the ridge fits into the groove, and the mouth is closed. As soon, therefore, as the larva seizes its prey its mouth is closed, and the only communication into it is through the tubes in the jaws, the basal ends of which now open into the corners of the mouth.

Immediately behind the mouth is the powerful sucking-pump, the pharynx, which I mentioned in connexion with the embryo. By expansion and contraction of its muscles it sucks in the juices of the prey through the tubes in the jaws. But if this were the whole process of feeding there would be a considerable waste, as a worm or a tadpole consists of a large amount of solid material; and yet, if one watches one of these larvæ feeding, one will find that almost nothing is left of the prey except the skin. This is due to the fact that at short intervals the sucking-pump stops working and saliva is poured into the prey. This saliva digests and dissolves away the solid parts of the food, which are then sucked in by the larva. The process of digestion, which in most animals takes place internally, is carried on in these larvæ outside the body.

With regard to the *duration of the larval period*, in my examples this varied from six to nine weeks. This period is divided into three stages, there being two moults prior to the final one which produces the pupa. Each of the first two stages only lasts about ten days, so that the last stage is a very long one, as it is in all other insects.

This last stage is also divisible into two parts, the first occupying four or five weeks, during which the larva feeds and grows as in the previous stages, the second occupying two to four weeks, being spent out of the water making a cell in the earth, and resting preparatory to becoming a pupa.

In the few cases which I had the opportunity of observing, the full-grown larva always left the water in the morning between eight and ten o'clock; but whether this is the rule with this species, or whether it was connected with the artificial conditions in which my larvæ were reared, I do not know.

Once the larva leaves the water it crawls about very actively, seeking a suitable place to enter the earth. If left to itself it usually selected a stone and burrowed underneath it, but I found that if I made an artificial burrow—with a pencil, for instance—the larva could be made to crawl into this, and as a rule would make its "cell" in it. By making such a burrow against the glass side of a box filled with earth, I was able to watch the process of the formation of the pupal cell.

Once the larva has entered and adopted the burrow, it straightway begins to prepare its cell, and this is done by enlarging part of the burrow. The jaws are now used for transporting pellets of soil from one position to another and for breaking up the pellets into their separate particles. Very little earth is actually pushed into the unused part of the burrow, the cell being formed almost entirely by breaking up the pellets of soil and battering the fine particles against the sides. The vertex of the head is the main battering-ram, but the larva, which during the whole process of making the cell lies with its tail bent over its head, also flattens out the earth with its body.

The actual making of the cell occupies about twelve hours, and during that time the larva does not rest for a moment. At the end of that time it is apparently tired out, and rests in any position, often stretched across the cell, its head pressed against one side and its curved body against the other. It thus rests for about twenty-four hours, after which it bends its tail underneath it and usually adopts a sitting-up position—reminding one of Tenniel's illustration in "Alice in Wonderland" of the caterpillar sitting on the mushroom. It is, however, very restless, and frequently changes its position, tossing from side to side.

The pupa appears, after the larva has been thus resting for a fortnight or more, by the larval skin splitting along the back and being cast off at the tail end. On its back are to be seen a number of short projecting spines, and Lyonnet suggested in the case of another pupa, similarly though better equipped, that these are for the purpose of raising it off the damp soil of the cell. This may be true, but in my experience the pupa most usually lies, so to speak, on its face rather than on its back.

The pupal stage lasts about three weeks, and the only change noticeable during that time is a slight pigmentation of what is at first a perfectly white pupa. At the end of the pupal stage the skin ruptures along the back, and the perfect insect comes forth at first white and soft, but in the course of two or three days it assumes its normal coloration, and after a longer period its normal hardness. After a week or so it makes its way out of the pupal cell by biting and scraping, and at once goes to the water.

In its native haunts it spends most of its time amongst the stones and mud at the bottom, occasionally coming up to renew its air-supply, and in my tubs also it was seldom to be seen.

With regard to its winter habits, it apparently buries itself at the bottom of the loch as soon as the cold weather begins, and sleeps until the following spring. In my tubs it disappeared completely in October or November, burrowing deep into the soft oozy mud at the bottom, and there it remained until the following March. During all this time the metabolic processes must be practically at a standstill, as otherwise the insect would require to renew its air-supply at frequent intervals.

Having now outlined the life-history of this type of the swimming carnivorous water-beetles, I will take an example of the other group, and the one I have chosen goes by the name of *Hydrocharis caraboides*. There is only one species of *Hydrocharis* in the British Islands, and it is practically confined to the south-east of England, only very occasionally having been found anywhere else in the country. It inhabits stagnant ponds and drains, and is not uncommon in a few places in Surrey, Essex, and Middlesex.

I began to experiment with it five years ago in the north-east of Ireland, having obtained my specimens from Surrey. Each year I obtained eggs, reared the larvæ, and renewed and increased my stock, so that it is obviously not the climate of north-eastern Ireland which prevents this species from being a native there.

The conditions in my tubs were just such as are to be found in any pond or drain in the country, and apparently the only reason why this species is confined to the south-east of England is that competing species prevent it from extending its range.

Whereas *Dytiscus* lays its eggs singly in holes pierced by it in the living vegetation, *Hydrocharis* builds an elaborate silken cocoon which floats in the water, and in which about fifty eggs are deposited.

The spinning of the cocoon is a wonderful process. The beetle carries on its underside a film of air, which is part of its supply for breathing. The cocoon is actually spun on a part of this film of air, which is then detached from the rest of the film as a bubble enclosed in silk. The egg-laying commences soon after the cocoon is begun, and the eggs are arranged side by side in the cocoon standing upon one end, being fastened in position by silken threads. A space above the eggs is filled with very loosely woven silk.

In closing up the cocoon a peculiar plate-like structure is formed of very closely woven silk, and this ends in an upward projection known as the "mast." The purpose of this "mast" is not known. It is not a tubular structure, but merely a band of silk. It has been stated that if it is cut off the eggs die, but in the case of another species I have hatched eggs removed from the cocoon and submerged, so that the suggestion that the mast is necessary for keeping up the air-supply is without foundation.

I shall not weary you with details of the development of the embryo beyond mentioning that, unlike *Dytiscus*, the embryo from the first occupies the whole length of the egg, and that the nerve chord, again unlike *Dytiscus*, grows with the embryo as it develops. The only other point I need mention is that in the cocoon all the embryos develop head downwards.

The egg-laying period of *Hydrocharis* extends from about the middle of May until about the middle of July in my tubs, but it may perhaps be rather longer in the south-eastern parts of England. The incubation of the egg occupies nine or ten days, and, as in the case of *Dytiscus*, towards the end the embryo is very tightly packed within the shell. There is, however, no special hatching apparatus that I have been able to find. The pulsating organ or sucking-pump in the head is visible, and there are also movements of the embryo, but at the end the skin splits along the back and the larva treads it off, giving a peculiar backward wriggle.

Now, under normal conditions the newly-hatched larva does not at once leave the cocoon, in fact it does not appear for one or even two days after hatching. As soon as it bursts the egg-shell it wriggles backwards out of the egg into the space above all the eggs, and it is interesting to note that the hairs on the body of the newly-hatched larva all point forwards. As the larvæ hatch, the empty shell and the silk bindings become broken down—I think they are chewed by the larvæ—and the whole cocoon ultimately becomes filled with the larvæ.

In those cases where I dissected the eggs out of the cocoon and allowed them to develop on the wet cotton wool, the newly-hatched larvæ congregated into a mass and remained so for a day or two, after which they became active in search of food.

You will notice that the larva possesses on each body segment a pair of lateral processes, and on the last segment a pair of ventrally placed processes of a different kind. These latter, which are possessed by all water-beetle larvæ which come to the surface for their air, have probably some connexion with raising the tail to the surface for breathing, but the hairy lateral processes have been called gills. Many larvæ of the *Palpicornia* have lateral processes, usually smaller than those of *Hydrocharis*, but in no case are they really gills, and the larvæ quickly drown if prevented from bringing their tails to the surface to renew their air-supply.

The larvæ of *Hydrocharis*, like those of the *Dytiscus*, will eat almost any kind of animal matter, and hence they are easily supplied. I fed them mostly upon chopped worms, but their method of feeding is very different from that of the *Dytiscus*. They seize their food with the jaws, antennæ, and the other mouth parts, and they then come to the surface, and, raising their heads and part of the body out of the water, they proceed to chew up the food by opening and closing the jaws, turning it from time to time with the other mouth parts. The jaws are not perforated, nor is there any mouth-lock as in *Dytiscus*, and they suck in the juices of the prey by the mouth, spitting up saliva at intervals, which actually froths over the food and digests it, the dissolved material then being sucked down. The external digestion is so complete that in the case of a thick piece of worm all that is ultimately rejected is the thin transparent outer pellicle.

In the mouth parts of the larva I want to draw your attention to a curious want of bilateral symmetry, noticeable not only in the jaws —one of which, the left, has a small extra tooth near its base—but also in the upper lip. In many species there is an absence of bilateral symmetry where a pair of organs are complementary. Thus in the jaws of the beetle itself, the base of the left one is hollowed out to receive the base of the right one, which is convex, the two being related as pestle and mortar for grinding up the food. The larva of another species of the same group also shows asymmetry of the jaws, but here again it is definitely associated with the method of feeding. This species feeds upon pond snails, and the left jaw holds the shell while the right jaw with its large double tooth cuts through it.

The asymmetry of the upper lip, however, is at present inexplicable, and curiously enough it occurs in several other species.

The larva of *Hydrocharis*, like that of *Dytiscus*, passes through three stages, the first two of which occupy from five to eight days, and the third stage, up to the time the larva is full grown, occupies about four weeks. It then leaves the water and burrows into the earth, forming a cell just as the *Dytiscus* larva did. I had many specimens of these larvæ, and so made many experiments with them, and one curious fact about them is that the instinct which leads them to burrow into the ground and make a pupal cell only lasts for one or, at most, two days. In no case, where I removed a larva even immediately after the completion of its cell, did it make any attempt to form another one, and if left on the surface of the soil it moved about listlessly and ultimately died, apparently of drought, since if placed in a damp position, for instance, in an artificial cell it survived and pupated. If a cell was damaged before completion the larva often completely destroyed it, apparently in the attempt to repair the damage, and would be found sitting amongst the ruins.

Once the cell is completed the larva rests for about three weeks, at the end of which time the skin is cast off and a greenish-white pupa appears. This is more spinose than that of *Dytiscus*; but it also prefers to lie upon its face, resting upon the two small tail projections and upon the "collar" of the prothorax.

The perfect insect appears after about ten days, so that the whole life-cycle occupies about nine or ten weeks from the laying of the egg to the appearance of the perfect insect. This time, however, may be greatly prolonged under less favourable conditions. Thus, the later egg-cocoons produce larvæ which take twelve or fourteen weeks to grow up, and the cocoons built in July produce beetles which do not leave the pupal cell for six or seven months. The larvæ leave the water in September and even in October, and after three or four weeks turn into pupæ. These pupæ turn into beetles in late October or November, but the beetles remain, apparently torpid, until the following March or April, when they make their way out and to the water.

I have mentioned that the larvæ of both *Dytiscus* and *Hydrocharis* breathe in the same manner by raising the tail to the surface. The perfect insects, however, assume very different positions when taking in their air-supply.

Dytiscus floats up to the surface tail first, taking in air between the body and the great wing-cases which cover it, and it is in this cavity under the wing-cases that the whole reserve of air is carried.

On either side of the body under the wing-cases is a row of pits, spiracles; the last pair of these are much larger than the others. When the insect rises tail first to the surface, the tubes connected with this last pair contract and expand, just as in the larva, renewing the air-supply in the whole tube system, while at the same time the body contracts and expands, renewing the reserve supply under the wing-cases.

Hydrocharis, on the other hand, comes to the surface head first, turns its head on one side and pushes its short club-like antenna through the surface-film. Now a large part of the under side of this beetle is covered with fine velvety hair, which retains a thin film of air upon it, just as a piece of velvet does when gently pushed under water. When the beetle raises its antenna above the water it brings this film of air into communication with the air above the water. It also has a reserve supply under its wing-cases, and this communicates at the sides with the ventral film, and by expansion and contraction of the body the used-up air is expelled above the water and fresh air is taken in. In *Hydrocharis* the most important spiracles are situated well forward, and thus the used air from the air-tubes is expelled and fresh air taken in at the front end of the body instead of the tail end.

Anyone who examines *Hydrocharis* and compares it with *Dytiscus* will at once see great structural differences. In a ventral view of the two types, comparing the heads, the most noticeable difference is in the antennæ, which are filamentous in the former and clubbed in the latter, and the max. palpi, which are short in the former and long in the latter, in which they are used under water as feelers, just as are the antennæ of *Dytiscus*.

Passing over other less remarkable differences in the heads of the two types and coming to the body, one at once notices the different disposition of the legs: in *Dytiscus* the first two pairs are close together, in *Hydrocharis* the three pairs are about equidistant. In *Dytiscus* the basal segment of each hind leg—the coxa marked 3*—is large, and the two coxæ are fused into a single piece which is firmly fixed into the body. In *Hydrocharis* the coxa is long and narrow; the two coxæ are separate, and each is hinged on to the body. The firm fixing in *Dytiscus* gives it a much more powerful leg-drive than the hinging gives to *Hydrocharis*, and hence *Dytiscus* is a more efficient swimmer.

These differences between the two types are therefore connected with differences in function. The antennæ of *Dytiscus* are feelers, while those of *Hydrocharis* are connected with breathing, and the disposition of the legs and their methods of attachment to the body are connected with differences in mode of progression, *Dytiscus* being a "swimmer," and *Hydrocharis* chiefly a "creeper" on the submerged vegetation.

In these two groups of water-beetles, the *Hydradephaga* represented by *Dytiscus*, and the *Palpicornia* represented by *Hydrocharis*, we have two types of adaptation to an aquatic existence. Each type has originated independently of the other, that is, they are not descended from a common aquatic ancestor. Each represents a part of a large terrestrial family, and each has probably developed an aquatic habit as a result of competition (stronger land forms having driven the weaker off the land and into the water)?

Just as each group has originated under the stimulus of competition, so, within each group, competition has moulded the different forms, and the peculiar details in the life-history of any one form are just those which enable it to retain its place in the community to which it belongs, and to hold its own in the great struggle for existence.

[F. B. B.]

WEEKLY EVENING MEETING,

Friday, May 16, 1913.

Henry E. Armstrong, Esq., Ph.D. LL.D. F.R.S., Vice-President, in the Chair.

Captain Cecil G. Rawling, C.I.E. F.R.G.S.

The Pygmies of New Guinea.

An expedition, organised by the Ornithologists' Union and assisted by the Royal Geographical Society, left England in October 1909 for the south-western coast of Dutch New Guinea, for the purpose of survey and to form collections of the fauna and flora of that district. As that portion of the island had never been previously visited, and as it lay in close proximity to the great central snow-clad range, it was hoped that many new species of mammals, birds, reptiles, etc., would be obtained. The expedition consisted of Mr. W. Goodfellow, a noted ornithologist, as leader, Mr. A. F. R. Wollaston, medical officer, botanist and entomologist, and Messrs. W. Stalker and G. Shortridge, assistant-collectors. The survey section was represented by myself and Mr. E. Marshall as assistant-surveyor and surgeon. We arrived at Batavia on December 1st, and were there joined by ten Gurkhas from Darjeeling. The Dutch authorities most kindly supplied forty Javanese soldiers and their necessary convict carriers, and transported the whole force to the mouth of the Mimika River, one of the only two known river-mouths on the south-western coast of New Guinea. Our own coolies had been recruited at Amboina, and later on when these broke down others were obtained from Boetan and Macassar. Neither the Javanese troops, their carriers, nor any of our followers, were of the stamp of man to withstand the climate of New Guinea. Our total losses by death reached 12 per cent, and those invalided out of the country 83 per cent. Of nearly four hundred men employed only eleven lasted out the expedition, and of these four were Europeans and four Gurkhas. As the Dutch have learnt from experience, no men of the Dutch East Indies, except Dyaks, are of any value as carriers in the western half of the island of New Guinea. On the morning of the 3rd January, 1910, the coast was sighted.

Beyond the water-laden mist which completely enveloped the low-lying country to the foothills of the central range, fifty miles distant, rose line upon line of knife-edged forest-clad ridges, lying east and west and parallel to the main range, which stretches from one end of the island to the other, a distance of eleven hundred miles. The western section, known as the Charles Louis Range, throws out a great promontory southward to the sea. That portion lying north of the Mimika district, formerly called the Snowy Range, and in which are to be found the highest peaks in the island, will in future be known as the Nassau Range, to the east of which again comes the Orange Range with its high point of Wilhelmina Peak.

The Nassau Range rises steadily from the Charles Louis Range in the west, culminating on the east at its highest point, known as Carstensz Peak, 15,964 feet. On our arrival the whole range lay bare of cloud, the great glaciers of Carstensz clearly visible, together with three other snow peaks of slightly less altitude lying directly behind. To the west of Carstensz stretched a great snowfield, divided by a small rift from a three-peaked snow-clad mountain known as Mount Idenburg (15,379). Still falling slightly, but to the eye hardly noticeable, next came Mount Leonard Darwin (13,822), the south side of which forms the greatest sheer fall of any precipice in the world, but of this I will speak later. The snow-line is at about 14,500 feet. As the mist drifted and vanished an attempt was made to locate the Mimika, always a difficult task, for the mouth of one river is exactly like another, each having a group of casuarina-trees on either side of the entrance. Close steaming is an impossibility, for, on account of the extreme shallowness of the water, no ship drawing over twelve feet can approach nearer than two or three miles of the shore. With the assistance of natives who came off in crowds to intercept the ship, and to whom the one word "Mimika" was sufficient guide, we were enabled to cast anchor off the river late in the afternoon of that day. The weather was fine, and the disembarkation of the men and stores was safely carried out during the three following days. This was fortunate, for the coast, on account of the rapidity with which storms arise, is a particularly dangerous one. From the middle of January to the middle of March, and again from the first week in June to the middle of October, the chances are very much against a landing being accomplished. During the former period the west monsoons are blowing, and during the latter period the east monsoons. In addition a powerful tide is always running, and the bars across the rivers are continually changing. Every river has a bar, many uncrossable except at high tide, even for a launch. The Mimika always had five or more feet of water available. The tides apparently followed no law. Sometimes there were two tides of equal height, at other periods only one tide in the twenty-four hours, a great rise and a great fall often of fifteen to twenty feet, though by careful watching it was observed that there was a second tide of from six inches to a foot. The same trouble has been found down the coast. During a stay of three weeks at the mouth of the Mimika in the month of December, high tide, and there was only one notable rise in the day, took place always between five and nine o'clock in the morning.

The natives, men of splendid physique but of brutal countenance, showed great joy at our arrival. Only on one previous occasion had they seen white men, and then only for an hour or so, but the trade they had obtained must have been such as to have made them long for the advent of the, to them, peculiar race who were willing to exchange steel axes for stone ones. Not until we had proceeded five miles up the river was a spot found upon which the base camp could be built.

The only available ground on the right bank was occupied by Wakatimi village, the site chosen for the camp consisting of a peninsula formed by the bend of the river facing the village. This we then believed was well above any possible rise of the Mimika, but such proved not to be the case, for in June the whole peninsula was flooded for three days to a depth of from two to six feet. The Mimika is tidal for six miles above Wakatimi, the natives shifting their temporary villages from river to river according to the amount of water descending from the hills.

Natives poured in to inspect us from all parts, showing no fear, but only a keenness to trade. Anxious as they were to obtain cloth, beads, knives, axes, etc., yet not a day's work could we ever get out of any of the coast tribes. The women do all the manual labour, and their time was far too fully employed for them to give any to our requirements. Fortunately they were willing to supply any amount of roofing for the huts, pandanus leaves being treated in a similar manner as in Java and elsewhere in the East. Canoes already in use they sold freely, a priceless asset to us, for no means of river transport had been imported into the country by the expedition. The natives refused to assist in poling or paddling our canoes up the river, for, as we afterwards learnt, the line of demarcation between the coast and up-river natives is very strictly observed, and the line at this place lies at the head of the tidal waters. After several attempts, the village of Parimau, at the head-waters of the Mimikia, was reached. Six marches separated it from Wakatimi. The people were of similar build and not to be distinguished from the coast people except that they were of pleasanter countenance and better workers. The Wakatimi natives had proved themselves a drunken, swashbuckling crew, always ready for a brawl, and perpetually excited by copious draughts of the potent beer of the sugar-palm. The Parimau men were unable to obtain alcohol on account of the absence of palms, and this may account for their more orderly disposition. This is only used in a comparative sense, for the people fly to arms on the slightest provocation, men and women alike. An individual quarrel is taken up by the whole village, in less than a minute a scene of quiet and peace being turned into a pandemonium. The weapon chiefly favoured is the stone club, at least one of which is always standing by the doorway ready for instant use. I saw no people actually killed in the broils, but many badly injured; care is always taken to avoid striking the head.

Parimau, although thirty miles from the coast in a direct line, is situated at a height of less than a hundred feet above sea-level. The intervening country was covered with a dense growth of Pandanus (four species), Artocarpus, (three species), Eriodendron, *Albizzia Moluccana*, Ficus (many species), *Sagus rumphii* and *Octomeles Moluccana*, together with Rattans (Calamus) of three or four species, and the scarlet-flowering creeper *Mucuna pruriens* and the wild *Vanilla planifolia.* Underfoot the land was boggy, and during the heavy rains almost impassable. Such is the vegetation which extends over the whole of south-west New Guinea from the bare rocks of the central range to the sea. At intervals of from four to six miles the land is intersected by great rivers, none of which are fordable, except on rare occasions, until the mountains are reached. The majority have their sources in the highlands to the north at about S. lat. 4°, the remainder rising in the low-lying jungle, and kept replenished by the constant rain. On entering the plains, the beds change from stone to mud, the course continually shifting as the dams of timber block the flow of the waters. The difficulty of cross-country movement cannot be exaggerated. Not only do the greater rivers render a crossing a dangerous and uncertain proceeding, but endless smaller streams, many waist-deep, add to the portage difficulties. Communication is maintained between the Kaparé, Mimika and Tuaba rivers by a single faint trail marked by the men breaking the twigs as they pass along. At the Tuaba River the track ceases, the country to the east being uninhabitable on account of the denseness of the forest and the excessive hardness of the timber. For this very reason, pig, wallaby and cassowary here find a sanctuary, from whence the more depopulated hunting districts are restocked. Flesh, however, forms but a small item in the diet of the natives, their chief and practically only food supply being sago and fish. Bananas and pumpkins are sometimes grown, but only in limited quantities, for their cultivation requires a certain amount of labour, to which the Papuan is adverse. Many individuals grow tobacco, to which all natives are very partial.

For many months attempts to penetrate into the hills met with little success. Stores had first of all to be accumulated at Parimau, the up-river station, a feat of great difficulty, as the imported coolies were limited in numbers, rapidly fell sick, and were much impeded by the varying conditions of the Mimika. None were available for cross-country transport, and the exploring party was forced to rely entirely upon what assistance could be obtained from the natives. These, however, proved most unreliable, on several occasions deserting when many marches from home and leaving us to get back the best way we could. Again, they refused to enter the hills, partly because they were not allowed to cross the line of demarcation between their own and the pygmy tribes who inhabited the lower mountains, and partly from fear of spirits or the hills themselves. The earliest attempt at exploration brought us out on to the Kaparé, a great river flowing to the west of the Mimika, and it was here that the first men of the pygmy tribe of Tapiro were run down and captured. The plainsmen were again successful in capturing two more a short time afterwards, and though terribly frightened they were treated with such kindness that we were able, later in the year, not only to trade with them, but to visit and stay in their village of Wambirimi, situated on the southern slope of the low-lying hills and at a height of eighteen hundred feet above sea-level.

It is unnecessary to describe the many fruitless attempts to discover the main village, hidden in the depth of the forest high up the mountain-side, or how we were received when the habitations were at last reached. No active hostility was shown to us, but it can hardly be said that we were received with open arms. Before the end of our stay with the Tapiro pygmies, however, their suspicions as to our intentions had been allayed to such an extent that they laid aside many of their weapons. Of the women and children we saw nothing, but their shrill voices, as they fled up the mountain-side, were sufficient evidence of their close proximity. Over sixty of the men put in an appearance, about half of whom were measured with the standard and craniometer: the average height worked out at 4 feet 8¾ inches. Taken as a whole they were well-made and wiry, whilst their colour was a dark chocolate. The hair, which was usually black, with sometimes a touch of brown or even red, was worn short. Many wore beards, the older men dying theirs a bright red. Circlets made out of the shoulder-blades of the wallaby, cassowary feathers, or knotted black string were in common use. Around the neck was suspended a small string bag containing tinder and dried leaves for use as cigarette-papers, while in addition many wore necklaces of bones of the wallaby or cus-cus, or black and red seeds. Over one shoulder was slung a large bag containing fire-sticks, boars' tusks and other small possessions. For clothing a long yellow gourd was the only form of dress in general use, and was kept in position by a string passed round the waist. Of their houses and surroundings I have treated in my published account of our travels; suffice it to say that the architecture and workmanship were considerably in advance of that of their large-framed brethren of the plains. The known tribes of pygmies inhabiting our earth are divided into two classes, Negrillos and Negritos. The former are to be found only in the forests of Central Africa, while the latter inhabit the Andaman Islands, the Malay Peninsula and the Philippines. To the list of Negritos must now be added the newly discovered tribe of Tapiros, who so far as their stature is concerned take rank next above the Congo pygmies. I am sorry that it is impossible for me in the limited time at my disposal to discourse at greater length upon these small people, but those who wish to study the matter will find much information in the chapter of Dr. H. S. Harrison, which has been incorporated in the above-mentioned account of our attempt to penetrate to the heart of New Guinea. The photographs which will now be thrown upon the screen will give you a better and more accurate idea of these people than it is possible for any words of mine to do.

It was soon seen that no advantage would be gained by forcing a passage due north by the valley through which the Kaparé flowed, for besides the difficulty of clearing a path along the almost precipitous sides of the valley, the only goal to be reached would be the foot of the great precipice lying twelve miles to the north, and many marches to the west of the snows. This line was therefore abandoned and a path cut eastward to the Tuaba, a great and often impassable river. An advance by this waterway was likewise purposeless, and the line was continued across to the Kamura, then for two marches up its bed and on to the Wataikwa. In addition to the want of transport, the expedition was much hampered by torrents of rain and flooded rivers and jungle. So great were these rivers that the line of communication was liable to be cut for days at a time, an awkward event in a country where local food supplies are *nil*.

All forward work was done entirely by the Europeans, Gurkhas and those few natives who could be induced to work. In addition to the total breakdown of the imported labour on two occasions, the steam-launch lent to us by the Dutch authorities and the motor-boat bought in June, were quickly rendered useless, the former on account of its not being of sufficient power to draw canoes against the current, the latter by being wrecked when returning down the river during a flood. All supplies had therefore to be man-hauled up the river, an operation which had no end. Progress was consequently extremely slow, as, in addition to the cutting parties, two collectors and their staffs had to be at the same time maintained in the field. Goodfellow was forced to leave the expedition in the early part of October, suffering from fever and beri-beri, and Shortridge in November. Mr. C. Grant, who had arrived in the country in July, was now the sole representative of the collecting section, for Mr. W. Stalker had lost his life in the forest soon after landing. From Goodfellow I took over command.

During the months of July and August the road was pushed on to the Iwaka, a deep and rapid river four marches to the east of Pariman, and in the direct line to the snows. At the same time the clearing at Pariman was being regularly worked at, fourteen acres being finally cleared in five months, and a fine view obtained of the whole of Carstensz and its neighbouring ranges, their positions being fixed and heights obtained, including a new snow-capped mountain, now called Mount Idenburg, after the Governor-General of the Netherlands India.

No further progress, with the exception of sundry journeys into the hills, could be undertaken until the arrival of fresh coolies from Macassar. These arrived on December 23, but were most disappointing from the point of view of physique and carrying power. Within two months more than half of these were on the sick list and had to be invalided out of the country. Three months' stores were now rapidly accumulated at Pariman. From thence one hundred and ten loads were collected on the Wataikwa. It was necessary to move in two, and on one occasion in three, parties, relays being the most economical way to accumulate stores at the front, while at the same time carrying on the work. Marshall and Grant therefore moved first, carrying a certain amount of stores to the Iwaka, and then continuing the path up the right bank of that river for two marches in a search for a crossing place, and there formed a fresh base camp. In this search they were not successful, for the river showed no signs of diminishing in volume, was found to run from the north, and not from the east as we had hoped, was unfordable anywhere, and too broad for a felled tree to form a temporary bridge. On Wollaston's and my arrival, ten days' food supplies were in hand; the river had been well prospected to the north, and it was evident that a crossing had to be made by some means or other.

Our intention was to reach the summit of a range of hills which formed an almost complete circle, and from whence a full view of the snows and main range might be obtained. As I said before, but ten day's supplies were in hand, and if this object was to be obtained an immediate move was imperative. Fresh efforts were made to fell trees across the river, but all attempts proved fruitless. Large trees were scarce, and those which fell correctly either broke their backs or were at once swept away by the current.

Late that night, however, luck changed, and the Gurkhas succeeded in felling one tree which held long enough for a single strand of rattan to be drawn across in position. In two days more, thanks to the pluck and cleverness of the Gurkhas, the bridge was complete. An immediate advance was made to the east, for only eight days' supplies remained. We were now on a branch of the Wania River, and up this new bed we advanced for two days. On account of the roughness of the road and the denseness of the vegetation slow progress was made, so slow in fact that it was soon realized that by this route the summit of the range could not possibly be reached with the amount of provisions still in hand. We therefore cut up a spur lying directly to the east, reached a height of 3000 feet that night, and the summit of the hill (5500 ft.) the day following. The going underfoot was such as we had never before experienced, for so dense was the mass of live and dead timber, that the path had to be made over and through the forest, solid ground not being found that day or the next, when a small party advanced along the crest. At the greatest height (6000 ft.) a fine point of observation was found, but on account of the clouds which early collected no view could be obtained. Rations were again reduced to a minimum, and a fresh attempt made the following day. We were well rewarded. To the south lay the interminable plain without a break, except where the shining waters of the great rivers appeared and vanished in the dense forest. Not a lake or a treeless swamp lay in the whole hundred miles from the Charles Louis Mountains in the west to the farthest point beyond Carstensz in the east and to the coastline fifty miles away. Along the foot of the lower hills lay that belt of forest some five miles in width through which we had had so much difficulty in cutting a path across to the Iwaka. There the rate of progress, with an average of five men at work a day, had been no more than three hundred yards in the twenty-four hours, the average falling some days to a bare one hundred and fifty yards. And then, when completed, the road had to be abandoned and a fresh line cut, for the going was such as to render it impracticable for coolie transport. To the east lay the great mass of Cocks Comb, its three equal points showing clear against the sky. Beyond and to the north lay the wild southern slopes of Carstensz. Directly behind to the north-west rose Mount Godman and Wataikwa Mountain and the great ridges running from the north. But grander than all was the main backbone of the Nassau Range. From Carstensz in the east to Mount Leonard Darwin, and beyond that again to the west, but with a dwindling altitude, lay a great precipice, eighty miles or more in length, seamed with cracks and fissures, but, to the eye, unclimbable. The greatest sheer height measured with a theodolite showed six thousand five hundred feet, but we were never in a position to use that instrument when the foot and the summit were visible at the same time. The greatest vertical height is Mount Leonard Darwin itself, and here, it is little less than ten thousand feet, or about one and three-quarter miles. It appears to consist of hard limestone, the strata being clear and distinct. The dip is at an angle of twenty-five degrees to the north, and as no higher mountains were seen beyond, and as the south-flowing streams take their rise from the precipice, there is little doubt that the main water parting between north and south Dutch New Guinea is along the summit of this precipice. The mountains to the south are much broken, and so covered with vegetation that the strata can be but seldom seen. An outcrop of slate with perpendicular strata exists in the bed of the Iwaka nineteen miles to the south, and close by is a quantity of clay. Seams of coal, varying in width of from four to eight inches, and strong evidences of petroleum, were also found in the same neighbourhood. A few miles below granite boulders lay scattered about the river beds, together with iron-ore, and tin in small quantities. There were no signs whatever of the presence of gold. However rich the country may be in these metals, the climate is such that the cost of imported labour to work them is prohibitive. No local labour is available, no food supplies, and no local means of transport. The rainfall alone is such as to render the country practicably uninhabitable. No high ground being available, the natives are forced to camp on whatever may be at that time the ground least threatened by the floods. This accounts for the fact that there are no permanent buildings in the whole district. Parimau was on three separate occasions washed out of existence, the natives saving themselves and their goods in their canoes. The village behind the Wakatimi base camp was completely destroyed in June and not rebuilt. The rivers alter their courses and beds almost daily, and to such an extent that permanent bridging would soon be rendered useless.

From the farthest point reached, that is to say, eleven marches from the up-river camp of Parimau, a distance which had taken six weeks to cover, we retired on February 14, 1911, accomplishing the return journey in six days, for food was short, the road cut, and all were only too anxious to leave the country now that the work was accomplished. Clearly had it been shown how impossible was the task of reaching the snows with the Mimika as the line of communication. Instead of being able to follow the natural lie of the country we had been forced across the great rivers, rivers capable of cutting one's communication with the base for weeks at a time, always difficult to cross, often dangerous, and sometimes impassable. While Marshall and Wollaston were making one further attempt to obtain a view of the pygmy women, and doing their best to overcome the scruples of the men, a task in which they were not successful, I proceeded to Wakatimi, and with the assistance of the motor-boat, which was, after much work, made to float and move, explored the next great river to the east.

The Kamura was found to have two mouths, several miles apart, and carrying an equal volume of water. The west branch, known as the Atoeka, empties itself directly into the sea, the east branch, the Kamura, into a bay some six miles in length, and two to three in width. Into this latter bay also flows the Wania, called by the coast tribes the Wandia, probably the greatest river between the Mimika and the Oetakwa. The Oetakwa was last year partially explored by Lieutenants Van der Bie and Postema by order of the Netherlands Government. It flows directly from Carstensz, and is of sufficient size to allow of a ship crossing the bar and proceeding for seventeen miles up the river. From this spot a launch can proceed for two days, and again canoes for still one more march. From this point the expedition pushed on for seven marches, when the members were forced to abandon the attempt to reach the snows.

But to return to the Kamura. The bifurcation takes place about fifteen miles from the coast by river, the main stream above being fed by the Tauba, Kamura, Wataikwa and Iwaka rivers. These rivers are navigable for launch and canoe to the edge of the foothills.

Two large villages were found, of the same name as the rivers, the Atoeka and Kamura, both situated beyond the tidal waters. Atoeka is in fact the largest village in these parts, consisting of about six hundred huts with a population of from two thousand to two thousand five hundred, and with tobacco and cocoanut plantations round about. The people are similar in almost every respect to those met with on the Mimika. Not so the Kamura tribe, with their two hundred and fifty huts and one thousand to fifteen hundred souls. The latter are of a decidedly lower type, of heavier build, and with more brutal countenances. Many of the men and several of the women were entirely nude. Only a short stay was here made, for the people were bent on plunder.

A further attempt was made to explore the Wania when Marshall and Wollaston reached Wakatimi from Parimau, the latter place having been abandoned to the natives. In this we were not successful, the motor-boat losing her propeller in a storm. Little need be said of this trip, for the results, though decidedly exciting and unpleasant, were not of geographical interest. The journey took six hours out and six days to return, the party being extremely lucky to escape with their lives. We had been in a heavy storm on a lee shore, surrounded by foam-covered reefs, in a sea teeming with sharks, and only held by an old rope and light anchor. Had the boats swamped, or had anything given, it was certain death for the three of us, together with our seven men.

The whole coast is a most dangerous one, on account of the shallowness of the water and the rapidity with which storms arise. On several occasions relief ships arrived off the coast, and were forced to depart without landing their stores or being able to take away the invalids. None of the ships, each of which drew from twelve to fourteen feet of water, could approach nearer than two and a half miles from the shore This, together with the fact that every river has a pronounced and ever-moving bar, renders the south-east coast too dangerous for regular navigation.

On April 5, the gunboat *Mataram* and the marine ship *Zwaan* appeared off the coast, and by these we sailed for Amboina. Here the expedition broke up, Cramer and his men destined for Java, our coolies for Macassar, the Gurkhas for Darjeeling, and we four for England. The Javanese soldiers had proved invaluable in guarding Wakatimi and Paramau during our long absences in the mountains, and when encamped on the rivers to the east and west ; and Lieutenant Cramer, the leader of the escort, and the only European who had remained with us from the beginning to the end, had filled a difficult rôle with much ability, tact, and success. Of the Gurkhas I must specially mention Mahesur, Jangbir, and Harkajit.

To sum up the final results of the expedition : large and valuable collections of birds, mammals, reptiles, butterflies and moths had been formed, together with botanical and ethnographical specimens ;

a new and unknown race of pygmies discovered, studied, measured, and photographed; a range of mountains, containing the greatest precipice in the world, together with three thousand square miles of country, surveyed and mapped, new snow-mountains found, and many great rivers explored; and a long stretch of coastline surveyed. We had accomplished the longest cross-country journey ever undertaken in Dutch New Guinea—i.e. eleven marches from the up-river camp—had proved the impossibility of the Mimika River as a line of advance to the snows, and on the other hand the value of the great rivers to the east if the same goal is intended.

May, 1913. Since writing the above, news has been received that Dr. Wollaston, using the Utakwa River as his line of advance, has succeeded in reaching the snows of Carstensz. His journey has been completely successful, and the one failure of the pioneer expedition has been rectified.

[C. G. R.]

WEEKLY EVENING MEETING,

Friday, May 30, 1913.

Sir James Crichton Browne, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Owen Seaman, Esq., M.A. D.Litt., Honorary Fellow
of Clare College, Cambridge.

Parody.

[Synopsis.]

Derivation. Parody, in its most elementary form, a verbal echo; yet often demanding great ingenuity, as shown in the work of some of the greatest masters (Calverley, Hilton, J. K. Stephen).

Reason for its popularity with young writers. Not to be always explained by the joy of sacrilege, which does not enter into burlesques of such originals as, for instance, "The Walrus and the Carpenter."

Progressive stages of parody. Imitation (1) of an author's subject-matter (Bret Harte's "Condensed Novels"), (2) of his literary methods and attitude of mind as exhibited in (*a*) an isolated example (Calverley's "The Cock and Bull," and Browning's "The Ring and the Book"), and (*b*) in his work at large. Parody, in its highest form, a department of pure literary criticism.

Eccentricities are comparatively easy to imitate. To be able to seize an author's permanent qualities is a much harder task, requiring close familiarity with his work.

The right occasion for a parody. The rarity of excuses for a collective series. "Rejected Addresses," "Lost Masterpieces," "New Rhymes for Old."

The right scope of a parody. Is it allowed to criticize the author himself as well as his work? Necessity for an attitude of detachment on the part of the parodist.

The right subject for a parody. Limits imposed by the laws of decency. Humour's way of paying homage to serious achievement. Need for a certain community of spirit with the writer who is to be parodied.

The test of a good parody. Its highest triumph to be approved by the author whom it ridicules.

The part played by parody in the service of literature.

[O.S.]

GENERAL MONTHLY MEETING,

Monday, June 2, 1913.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Leonard Rome Guthrie, Esq.
George Wilson Heath, Esq., F.R.G.
Ronald Malcolm, Esq.

were elected Members of the Royal Institution.

The Secretary announced the decease of Lord Avebury on May 28, 1913, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution in the decease of the Right Honourable Lord Avebury, P.C. D.C.L. LL.D. M.D., Vice-President of the Royal Society of London, Corresponding Member of the French Academy, Commander of the Legion of Honour, Prussian Order pour le Merite.

Lord Avebury was a Member of the Royal Institution for sixty-four years, and he served the Institution on several occasions as Vice-President and Manager. He delivered courses of Afternoon Lectures at the Royal Institution in 1864 and 1868, the subjects being "The Antiquity of Man" and "Savages." He has given no less than ten Friday Evening Discourses, viz. as follows: "The Ancient Lake Habitations of Scotland" (1863), "The Metamorphoses of Insects" (1866), "The Relations of English Wild Flowers to Insects" (1875), two Friday Evening Discourses on "Habits of Ants" (one in 1877 and the other in 1879), "Fruits and Seeds" (1881), "Forms of Leaves"

(1885), "Forms of Seedlings" (1886), "Shapes of Leaves and Cotyledons" (1890), and "Buds and Stipules" (1898).

Lord Avebury was President of the British Association in its Jubilee year 1881. He also occupied the Presidential Chair of the Entomological Society, Linnean Society, Anthropological Institute, Statistical Society, Society of Antiquaries, e c., and he was an Honorary Member of many learned and scientific Societies. He was Vice-Chancellor of the University of London from 1872 to 1880, and from that time represented that University in Parliament for a period of twenty years. He was a prolific author, whose works were extremely popular, including "The Origins of Civilisation," "Prehistoric Times," "The Scenery of Switzerland and of England," and many other works.

The Managers of the Royal Institution desire to express their most sincere sympathy with Lady Avebury and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Annual Report on Kodiakanal Observatory for 1912. 4to. 1913.

The Secretary of State for India—Memoirs of the Department of Agriculture: Entomological Series, Vol. IV. No. 3. 8vo. 1913.
Agricultural Journal, Vol. VIII. Part 2. 8vo. 1913.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXII. 1° Semestre, Fasc. 8. 8vo. 1913.

Alchemical Society—Journal, Vol. I. Parts 4-5. 8vo. 1913.

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WEEKLY EVENING MEETING,

Friday, June 6, 1913.

HIS GRACE THE DUKE OF NORTHUMBERLAND, P.C. K.G. D.C.L. LL.D. F.R.S., President, in the Chair.

FRANCIS WARD, ESQ., M.D. Edin. M.B. C.M.

Reflection as a Concealing and Revealing Factor in Aquatic and Sub-Aquatic Life.

[ABSTRACT.]

As a result of observations and experiments carried out on ponds built for the purpose, and by the use of apparatus for observing organisms in their natural environments, I have arrived at certain conclusions as to the value of reflection as a concealing factor in various forms of aquatic and subaquatic life. The general principle upon which these ponds are built is as follows : In one bank of the pond is a glass window, and beyond this window an underground observation chamber. No light enters this chamber except through the surface of the water By this means everything in the pond is seen by *entirely* natural illumination, the observer cannot be detected, and as there is no reflection from the glass the making of photographic records is greatly simplified. In the first pond, built for the observation of objects in the water, the glass is perpendicular. In the second, for observing objects on the surface, the glass is at an angle of 45° to the surface.

The apparatus I use consists of a tube 18 inches square and 5 feet long, on one side at the lower end is a window; a reflex camera slides into this tube, so that the lens is opposite the glass. When in use, a heavy weight carrying a hook is lowered into the water, with the end of the tube attached to the hook. The whole apparatus can be tilted at any angle, and by this means the incident rays from any object in any position—except overhead—are made to strike the glass at right angles, and thus distortion, due to refraction through the glass, is rendered negligible.

The apparatus has been mainly used as a check upon observations in the ponds.

For observing and photographing life on the bottom, I use a tube 3 feet long, with a glass partition inside, a foot from the end. This apparatus acts as a boat-like sea telescope, and is fitted with a camera. Lastly, through the kindness of Professor Herdman, I have established at Port Erin a large wooden tank above ground. Three sides of the tank incline at an angle of 45° ; the fourth is perpendicular. In the perpendicular side is a glass window, and attached to it on the outer side is an observation box, 6 feet by 4. The tank and observation box revolve together on a platform ; by this means an object in the tank can be seen by reflected and transmitted light at will.

It is usual to consider pigmentation as the important factor in the concealment of sub-aquatic life. Among organisms that live in more or less the same character of surroundings, this is true to a great extent ; but in the forms of life that are constantly changing their environments, the best concealed are those that most effectively reflect their surroundings.

When, however, an organism depends mainly upon reflection for its concealment, the reflection of light from above has to be modified, or else the organism is revealed. In some forms of life, particularly fishes, pigmentation upon the back is the method of modifying this reflection from above. In other forms this top light is cut off by position, e.g. in light-coloured anemones, which are only to be found attached to the under-surface of shelving rocks.

Before proceeding further, I would like to illustrate the appearance of a white object, as seen from under the water.

I show a sheet of white cardboard pinned on a red stick, which in turn was stuck in the centre of an *empty* pond. The sides and bottom of the pond were covered with green confervæ. In this position the card appeared white, and incidentally the stick red.

The pond was then filled up with water, and now the white card so exactly reflected the colour around, that it became practically invisible, yet its position was revealed by a streak of light along the upper edge of the card.

In nature all white sub-aquatic organisms reflect in a similar manner, and white is never seen under the water, except when there is no provision made for modifying the reflection of light from above, or when the organism turns on its side.

As an illustration of this point let us consider the white anemone (*Actinoloba dianthus*). I show a colour-plate of this anemone attached to the top of a rock and, of course, it appears white, but as soon as it moved only a distance of two inches under the shelving edge of the rock, the top light was cut off, and you will see the white anemone appears green as it reflected the prevailing colour below.

It will be noticed that the white serpula on the rock reflects in the same manner ; and as the light parts of rock also appear of a greenish colour the anemone and the serpula are practically invisible.

I say practically invisible advisedly, for the greenish anemone when closed makes a uniformly shaded green mass against a patterned rock.

It will have been noticed that the red stick holding the white card, when seen from under the water in green surroundings, appeared a dull black.

When *Tealia crassicornis* (a red and white anemone) is attached to the under surface of a rock with a green coloration below, the whites of this anemone appear green and the red markings appear dark, so that now the anemone shows a general green coloration with dark markings upon it, which fit in with the dark markings on the stone.

Many forms of light-coloured marine life are found under shelving rocks. I consider they escape destruction in this position, owing to the fact that they reflect their surroundings.

Next let us consider the modification of reflection by pigmentation. This is best illustrated in fishes. Until one has observed fish by *entirely* natural illumination, it is difficult to realize how important a part reflection plays in making both silvery and highly-pigmented fishes inconspicuous.

The silvery fish does not appear silvery, but red, brown or green, according to the general colour around, and in addition it will reflect upon its body stem for stem the reeds into which it has rushed in order to hide itself.

As an illustration to show how a highly-pigmented fish reflects light, I show a tench, only six inches under the water, and it will be seen the dark back appears quite silvery.

Pigmentation on the back conceals a fish against the bottom, but undoubtedly the important function is to conceal it, for protective and aggressive purposes, from other fish on the same level as itself. I would point out that the same light which is reflected from the sides of the fish, through the eye of the fish, controls the amount of contraction of the pigment cells on the back: thus the reflection from above is correctly modified, and the fish is rendered a uniform shade. But this uniform shade only conceals against a uniform background.

Thayer has shown in the animal world, how the counter-shaded bird, or beast, without markings, when seen against a patterned background becomes conspicuous, because it interrupts the pattern.

The same is seen in the fish world, and in illustration I would draw your attention to the appearance of a perch (*Perca fluviatilis*) swimming past a reed bed.

In the autochrome of a brown trout lying under a stepping stone, I show the value of reflection; here, the back green, and the belly red, as they reflect the stones above and below, are undoubtedly the main factors in concealing this fish, and the markings simply prevent the body from appearing patternless.

I would next draw your attention to the possible influence of reflection of light, from some forms of marine vegetation, upon the pigmentation of various marine organisms.

Several red and brown seaweeds seen by transmitted light appear red and brown, but when seen against a dark background they reflect at various points a brilliant bluish purple colour. *Chondrus* shows this well.

In certain positions the whole side of a dark rock, covered with red and brown seaweed, shows blotches and streaks of bluish purple. This is well marked upon the concrete blocks on the old breakwater at Port Erin.

Lobsters, crabs, and many other forms of marine life, usually found in crevices among dark rocks covered with red and brown seaweeds, show a pigmentation exactly similar in appearance to the colour reflected from the seaweed. This is particularly well marked in the swimming crab, *Portunus puber.*

I would now refer to the appearance of life on the surface, as seen from below. This appearance entirely depends upon the position that the particular organism occupies on the surface, relatively to the point of observation from below the water.

On looking up to the surface, an observer sees above him a circle of light, through which he can see the sky and clouds. Beyond this circle there is total reflection, and the surface of the water reflects the general colour below.

Transparent organisms are practically invisible, both in the circle of light and beyond.

Now it is generally understood that forms of life that occasionally or habitually float on the surface are white underneath, so as to conceal them against the clouds and wave foam.

In dealing with this subject it is necessary to make a difference between white organisms that are opaque and those that are translucent.

Commencing with the opaque, I will illustrate the point with the appearance of a thick white saucer.

This was floated from some distance over my head. Outside the circle of light the surface of the water was reflecting the green bottom of the pond, the white saucer did the same, and, therefore, was invisible against the surface of the water. When, however, it came into the circle of light it still reflected the dark colour below and was revealed as a well-defined dark object against the sky and clouds.

A white-breasted gull swimming on the surface is concealed and revealed in an exactly similar manner. Therefore, an opaque white organism is not concealed against the clouds at any time, and when seen against the clouds the whiter the object the more conspicuous it becomes, because it reflects the dark water below. A white object is, however, concealed by reflection in the area of total reflection.

How does this explanation affect the concealment of a white opaque object on the sea from a fish ?

The size of the circle of light on the surface depends entirely upon how far the fish is under the water, for lines drawn from the two ends of the diameter of the circle make an angle of 97° at the eye of the fish.

When the fish is some depth under the water there may be several white sea-gulls on the surface within the circle of light, but as the fish comes up to feed his circle of light is narrowed down, and the gulls slip into the area of total reflection and by reflection become invisible to the fish.

For my experiments with translucent organisms I used the shell of an *argonauta*. In the circle of light you will see the shell is still very obvious, but as it transmitted a considerable amount of light it did not appear black like the white saucer.

In the area of total reflection, however, the nautilus appears white, for in consequence of not being an opaque object it is no longer a reflector.

The nautilus seems to slip between two stools, it is too opaque to be concealed in the circle of light, and too translucent to be concealed in the area of total reflection. In the latter situation it certainly may be protected by simulating the appearance of wave foam, for wave foam in the area of total reflection appears as a flickering light.

So far we have only considered that portion of an object that is actually immersed. If, however, the organism under consideration is not too far distant, that portion of it above the water is visible on the edge of the circle of light, and the parts respectively above and below appear to be separated by a considerable interval of water surface.

When the portion above the water is white, as in a gull, it is difficult to detect against the sky.

The above remarks only refer to open water, and I will illustrate how a wading bird is concealed against the image of a reed-bed many yards behind him.

Looked at from below the markings on a heron are in bold upright lines, for the plumage is greyish white with black patches on each side of the head, and the black patches on the shoulders appear continuous with the black primaries on his wings. Seen against an open sky, the white parts of the wading heron blend with the sky, but the black parts stand up in bold relief. The head and shoulders of the bird are seen on the edge of the circle of light, but so also is the reed-bed many yards behind. The tops of the reeds appear as perpendicular images, and the perpendicular markings on the heron blending with them, make the bird inconspicuous.

I have referred to white as a concealing agency. Black objects, when they retain air-bubbles on their surface, also become reflectors under the water. The black water-spider under a leaf appears green and is lost to sight. A water-hen swimming on the surface in the area of total reflection reflects the green weeds below, and becomes difficult to discern against the surface which is reflecting the same colour.

In conclusion, I will refer to reflection as a revealing factor. I have already illustrated this point with the anemone. I now show a slide of a shoal of young rudd wheeling round, and as they turn each fish is revealed as a flash of light as he catches the light from above.

Among diving birds the cormorant does not retain air bubbles in his feathers to the same extent as the loose-plumaged water-hen, yet by reflection he appears light or dark, according to the nature of the bottom over which he is swimming. When, however, the cormorant dives his track is marked by a series of brilliant flashes of light.

Now this bird when swimming on the surface has the habit of dropping his head under water at regular intervals—shags do the same. Seen from below, every time he does this, there is a flash of light not unlike the flash from a silvery fish turning. It is quite possible that fish, such as pollock and codling, are attracted by this flash, and thus swim towards their destroyer.

These flashes of light are still better shown in the case of the penguin, and this I illustrate with individual pictures cut out of a cinematograph film.

I have had to leave the subject of refraction of light on the present occasion ; firstly, because time does not permit of my dealing with it, and secondly, because during the last fortnight I have tested all my experiments at Port Erin, and some of the results have made me reconsider the conclusions at which I had arrived with regard to the refraction of light in its relation to marine organisms.

[F. W.]

GENERAL MONTHLY MEETING,

Monday, July 7, 1913.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Ernest Rutherford, Esq., D.Sc. LL.D. F.R.S.

was elected a Member of the Royal Institution.

The Chairman reported the decease of Her Grace The Duchess of Northumberland on July 6, 1913, and the following Resolution was passed unanimously, all present standing :—

Resolved, That the Managers and Members of the Royal Institution of Great Britain, at the General Meeting this day, desire to record their sincere sympathy and condolence with His Grace The Duke of Northumberland, President of the Royal Institution, and with the House of Percy, on the decease of Her Grace The Duchess of Northumberland.

The Managers reported that they had received a sum of £150 being part of the Legacy of the late Mr. C. E. Layton.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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United States Department of Agriculture—Experiment Station Record, Vol. XXVIII. Nos. 3–6. 8vo. 1913.
Hurricanes of the West Indies. By O. L. Fassig. 4to. 1913.
Report of the Chief of the Weather Bureau, 1911–12. 4to. 1913.

United States, Department of Commerce and Labour—Magnetic Observations near Tucson, Arizona, 1909–10. 4to. 1913.

United States Department of the Interior—Geological Survey: Bulletin, Nos. 502–503, 510, 521. 8vo. 1913.

Geologic Atlas, Nos. 183–184, 186. fol. 1912.

Water Supply Papers, Nos. 259, 293, 300, 310–311, 313, 316. 8vo. 1913.

Mineral Resources, 1911. 2 vols. 8vo. 1913.

United States Patent Office—Gazette, Vol. CXC. No. 5; Vol. CXCI. Nos. 1–4. 8vo. 1913.

Vienna Imperial Geological Institute—Verhandlungen, 1913, Nos. 2–5. 8vo.

Wellcome Chemical Research Laboratories—Publications, Nos. 143–150. 8vo. 1913.

Western Australia, Agent-General—Geological Survey Bulletins, No. 42. 8vo. 1912.

Statistical Abstract for Feb.–March, 1913. 4to.

Western Society of Engineers—Journal, Vol. XVIII. Nos. 3–5. 8vo. 1913.

Yorkshire Archæological Society—Journal, Vol. XXII. Part 3 (No. 87). 8vo. 1913.

Yorkshire Philosophical Society—Annual Report, 1912. 8vo. 1913.

Zoological Society of London—Proceedings, 1913, Part II. 8vo.

GENERAL MONTHLY MEETING,

Monday, November 3, 1913.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L F.R.S., President, in the Chair.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Kodaikanal Observatory Bulletin, Nos. 31 33. 4to. 1913.

Geological Survey: Memoirs, Vol. XLI. 8vo. 1913.
Records, Vol. XLIII. Parts 1–2. 8vo. 1913.

Report on the Government Museum and Connemara Public Library for 1912–13. 4to. 1913.

Memoirs of the Department of Agriculture: Botanical Series, Vol. VI. Nos. 2–3; Chemical Series, Vol III. No. 2; Bacteriological Series, Vol. I. No 2. 8vo. 1913.

Agricultural Journal, Vol. VIII. Parts 3–4. 8vo. 1911.

Astronomer-Royal—Greenwich Observations, 1911. 4to. 1913.

Photo-heliographic Results, 1911. 4to. 1912.

Annals of the Cape Observatory, Vol. VIII. Part 3. 4to. 1912.

Report of the Cape Observatory, 1912. 4to. 1913.

British Museum Trustees—Catalogue of the Marine Reptiles of the Oxford Clay, Part II. 4to. 1913.

Catalogue of the Mammals of Western Europe. 8vo. 1912.

Catalogue of the Chætopoda, Part I. 8vo. 1912.

Catalogue of Lepidoptera Phalænæ, Vol. XII. and Plates. 8vo. 1913.
Catalogue of Indian Big Game. 8vo 1913.
Revision of the Ichneumonidæ, Part II. 8vo. 1913.
The House Fly as a Danger to Health. 8vo. 1913.
Catalogue of Ungulate Mammals, Vol. I. 8vo. 1913.
Catalogue of British Pisidium. 8vo. 1913.
Catalogue of Talbot's Nigerian Plants. 8vo. 1913.
Guide to Flight Exhibition. 8vo. 1913.
History of the Collections, Vol. II. Appendix. 8vo. 1912.
Catalogue of the Library, Vol IV. P-SN. 4to. 1913.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali, Vol. XXII. 1° Semestre, Fasc. 12; 2° Semestre, Fasc. 1–6. Classe de Scienze Morali, Serie Quinta, Vol. XXII. Fasc. 1–7. 8vo. 1913.
Rendiconto, 1913, Vol. II. 4to.

Accountants, Association of—Year Book, 1913. 8vo.
Journal, Vol. VI. No. 23. 8vo. 1913.

American Academy of Arts and Sciences—Proceedings, Vol. XLVIII. Nos. 18–20; Vol. XLIX. Nos. 1–4. 8vo. 1913.
Memoirs, Vol. XIV. No. 1. 4to. 1913.

American Geographical Society—Bulletin, Vol. XLV. Nos. 7–10. 8vo. 1913.

American Philosophical Society—Proceedings, Vol. LII. Nos. 208–210. 8vo. 1913.

Aristotelian Society—Proceedings, 1912–13. 8vo. 1913.

Asiatic Society, Royal—Journal for July-Oct. 1913. 8vo. 1913.

Asiatic Society of Bengal—Journal: Vol. VIII. Nos. 5–10; Vol. LXXV. Part II. 8vo. 1912–13.

Astronomical Society, Royal—Monthly Notices, Vol. LXXIII. Nos. 8–9. 8vo. 1913.
List of Fellows, 1913. 8vo.

Bankers, Institute of—Journal, Vol. XXXIV. Nos. 7–8. 8vo. 1913.

Belgium, Royal Academy of Sciences—Bulletin, 1913, Nos. 4–6. 8vo.

Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1913, Nos. 23–40. 8vo.

Birmingham Natural History and Philosophical Society—Proceedings, Vol. XIII. No. 1. 8vo. 1913.
Annual Report, 1912, and List of Members, 1913. 8vo.

Boston Public Library—Bulletin, Third Series, Vol. VI. Nos. 2–3. 8vo. 1913.

Botanic Society, Royal—Botanical Journal, Vol. II. No. 7, 1913. 8vo.

British Architects, Royal Institute of—Journal, Third Series, Vol. XX. Nos. 17–20. 4to. 1913.
The Kalendar, 1913–14. 8vo. 1913.

British Astronomical Association—Journal, Vol. XXIII. Nos. 9–10. 8vo. 1913.
Appendix to Memoirs, Vols. XV. and XVIII. 1913.
List of Members, 1913. 8vo. 1913.

Brooklyn Institute—Science Bulletin, Vol. II. Nos. 1–2. 8vo. 1913.

Buenos Aires—Bulletin of Municipal Statistics for March–June, 1913. 4to.

Cambridge Philosophical Society—Proceedings, Vol. XVII. Part 3. 8vo. 1913.

Canada, Department of Marine—Report of the Meteorological Service, 1909. 8vo. 1913.

Canada, Department of Mines—The Nickel Industry. By A. P. Coleman. 8vo. 1913.
Economic Minerals of Canada. 8vo. 1913.
Memoirs, No. 33. 8vo. 1913.

Carnegie Institution—Contributions from the Mount Wilson Solar Observatory, Nos. 69–73. 8vo. 1913.

Chemical Industry, Society of—Journal, Vol. XXXII. Nos. 13–20. 8vo. 1913.

Chemistry, Institute of—Proceedings, 1913, Part 3. 8vo. 1913.

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List of Fellows, 1913. 8vo.
Chicago, Field Museum of Natural History—Publications: Anthropological, Vol. XII. No. 1; Botanical, Vol. II. No. 8; Geological, Vol. IV. No. 2; Report, Vol. IV. No. 3; Zoological, Vol. X. Nos. 7–8. 8vo. 1912–13.
Church, Sir A. H., K.C.V.O. F.R.S. (*the Author*)—Precious Stones (Victoria and Albert Museum Handbooks). 8vo. 1913.
Churchill, Messrs. J. & A. (*the Publishers*)—Who's Who in Science, 1913. 8vo.
Civil Engineers, Institution of—List of Members, 1913. 8vo.
Proceedings, Vol. CXCII. 8vo. 1913.
Cornwall Royal Polytechnic Society—Eightieth Annual Report. 8vo. 1913.
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Dax, Société de Borda—Bulletin, 1913, No. 1. 8vo.
Dewar, Sir James, M.A. LL.D. D.Sc. F.R.S. M.R.I.—Admissions to Peterhouse, 1615–1911. By T. A. Walker. 8vo. 1912.
Durning-Lawrence, Sir Edwin, Bart., M.R.I.—Milton's Epitaph to Shakespeare. 8vo. 1913.
East India Association—Journal, New Series, Vol. IV. No. 3. 8vo. 1913.
Editors—Aeronautical Journal for July, 1913. 8vo.
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American Journal of Science for July-Oct. 1913. 8vo.
Athenæum for July-Oct. 1913. 4to.
Author for Oct. 1913. 8vo.
Canada for July-Oct. 1913. 8vo.
Chemical News for July-Oct. 1913. 4to.
Chemist and Druggist for July-Oct. 1913. 8vo.
Church Gazette for July-Oct. 1913. 8vo.
Concrete for July-Oct. 1913. 8vo.
Dyer and Calico Printer for July-Oct. 1913. 4to.
Electrical Engineering for July-Oct. 1913. 4to.
Electrical Industries for July-Oct. 1913. 4to.
Electrical Review for July-Oct. 1913. 4to.
Electrical Times for July-Oct. 1913. 4to.
Electricity for July-Oct. 1913. 8vo.
Engineer for July-Oct. 1913. fol.
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General Electric Review for July-Oct. 1913. 8vo.
Horological Journal for Aug.-Oct. 1913. 8vo.
Illuminating Engineer for Aug.-Oct. 1913. 8vo.
Journal of the British Dental Association for July-Oct. 1913. 8vo.
Junior Mechanics for July-Oct. 1913. 8vo.
Law Journal for July-Oct. 1913. 8vo.
London University Gazette for July-Oct. 1913. 4to.
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New Church Magazine for Aug.-Nov. 1913 8vo.
Nuovo Cimento for June-July, 1913 8vo.
Page's Weekly for July-Oct. 1913. 8vo.
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Fleming, Professor J. A., D.Sc. F.R.S. (the Author)—The Wonders of Wireless Telegraphy. 8vo. 1913.
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Franklin Institute—Journal, Vol. CLXXVI. Nos. 1–4. 8vo. 1913.
Geographical Society, Royal—Journal, Vol. XLII. Nos. 2–4. 8vo. 1913.
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Jefferson Physical Laboratory—Contributions, Vol. X. 8vo. 1913.
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Memorias, Tomo XV. 4to. 1913.
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Seventh Annual Report on London Waters. fol. 1913.
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Mining World (the Editor)—Index of Current Mining Literature, 1913, I. 8vo.
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Resultats des Campagnes Scientifiques accomplies par Albert I. Prince de Monaco, Fasc. 41, 44. 4to. 1913.
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Bulletin, 1913, Nos. 6-7. 8vo.
Moxon, C. F., Esq., M.R.I. (the Author)—On the Track of Truth. 8vo. 1913.
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Map of the British Empire. fol. 1913.
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Peru, Corps of Mining Engineers—Boletin, No. 79. 8vo. 1913.
Pharmaceutical Society of Great Britain—Journal for July-Oct. 1913. 8vo.
Philadelphia Academy of Natural Sciences—Proceedings, Vol. LXV. Parts 1-2. 8vo. 1913.
Photographic Society, Royal—Catalogue of Fifty-eighth Annual Exhibition, 8vo. 1913.
Physical Society of London—Proceedings, Vol. XXV. Part 5. 8vo. 1913.
Post Office Electrical Engineers, Institution of—Journal, Vol. VI. Parts 2-3. 8vo. 1913.
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Röntgen Society—Journal, Vol. IX. Nos. 36-37. 8vo. 1913.
Roumanian Academy, Bucharest—Bulletin Scientifique, Vol. II. Nos. 2-3. 8vo. 1913.
Royal College of Surgeons—Calendar, 1913. 8vo.
Royal Colonial Institute—United Empire, Vol. IV. Nos. 7-10. 8vo. 1913.
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Smith, the late B. Leigh, Esq., M.R.I.—Scottish Geographical Magazine, Vol. XXIX. Nos. 8-10. 8vo. 1913.
Smithsonian Institution—Miscellaneous Collections: Vol. LVII. Nos. 11-12; Vol. LIX. No. 19; Vol. LX. No. 23; Vol. LXI. Nos. 3-4; Vol. LXII. No. 1. 8vo. 1913.
Report on U.S. National Museum, 1912. 8vo 1913.
Bureau of American Ethnology, Twenty-eighth Annual Report, 1906-7. 4to. 1912.
Annals of the Astrophysical Observatory, Vol. III. 8vo. 1913.
Società degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. II. Disp. 7-9. 4to. 1913.
Sopote, M., Esq., B.Sc. (the Author)—Mind and Brain and other Studies. 8vo. 1913.
South Africa, Union of—Agricultural Journal for June-Sept. 1913. 8vo.
South African Association for the Advancement of Science—Report of the Tenth Meeting, 1912 (Port Elizabeth). 8vo. 1913.
South Australian School of Mines—Annual Report, 1912. 8vo. 1913.
Statistical Society, Royal—Journal, Vol. LXXVI. No. 8. 8vo. 1913.
Steeves, G. Walter, Esq., M.A. M.D. M.R.I. (the Author)—Some Main Issues. 8vo. 1913.
Tariff Reform League—Monthly Notes on Tariff Reform for Aug.-Oct. 1913. 8vo.
Tohoku Imperial University, Sendai, Japan—Science Reports: First Series, Vol. II. Nos. 1-2; Second Series, Vol. I. No. 2. 4to. 1913.
United Service Institution, Royal—Journal for July-Oct. 1913. 8vo.
United States Bureau of Standards—Bulletin, Vol. VIII. No. 4; Vol. IX. No. 1. 8vo. 1913.
Technologic Papers, Nos. 12-13. 8vo. 1913.

United States Department of Agriculture—Experiment Station Record, Vol. XXVIII. Nos. 7–9; Vol. XXIX. Nos. 1–3. 8vo. 1913.

Farmer's Bulletin, No. 535: Sugar and its Value as Food. 8vo. 1913.

Rivers and Floods of the Sacramento and San Joaquin Watersheds. 8vo. 1913.

United States Department of Commerce—Magnetic Observations at Cheltenham, 1911–12. 4to. 1913.

United States, Department of the Interior—Geological Survey: Bulletin, Nos. 522, 527, 530, 532–535, 537. 8vo. 1913.

Water Supply Papers, Nos. 292, 305, 307, 308, 314, 315, 317, 318. 8vo. 1913.

Professional Papers, Nos. 78–79, 80, 85A. 4to. 1913.

United States Patent Office—Official Gazette, Vol. CXCI. No. 5—Vol. CXCV. No. 3. 8vo. 1913.

Upsala, Royal Society of Sciences—Nova Acta, Ser. IV. Vol. III. No. 7. 4to. 1913.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1913, Heft 6. 4to.

Vienna, Imperial Geological Institute—Jahrbuch, 1912, Heft 4; 1913, Heft 1. 8vo. 1913.

Abhandlungen, Band XVI. Heft 4. 4to. 1913.

Verhandlungen, 1913, Heft 6–12 4to.

Warsaw, Academy of Sciences—Comptes Rendus, 1912, Nos. 8–9. 8vo.

Proceedings, Nos. 8–9. 8vo. 1912.

Washington National Academy of Sciences—History of the First Half Century. 8vo. 1913.

Memoirs, Vol. XI. 4to. 1913.

Webb, W. L., Esq. (the Author)—Biography and Account of the Discoveries of T. J. J. See. 8vo. 1913.

Western Australia, Agent-General—Statistical Abstract for April–July, 1913. 4to.

Geological Survey: Bulletin, No. 44. 8vo. 1912.

Report of Department of Mines, 1912. 4to. 1913.

Western Society of Engineers—Journal, Vol. XVIII. No. 6. 8vo. 1913.

Zoological Society—Proceedings, 1913, Part III. 8vo.

List of Fellows. 8vo. 1913.

Zurich Naturforschenden Gesellschaft—Vierteljahrsschrift, 1913, Heft 1–2. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 1, 1913.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C.
D.C.L. F.R.S., President, in the Chair.

Howard Marryat, Esq.
H. Gordon Selfridge, Esq.

were elected Members of the Royal Institution.

Professor Henri Deslandres, D.Sc., Chevalier de la
Légion d'Honneur, Membre de l'Institut, (Paris).

was elected an Honorary Member of the Royal Institution.

The Chairman reported the decease of Sir William H. Preece, K.C.B., on November 6, 1913, and the following Resolution was passed unanimously, all present standing :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution, and the world of Electrical Science, in the decease of Sir William Henry Preece, K.C.B. LL.D. D.Sc. F.R.S. M.Inst.E.E. M.Inst.C.E., Officer of the Legion of Honour; the author of many original Papers on Electrical subjects, and of the following books: "The Telephone" (1889); "A Manual of Telephony" (1893); and "Telegraphy" (1905).

Sir William Preece became a Member of the Royal Institution in 1875. He occupied Managerial Office, and favoured the Institution by delivering Friday Evening Discourses on "The Application of Electricity to the Protection of Life on Railways" (1876); "The Telephone" (1878); "Multiple Telephony" (1879); "The Telegraphic Achievements of Wheatstone" (1880); "Safety Lamps in Collieries" (1888); and "Signalling through Space without Wires" (1897).

Sir William Preece occupied the Presidential Chair of important Engineering Institutions, including the Civil and Electrical Engineers; was one of the pioneers in the development of Wireless Telegraphy; and formerly Electrician and Engineer-in-Chief to the General Post Office.

The Managers desire to offer, on behalf of the Members of the Roya Institution, the expression of their sincere sympathy with the family in their bereavement.

The following Lecture Arrangements Before Easter 1914 were announced :—

PROFESSOR H. H. TURNER, D.Sc. D.C.L. F.R.S., Savilian Professor of Astronomy, Oxford. Six Lectures (Illustrated) adapted to a Juvenile Auditory, on A VOYAGE IN SPACE: 1. THE STARTING POINT—OUR EARTH; 2. THE START THROUGH THE AIR; 3. JOURNEYING BY TELESCOPE; 4. VISITS TO THE MOON AND PLANETS; 5. OUR SUN; 6. THE STARS. On Dec. 27 (*Saturday*), Dec. 30, 1913; Jan. 1, 3, 6, 8, 1914.

PROFESSOR WILLIAM BATESON, M.A. D.Sc. F.R.S., Fullerian Professor of Physiology, Royal Institution. Six Lectures on ANIMALS AND PLANTS UNDER DOMESTICATION. On *Tuesdays*, Jan. 20, 27, Feb. 3, 10, 17, 24.

PROFESSOR SIR JOHN H. BILES, LL.D. D.Sc. M.Inst.C.E., Professor of Naval Architecture, Glasgow. Three Lectures on MODERN SHIPBUILDING: 1. SMOOTH WATER SAILING; 2. OCEAN TRAVEL; 3. THE WAR NAVY. On *Tuesdays*, March 3, 10, 17.

ARTHUR H. SMITH, Esq., M.A. F.S.A., Keeper of Greek and Roman Antiquities, British Museum. Two Lectures on LANDSCAPE AND NATURAL OBJECTS IN CLASSICAL ART: 1. EARLY GREECE AND ITS PRECURSORS; 2. LATER GREECE AND ROME. On *Tuesdays*, March 24, 31.

WILLIAM McDOUGALL. Esq., M.A. M.B. F.R.S., Fellow of Corpus Christi College, Oxford. Two Lectures on THE MIND OF SAVAGE MAN (Illustrated by the Pagan Tribes of Borneo): 1. HIS INTELLECTUAL LIFE; 2. HIS MORAL AND RELIGIOUS LIFE. On *Thursdays*, Jan. 22, 29.

PROFESSOR SIR THOMAS H. HOLLAND, K.C.I.E. D.Sc. F.R.S., Professor of Geology, Manchester. Two Lectures on TYPES AND CAUSES OF EARTH CRUST FOLDS. On *Thursdays*, Feb. 5, 12.

PROFESSOR I. GOLLANCZ, Litt.D. F.B.A., University Professor of English Literature, King's College, London. Two Lectures on HAMLET IN LEGEND AND DRAMA: 1. THE MYTH; 2. THE PLAY. On *Thursdays*, Feb. 19, 26.

PROFESSOR C. T. JENKIN, M.A. M.Inst.C.E., Professor of Engineering, Oxford. Three Lectures on HEAT AND COLD: MODERN APPLICATIONS; HEAT ENGINES; THEORY OF CALORIC; FREEZING ENGINES, etc. On *Thursdays*, March 5, 12, 19.

CALEB WILLIAMS SALEEBY, Esq., M.D. F.R.S.E. *M.R.I.* Two Lectures on THE PROGRESS OF EUGENICS: 1. THE FIRST DECADE OF MODERN EUGENICS, 1904-1914; 2. EUGENICS TO-DAY: ITS COUNTERFEITS, POWERS AND PROBLEMS. On *Thursdays*, March 26, April 2.

PROFESSOR FREDERICK CORDER, Curator and Professor of Music, Royal Academy of Music. Three Lectures (with Musical Illustrations) on NEGLECTED MUSICAL COMPOSERS: 1. LUDWIG SPOHR; 2. HENRY BISHOP; 3. JOACHIM RAFF. On *Saturdays*, Jan. 24, 31, Feb. 7.

JOHN ALLEN HARKER, Esq., D.Sc. F.R.S. *M.R.I.*, National Physical Laboratory. Two Lectures (with Experimental Illustrations), on THE ELECTRIC EMISSIVITY OF MATTER: 1. THE METALS; 2. OTHER SUBSTANCES. On *Saturdays*, Feb. 14, 21.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D., D.Sc. F.R.S. *M.R.I.*, Professor of Natural Philosophy, Royal Institution. Six Lectures on RECENT DISCOVERIES IN PHYSICAL SCIENCE. On *Saturdays*, Feb. 28, March 7, 14, 21. 28, April 4.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Memoirs of Department of Agriculture: Chemical Series, Vol. III. No. 3. 8vo. 1913.
Agricultural Research Institute, Pusa: Bulletin No. 34 8vo. 1913.
The Imperial Bacteriological Laboratory, Muktesar. By J. D. E. Holmes. 8vo. 1913.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXII. 2° Semestre, Fasc. 8. 8vo. 1913.
Alchemical Society—Journal, Vol. II. Part 6. October, 1913 8vo.
Allegheny Observatory—Bulletin, Vol. III. Nos. 4-6. 4to. 1913
American Academy of Arts and Sciences—Proceedings, Vol. XLVIII. No. 21, Vol. XLIX. Nos. 5-7. 8vo. 1913.
American Geographical Society—Bulletin, Vol. XLV. No. 11. 8vo. 1913.
Astronomer Royal, The—History and Description of the Cape Observatory. By Sir D. Gill 4to. 1913.
Bankers, Institute of—Journal, Vol. XXXIV. Part 9. 8vo. 1913.
Batavia, Royal Magnetical and Meteorological Observatory—Observations, Vol. XXXII. 1910. 4to. 1913.
Observations at Secondary Stations, Vol. I. 4to. 1913.
Regenwaarnemingen in Nederlandsch-Indie, 1911, Deel II. 8vo. 1913.
Belgium, Royal Academy of Sciences—Bulletin, 1913, Nos. 7-8. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XX. Nos. 1-2. 4to. 1913.
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Journal for November, 1913. 8vo.
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Philosophical Transactions, B, Vol. CCIV. No. 309. 4to. 1913.
Celebration of the 250th Anniversary of the Royal Society, 1912. 8vo. 1913.
Royal Society of New South Wales—Journal and Proceedings, Vol. XLVII. Part 1. 8vo. 1913.
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Sanitary Institute, Royal—Journal, Vol. XXXIV. No. 11. 8vo. 1913.
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Smithsonian Institution—Miscellaneous Collections, Vol. LXI. Nos. 1, 16. 8vo. 1913.
Annual Report, 1912. 8vo. 1913.
Societa degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. II. Disp. 10. 1913. 4to.
South Africa, Union of—Agricultural Journal for October, 1913. 8vo.
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United States Department of Commerce—Results of Magnetic Observations near Honolulu, 1911–12. 4to. 1913.
Bulletin of the Bureau of Standards, Vol. IX. Nos. 2–3. 8vo. 1913.
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United States Patent Office—Gazette, Vol. CXCV. Nos. 4–5; Vol. CXCVI No. 1. 8vo. 1913.
Victoria Institute—Journal, Vol. XLV. 8vo. 1913.
Wainwright, J. T., Esq. (the Author)—Investigation into the Second Law of Thermodynamics. 8vo. 1913.
Western Australia, Agent-General—Statistical Register, 1911. 4to. 1913.
Western Society of Engineers—Journal, Vol. XVIII. No. 7. 8vo. 1913.

WEEKLY EVENING MEETING,

Friday, May 23, 1913.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D. F.R.S., President, in the Chair.

PROFESSOR SILVANUS P. THOMPSON, D.Sc. LL.D. F.R.S. *M.R.I.*

The Secret of the Permanent Magnet.

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